BOOK REVIEWS

Gretchen Luepke, Book Review Editor

THE BEHAVIOR OF THE EARTH. *Claude Allègre; translated by Deborah Kurmes Van Dam. 1988. Harvard University Press, Cambridge, Mass. and London. 272 p. Hardcover, $35.00.*

This is a book about concepts and the people who forged them. Allègre touches on most of the great problems of our dynamic earth. He synthesizes, simplifies and challenges major ideas in a glowing and enthusiastic prose that is both educational and entertaining. Although the book is easily readable by a seriously interested layman, it will appeal most to earth scientists who wish to back off from their specialities, think about how science advances, and gain new perspectives for their future efforts. Behavior of the Earth should appeal to historians of science; it could also serve as a core textbook for an undergraduate earth sciences seminar. But this is no lightweight work. It is full of challenging ideas for professional geologists and geophysicists.

The abundant illustrations are designed to illuminate ideas and the basic evidence behind them. Many of the international players in the plate tectonics revolution, beginning with Wegener, are also pictured informally (find your friends!). Finally, high-quality, two-page colored maps of the continental age provinces, ocean-floor ages, and continent and ocean topography are included.

The book begins with a preface that reveal's Allègre's consuming enthusiasm for the working's of the earth: "A region is nothing unless it takes its place in a general framework or unless the mechanism and structures it illustrates are significant for the functioning of the earth as a whole" (p. xi). This enthusiasm is coupled with an intense interest in individual scientists and their interactions as they constructed the modern revolution in our understanding of the earth. The emphasis on major syntheses may offend some; little credit is given for the meticulous gathering of facts and data upon which the grand edifice is constructed, and hence more geophysicists than geologists are numbered among the heroes. But the spirit is kindly, challenging, realistic, and often amusing, even when one belongs to the camp being criticized. Moreover, the author seems keenly aware of the scientific infrastructure. For example, he says, "The study of mountains" requires "the willpower to attack a problem whose complexity seem to defy solution" (p. 12). Field geologists, take comfort!

Chapter 1 is entitled "The Wegenerian Synthesis" and Chapter 2 "Retreat to Specialization." Allègre gives a penetrating analysis of the evidence that led Wegener to the continental drift hypothesis and of the reasons (many non-scientific) for a general rejection of his ideas. Harold Jeffrey's "rigorous" calculations of earth rigidity were influential despite other indications, such as post-glacial uplift, of creeping flow in the earth. The contemporary lack of tools for quantifying and testing Wegener's theory made the "retreat to specialization" almost inevitable until "Seafloor Spreading" (Chapter 3) and "Plate Tectonics" (Chapter 4) supplied new tools. The "violent" debates of the early 1970's for and against plate tectonics are interestingly dramatized (p. 117-118). Oversimplifications of the Wilson cycle are rejected. Intraplate deformation (as in China) and volcanism (as in Hawaii) show that "Plate boundaries thus have no monopoly on discharges of energy!"

Chapter 5 covers "The Birth of Marine Geology" and Chapter 6, "Plate Boundaries." Chapter 7 deals with "Mountain Building". This chapter begins by tracing early ideas of the underlying processes: geosynclines, contracting earth ("proved" by Harold Jeffrey's calculations of rigidity), mobility in the Alps, the famous convection model of David Griggs, and the pioneering synthesis of Arthur Holmes. Then came the integrating influence of mobility theory as developed by Dietz, Hess, Dewey and Bird, and many others, applied to Asia, the Andean cordillera, New Guinea (ophiolites), and the Himalaya.

Chapter 8 brings us to "The Continental Crust," which is not slighted among the general problems. The origin of continental crust is treated in simplified fashion, with wonderful historical insights about the early players in the puzzle about the significance of radiogenic isotopes. "Mosaic tectonics" (terrane assembly) is also highlighted, but Allègre cautions, "We should not hope to reconstruct ancient puzzles in complete detail!" (p. 210); "The pages in which earth's history have been recorded have been cut, stuck together, torn, and pasted together every which ways. Reading them is bound to be difficult" (p. 211). Intracontinental basins like the Michigan Basin again emphasize the deformability of "rigid" continental plates.

Chapter 9, "The Dynamics of Plate Movement," takes us to convection, partial melting in the mantle, and the key role of hotspot plumes like those beneath Hawaii, Iceland, and Yellowstone. Allègre ends with a short epilogue that explores such philosophical topics as the interplay between discovering and convincing. "Persuasion is the ultimate object of communication with the multitudinous, lively, and abrasive body that is called the scientific community"; with continental drift, "...it seems that it was more difficult to convince than to discover!" (p. 243). The role of national attitudes and the occasional overpowering influence of a few scientists is interesting. A glossary of technical terms and an index follow the epilogue.

The book is not without a few minor flaws. For example, the caption on Figure 9 states that the core transmits waves faster than the mantle. On p. 11, folded mountain belts are said to be perpendicular rather than parallel to trenches; on p. 33, continental crust is said to be mostly granite; and on p. 139, the Moho is identified with the transition from basalt to gabbro rather than from gabbro to peridotite. These small slips detract little from a volume that is otherwise meticulously written and illustrated, and beautifully translated from the French.

Allègre shares with the reader a profound and sensitive understanding of how science progresses. He has captured the mystery and excitement of scientific discovery while illuminating the meticulous, often stumbling, efforts along the path to discovery. Experienced earth scientists will find a new perspective in this book, and they'll be reminded of the surprise--and exhilaration!--of the unexpected in science.

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THE AGE OF LAMARCK: EVOLUTIONARY THEORIES IN FRANCE 1790-1830. *Pietro Corsi; translated by Jonathan Mandelbaum. Revised & updated, 1988. University of California Press, Berkeley. 360 p. Hardcover, $45.00.*

Pietro Corsi's aim is to dispel two myths: one about Lamarck the isolated thinker and loner, and the second about about "the nearly total silence with which naturalists of the period supposedly greeted his ideas." For this purpose, Corsi has carefully consulted scientific journals of that time, launched and edited by various naturalists (today mostly unknown with the exception of Jean-Claude de Lamétherie, editor of Journal de physique), namely, C,-N.-S. Sonnini de Manoncourt; P. Denys de Montfort; P.-A. Latreille; J.-J. Virey; J.-B.-G.-M. Bory de Saint-Vincent; J.-L.-M. Poiret; André-Étienne d'Audebard de Ferussac; and the better known Henry Ducrotoy de Blaineville, J.-B.-J. d'Omalius d'Halloy, Marcel de Serres, and E. Geoffrey de Saint Hilaire.

Corsi convinces us that with the exception of Cuvier, all of the above naturalists were in favor of "some aspects" of Lamarck's scientific views, although they sometimes expressed ideas unacceptable to Lamarck himself, such as the theory of embryonic capitulation by Geoffrey. Furthermore, Corsi mentions that George-Louis Duvernoy (a student of Cuvier) commented actually before Lamarck on "the classic principle that organs were fortified by use and weakened by disuse"; that Francois-Marie Daudin wrote that "the form and structure of the parts of [a bird's] body are closely related to its habit and disposition"; and that the influence of the environment was recognized by many naturalists such as B.-G.-A. Lacepède, John Latham, M.-J. Brisson, and even Cuvier. According to Lacepède, species might undergo metamorphoses because of climatic conditions. Corsi has made it clear that Lamarck took part "in a far-reaching momentous debate" among his contemporaries and was therefore not an isolated worker.

In regard to the second myth that Lamarck was surrounded by total silence, Corsi admits that the "radical character of Lamarck's thesis on the origin of minerals" (originated from decaying plants and animals) made any dialogue impossible, both with the followers of the old school (Lamétherie, Sonnini, Faujas de Saint-Fond, Patrin, Louis Bertrand) and the new school (Cuvier, Haüy, Dolomieu) . Nevertheless, naturalists did react, often because they disliked Cuvier's dogmatic attitude. Faujas praised Lamarck's Système des animaux sans vertèbrates but said nothing about geology or chemistry. Sonnini, Denys de Montfort, Latreille, and Virey approved of some of Lamarck's ideas, whereas Bory de Saint-Vincent became a leading Lamarckian transformist by 1820. In fact, says Corsi, Cuvier was appalled about the interest aroused by Lamarck's theory of transformation and scared that Lamarck would form a school of his own. During the 1820's, total and partial support for Lamarck's ideas increased.

I believe that Corsi has not left a stone unturned to convince his readers that the two above myths are indeed fallacies. He adds that Lamarck's followers rarely understood him completely, just as modern historians have problems with Lamarck's text. This might even apply to Corsi himself. Besides his debunking of two myths, he spends much time explaining the real thought of Lamarck, particularly in biology and geology.

These are long and thorough analyses from which many historians may gain, and yet geology is misinterpreted. On several occasions, Corsi, compares Lamarck's work Recherches sur l'organisation des corps vivans... to Hydrogéologie, both published in 1802, saying that geological and biological dynamics were not incompatible for Lamarck. Indeed, Lamarck explains how "rain water will first hollow out the plain into several depressions. Soon after, the water concentrated in these hollows will open up a passage for itself toward the lowest areas in its neighborhood....Gradually, the water in the various depressions scattered over the plain will flow toward the channels to the sea and through them will in time develop the brooks, torrents, streams and rivers which furrow the earth in all directions." (Lamarck, p. 13). These words, it is true, resemble Lamarck's belief that organic dynamics produce organs over a long period of time: "In many animals (especially the least developed) the vital organs exist only through an external influence..." (Corsi, p. 138). Lamarck explained the gradual development of the food canal as follows: "imperceptibility the depth of this small pit will gradually increase though the habit of filling itself and the ever more frequent use of its pores. It will shortly acquire the shape of a sac or tubelet-like depression with porous walls, [This cavity] may be blind or have only a single exit. Here is the first food canal, the simplest digestive organ." (Corsi, p. 142). Corsi neglects, however, the full geological thought of Lamarck which was studied in detail by Albert V. Carozzi in his translation of Lamarck (Hydrogéeology 1964).

It is true that Lamarck was at first proposing an initial plain of land from which future mountains were carved by fresh water and that the westward movement of the sea basins produced by the moon (tidal ebb and flow) was counteracting an accumulation of the land-derived material which would have filled the ocean basins with time. Later on, Lamarck talked about the raising of plains by the accumulation of living beings. Corsi concluded that to "dispel any doubts on the coherence of this theory, Lamarck returned once again, at the end of his work, to the progressive elevation of plains: '...plains keep rising as long as they are covered with living bodies...Every stretch of land covered with living beings, rising by a foot a century, will thus have time to reach a tremendous height above the ground before being destroyed or submerged by sea waters.'"(Corsi, p. 109). This refers to p. 174-178 of Lamarck. Had Corsi continued his reading carefully, he would have found that Lamarck had come to the conclusion that other factors are "responsible for the highest nonvolcanic mountains," namely "polar flattening" and "equatorial bulges" (Lamarck, p. 178-183).

Without mentioning any names, Lamarck was in fact referring to Maupertius and his coworker's measurements of the flattening of the poles (Élemens de Géographie, 1742). Before and after Maupertius, many naturalists believed that the center of gravity of the earth changes constantly and that during the displacement of the earth's axis, ocean basins cover successively various points on earth.

I doubt whether such a Hydrogéologie can still be linked to Lamarck's theory of transformism. The constancy of the water's movement on the terrestrial surface created indeed only minor changes, but the shifting of polar points, polar flattenings, and equatorial bulges were responsible for the highest and oldest mountains on earth (Lamarck, p. 145-148).

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THEORY OF EARTH SCIENCE. *Wolf von Engelhardt and Jorg Zimmermann; translated by L. Fischer. 1988. Cambridge University Press, New York and London. 381 p. Hardcover, $75.00.*

As a practical matter for interdisciplinary committee proposals, and technical projects, it has increasingly become the duty of the geoscience specialist to prepare or at least be cognizant of the whole of knowledge and practice of his discipline. This includes being able to show the significance of nomenclature, methods, theories, and over-arching guiding principles of given geospecialities to those of the earth sciences as a whole, as distinct from those in other natural sciences. Unfortunately, the resources for these requirements are not always at hand. More than one practitioner has sought (largely in vain) a source of synthesis and perspective for the currently evolving state of the profession.

Most geoscientists have been discouraged from looking to philosophy or history of science for data and methods for present use. A widespread thesis in generalized philosophy-of-the-sciences has been that all true theories in well-founded scientific (sub)disciplines should "in the long run" be reducible to physics. Thus the appearance of a more concrete and material (vs formal-logical) theory of a specific natural science different from physics is a very rare and highly relevant undertaking, to be carefully noted both by geoscience and its philosophers/historians. In traditional "theories of 'Science'" from neo-Kantian, logical positivist, and most contemporary schools of meta-science, with few exceptions, the earth sciences unfortunately have been given rather short shrift in most philosophy of science discussions as neither "hard" nor "soft" science--somewhere below physics and above geography in a hierarchical ranking of the disciplines.

Partly in response to the above motives, Wolf von Engelhardt (Professor of mineralogy at Universität Tübingen) and Jorg Zimmermann (professor of philosophy at Universität Hamburg) have attempted a difficult multidisciplinary task in philosophical geoscience/applied philosophy in Theory of Earth Science, an excellent English translation of the author's previous German original (Theorie der GeoWissenshaft, 1982; Shoeningh Verlag, Paderborn, West Germany). This book is a noble effort and a not inconsiderable success. The authors outline their "meta-theory" of theory development and change in modern earth sciences in eight chapters. They discuss the types and functions of geoscience technical and textural communications; the hierarchies and categories of basic geoscience data acquisition and analysis; the role of induction, hypotheses, and proliferational vs revolutionary change, as well as "normative principles" such as (neo)catastrophism, uniformitarianism, entropy, and systems in the earth sciences.

The main text comprises a large admixture of philosophical preliminaries, which provide a framework for attempting to characterize what is unique in the language, subject matter, theories, guiding principles, methods, and technologies of the earth sciences. The authors' viewpoint (p. 253ff) is clearly that most things borrowed or shared by the geoscientists from (e.g.) physics, chemistry, botany, and meteorology are not dominant in theory or practice, and have been much adapted and reinterpreted to fit earth science needs and outlooks. They seek to stress what are often called "integrative functions" of these meta-scientific characteristics in a scientific discipline. Examples are identified as at work in a number of geoscience subdisciplines, and at a number of of meta-scientific "levels," which the authors define as empirical, systematic, theoretizing, and regulative/metatheoretical. Several geological examples whose four levels are repeatedly examined throughout the text include mineralogic, structural, and geochemical analyses of a complex meteorite impact feature (the Nordlinger Ries, in northwest Germany). The theory of plate tectonics is most significantly underscored as perhaps the most successful and widespread interdisciplinary co-opting of the subdisciplines of the geology to date (Chapters 6 & 7).

In Chapters 1 and 2, through analysis of representative geoscience texts and journals, the authors attempt to reconstruct the basic discourse and research practices common to most all geoscientific disciplines. In Chapter 3, a language-philosophy/semiotic discussion of the inferential and classificational earth sciences describes the four underlying stages or levels, in a hierarchical, reticulational, or hermeneutical model of (geo)scientific functioning. These four levels will also be recognized by afficionados as similar to those of Lakatos and not dissimilar to Hübner's five classes of scientific precepts (instrumental, functional, axiomatic, judicative, and normative). The remaining four chapters are fuller explications of these levels, centering around characterizing idiosyncratic if not unique features of geoscience corresponding to each level. Of over 35 such geoscience-characteristic features, this reviewer tentatively identified several representatives, according to level:

Level 1:
1) Language closely anchored in prescientific vernacular (p. 35-43).
2) dominance of mixed morphological, genetic, & functional categories (p. 43, 47-50, 83-96, 113-117,120, 129)
3) high value of factual reporting in absence of theory or problem addressed by (new) facts (p. 10, 224)

Level 2:
1) Innately-open interpretative status of geodata bases & maps (p. 132)
2) constant concern with description /explanation of singular historical & local phenomena (p. 78, 176, 205, 212)
3) strongly contingent nature of facts based on remote sensing of signs and indices of largely inaccessible subterra (p. 51-53)

Level 3:
1) greater number, role and complexity, theory-invariant facts (p. 129)
2) greater number, role and complexity, geographically-local theory (p.35)
3) frequency of temporally-specific Level 1 & 2 facts (p. 174, 202)

Level 4:
1) lack of compelling methods of determine explanatory content of geoscientific theories (p. 14, 119, 152-158, 200ff).
2) lack of comprehensive research programs linking terrestrial to planetary geoscience (p. 5, 179)
3) increasing role of plate-tectonics as de facto unifier of geoscience subdisciplines (p. 25, 253ff)

The success of this exposition is its systematic organization and use of telling examples from primarily mineralogy and structural geology. It also succeeds along the way in summarizing a number of highly epistemological and other philosophical concepts that might otherwise be unfamiliar to many geoscientists. The signal attempt of an interdisciplinary work like Theory of Earth Science, to mediate between science and the philosophy, history, and sociology making up a theory of earth science, is important and properly motivated in an era when over-modest "hand-washing" by some meta-scientists of any responsibility for accuracy and relevance to science itself is all too common. In a concrete first- and second-order metascientificae specialis such as this, it is tacitly assumed that in contrast with the "model science" of physics (where the story and methodology of experiments and discoveries are generally preserved external to the science without their original scientific utility and impact), a key characteristic of the earth sciences qua historical is their "internally accretional" rather than merely "residual" nature. In this sense, the goal of a book like this is as much to explicate people's reasoning in geologic science as to deconstruct ideas of philosophers, historians, psychologists, and sociologists of (geo)science as to what people's reasonings in geology have or should have been like.

The book's difficulties lie in its occasional abbreviated accounts of (e.g.) the role of analogies of geoscience (p. 80), of specialized technical terms of philosophy such as "deictic discernability" (p. 41), and its largely nonhistorical approach to only some of the subdisciplines of contemporary earth science. Insofar as this reviewer (at least somewhat versed in the arcana of geological and meta-science) required two readings before writing this review, it will honestly take some serious attention and rereading on the part of the interested scientist to fully understand the book's arguments and conclusions. Perhaps the metascientific concept this book misses more than any other is that of scientific "disciplinarity". Numerous authors have variously written on meanings attached to the notions of discipline, science, field, speciality, and profession, but with less than four exceptions have not considered the disciplinary structure of the geoscience cluster (p. 330). The goal of Theory of Earth Science's Preface, of clarify ing the (meta)theoretic as well as pragmatic trends of intra-, inter-, and multidisciplinarity in the earth sciences since Plate Tectonics, presupposes a clarification of what has historically and presently comprises a geoscientific (sub)discipline. With a more detailed explication of the "reticulational" or "hermeneutic" interactions between between the four postulated levels, the above level-based criteria offer the raw material for such an endeavor, which one hopes will be advanced in further discussions.

The above weaknesses, however, are not fatal. Theory of Earth Science is not itself intended to be a final and comprehensive theory, but a prologomena identifying some tools and problems necessary to consider the task, and should be favorably judged as such. The wide range of philosophical viewpoints considered from German and French as well as Anglo-American philosophy of science is an example of what an eclectic and international approach (vs insular imperialism) ought to be. The extensive footnotes will surely help the serious as well as critical reader. In terms of vision and synthesis, I rate this book high, and only somewhat less high on balance and detail--although to include enough to satisfy everybody would have easily doubled the book's length! Of course there is an inevitable tradeoff between technical exclusivity of audience vs general appeal in any undertaking such as this. Geoscientists should not give up too soon in what one hopes will be their serious consideration of von Engelhardt's and Zimmermann's efforts. Anglo-American theorists of sciences should not be too severe on the occasionally summary reformulations of Kuhn's, Lakatos', and other philosophical models of scientific rationality. This was never meant to be a work of history or sociology of earth science, although throughout the text are many novel suggestions for work of this kind.

Those seeking a finer understanding of the conceptual structuring of their science, and those seeking resources to help them think anew about organizational and programmatic problems, will find Theory of Earth Science a useful resource. Philosophers, historians, and sociologists of the natural sciences are, and should continue where possible, the science-metascience partnership so well exhibited by the present effort.

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CLARENCE KING: A BIOGRAPHY. *Thurman Wilkins, with Caroline Lawson Hinkley. 1988, revised & enlarged edition. University of New Mexico Press, Albuquerque. 524 p. Hardcover, $29.95; softcover, $16.95.*

Giftedness in youth seldom portends once-in-a-generation accomplishment--but such it was with Clarence King, whose heroically-proportioned life has stirred his biographer to a second edition three decades after the first.

Expanded by 35,000 words, drawn from a host of new sources, including 200 letters by King himself, Wilkins and Hinkley help us to understand why Henry Adams--scion of presidents, Harvard professor, and one of the most influential men of his time--praised King as "the most many-sided genius of his day."

In a captivating, poignantly-informed account, the detailed figure of King--who was instrumental in moving Congress to establish the U.S. Geological Survey in 1879 and served as its first director--is painted against the turbulent panorama of politics, mining fraud, stock-market manipulations, and the explosive growth of earth science in western exploration. Even on excursions into art criticism, literature, and other channels, King was a prodigy to all who knew him.

King's diverse life has drawn both hagiologic and deprecating assessments. Wilkins' treatment--the most finely-detailed--is mature, understated, and understanding. The unobtrusive elegance and simplicity of style make this rich, scholarly biography an exceptional servant of history. Wilkins understands the geological dimensions of the story and critically weighs the social and political conditioners of larger events. He seats us enthralled beside King at the table of power around the country--even the White House--to eavesdrop on scientific, political, and economic schemes that shaped the future of careers, fortunes, geologic enterprise, and our national destiny.

The history expands the human aspects of much in Mary Rabbitt's authoritative, multivolume Minerals, Lands and Geology for the Common Defence and General Welfare, and provides some alternatives to the interpretations of Wallace Stegner in Beyond the Hundredth Meridian--Wilkins understands men and history.

King seems to be from an earlier age--he drew the admiration of statesmen, artists, scientists, industrial magnates, and the men who served him in the Survey. When elected, he was the youngest member of the National Academy of Sciences. Only 25, and five years out of Yale in 1867, King led the first modern geological survey. That ten-man party produced work during their first two years that moved the Army's Chief of Engineers to raise King's salary to $350 a month--the highest of any civilian employed by the Army Engineers, equivalent to field pay for a full colonel.

The first report of the King Survey was published on the mining industry--650 pages with an atlas of maps. King's contribution of three chapters was on the geological mode of occurrence of the mining districts, the geology of the Comstock lode, and the Green River coal field. The American Journal of Science called it the "most valuable contribution yet made to the literature of the mining industry in the United States." J.D. Whitney was awed and referred to it as " a superb piece of work, and far in advance of anything previously done in this country in the same line, and we know of nothing published in Europe superior to it."

King's own view of his contributions to geologic science (which seems to be favored by Wilkins) can be read in a letter to General A.A. Humphreys, Chief of Engineers, in May 1874, reporting on the progress of the fortieth parallel study: "The day has passed in geological science when it is either decent or tolerable to rush into print with undigested field observation, ignoring the methods and appliances in use among advanced investigators. It is my intention to give this work a finish which will place it on an equal footing with the best European productions and those few which have redeemed the wavering reputations of our American investigators."

King sent Samuel Franklin Emmons, his close friend and eminent economic geologist to Europe to report on the methods of the British and continental surveys, collect a proper reference library, and acquire the finest German microscopes available. The power of King's perspective could be found in all aspects of federal geology. A few years later King wrote, "What I did in and from Washington has the effect of ending a period of chaos in national geology, of founding a new and higher order of science in America."

King also published a series of sketches, Mountaineering in the Sierra Nevada, which a critic thought "probably the most exciting book ever written about mountain-climbing." The editor of the Atlantic Monthly thought it a pity that such literary talent was being "wasted" on science. Wilkins believes that King's artistry placed him with Bret Harte and Joaquin Miller as "founder of a California school of literature."

King's detective work in exposing the Diamond Swindle of 1872, which involved the possible public loss of hundreds of millions of dollars by today's standards, marked him as a giant of perspicuity and integrity. Systematic Geology, the final volume of the King Survey reports, which he had planned and sought the resources to undertake at an age of 25, demonstrated a precocity perhaps never seen since in American earth science.

His coterie, which included Henry and Clover Adams and the John Hays (Hay was a poet, novelist, historian, and Secretary of State under McKinley and Theodore Roosevelt) termed itself the "Five of Hearts" and "made the most fascinating conversation that any American salon ever heard." King was a spellbinder in clubs from the Pacific Union in San Francisco to the Century in New York; some though him the best raconteur and art critic of his day. William Cray Brownell, his fellow Centurian, noted that "inattention was impossible in his presence."

From 1867 to 1879--the year in which the U.S. Geological Survey was established--several federal geological surveys under personal leadership had been in existence. The first, the Geological Exploration of the Fortieth Parallel, created by Congress in 1867, was administered by the Army Engineers but directed by Clarence King. Congress simultaneously established a survey of Nebraska, run by Ferdinand V. Hayden. That survey was originally under the Land Office, but it grew into the Geological and Geographical Survey of the Territories under the Department of the Interior. A third survey evolved out of the Colorado River exploration in 1869 led by John Wesley Powell; this was first under the Smithsonian Institution and then the Department of the Interior. In 1872 the Army extended its mapping activities of 1869 and 1871 by the creation of the Geographical Surveys West of the 100th Meridian under Lieutenant George Wheeler.

As the country expanded, federal and state surveys of geography, coastal bathymetry, and mineral and agricultural resources burgeoned--some states had formed their own geological surveys by the 1820's. It became increasingly apparent that a consolidation of diverse functions and agencies was required --especially since the financial crisis of 1873 had ended the prosperity and optimism that had helped birth those uncoordinated surveys. Although there had been open conflicts of interest among some of the federal surveys and rivalries between military and civilian leadership, it may finally have been austerity that moved Congress to ask the National Academy of Sciences to formulate an economically effective plan that would represent a logical next step in the evolution of the federal government's role in science. The Academy recommended, along with a number of other federal agencies, that the U.S. Geological Survey be established within the Interior Department to study the economic resources and geological structure of the public domain. A congressional bill to that effect was introduced on February 10, 1879.

At this point Wilkins fires his carbide lamp to enter a remarkable labyrinth of internal history: King versus Hayden for the first directorship of the U.S. Geological Survey. Hayden's survey had seniority and he had gone out of this way to gain favorable publicity for it; Congress, scientific societies, and the media were on his side. But King's genius, productivity, and wide circle of friends in academe, the government, and the National Academy of Sciences served him well. John Wesley Powell praised King as the "pioneer and founder of a whole system of survey work which was novel and original...King...has an orderly, sagacious, logical mind which places him among the truly great men of science..." Recommendations arrived from Yale, Columbia, Johns Hopkins, New York University, the American Museum of Natural History, and other institutions. Every member of the National Academy committee that recommended the founding of the U.S. Geological Survey, except James Dwight Dana, favored King as Director. He was appointed on March 11, 1879 at the age of 37. Hayden's resentment ran deep, as was shown in an article in 1981 by Clifford Nelson *et al.* Hayden provided Dana and Archibald Geikie with pejorative accounts of Survey operations--They fueled editorials designed to block congressional approval of King's request to extend the Survey's work east of the Mississippi.

But by the end of the first year, King's efforts as Director moved Clarence Dutton to write to Geikie that "Mr. King is more than justifying all the high expectations which attended his appointment & his skill & ability to organize & administer have proven to be of the highest order. He succeeds in everything--& he is enthusiastically loved by everybody. He has drawn into the Survey the best geological talent of the country."

King's sometimes lavish lifestyle and personal indulgences opened him to scrutiny from time to time. His luxurious geologic camp seemed sybaritic to some. He also invited criticism by spending questionable time during his directorship heavily engaged as a partner in forming private mining ventures in Mexico--his backers represented the riches and power of the country. Some viewed him as flouting the strictures of the Organic Act--although technically a Mexican venture, the deal smelled of conflict of interest..

Wilkins understands life as a delicately balanced system of compromises, even for the anointed. His actuarial account of King jibes with that of Wallace Stegner's, but Stegner, unlike Wilkins, shows a repeated lack of sympathy for King in his biography of John Wesley Powell (the second director of the U.S. Geological Survey): "Clarence King, unlike [Henry] Adams was not hopeless and cynical about the country, he was a man of ebullient optimism. But his hope was a hope of private wealth and personal indulgence, sadly in key with self-interest that drove the politicians and the tycoons. And while Adams was recording the bazaar [in his acclaimed books] along Pennsylvania Ave., and Clarence King was hopping on his mule to track down a Mexican gold mine, John Wesley Powell was sitting down in a shabby hand-me-down office to organize the sciences of the earth and the science of man."

King's brief two-year tenure as director, which ended with his resignation against the wishes of almost all including Presidents Hayes and Garfield, has been variously judged, but Wilkins' view is congruent with that of Mary Rabbitt, whom he quotes: "...the importance of the Geological Survey achieved in its first 25 years, in fact its longevity, should be attributed not to the broad view taken by John Wesley Powell, but to the foresight of Clarence King in organizing the Survey's research to aid in the industrial progress of the country while seeking ultimately the advancement of science and to the perspicacity, administrative skill, and seemingly limitless energy of Charles D. Walcott [the Survey's third director] who held that the Survey's field was geology and not all science, who directed its research toward the aid of not just the mineral industry, as envisioned by King, but of all industries and practical undertakings that would benefit from a knowledge of the Earth and its resources, and who insisted that basic and applied science cannot be separated."

Wilkins believes that it was plainly King's quest of a personal fortune in mining ventures in Mexico that drove him to leave the directorship: "a glittering fortune passed for supreme achievement in America of the 1880's." However, "The business way of life ended in disaster for [King]--a disaster that posed a baffling moral to Henry Adams, who had wondered twenty years before if anyone in their generation was 'as likely [as Clarence King] to leave so deep a trail.' Adams found the contrast between Hay's achievement and King's misfortunes "quite superstitious....King had more suite,...more chances of luck, more foresight, and vastly more initiative and energy. King appears to have failed for lack of backup funds to handle times of emergency..." Hay later added, "With talents immeasurably beyond any of his contemporaries, [King had] everything in his favor but blind luck."

Stegner's view of King in his closing years is harsher: "Clarence King's failure was impressive. His fortune, once close to a million, had been dissipated in years of indulgence abroad and annihilated in the Panic of 1893. His art collection was mortgaged to his friend John Hay, who accepted it as security for his loans not so much because he wanted any security as because of a wish not to hurt King's pride...Clarence King failed for lack of character, persistence, devotion, wholeness. For that important job he seems to Adams cut out to do, Powell was actually much better equipped....he would do more than Clarence King and do it better."

King had been raised in a home that afforded exceptional opportunities for intellectual and spiritual development, and he could hardly have been better schooled. In high school, he was greatly influenced by Mary A. Dodge, who actively published essays on the abolition question. His mother had grown into an ardent abolitionist and his grandmother Sophie Little, whom he saw on visits to Newport, worked in the underground railroad and cast her feelings against the fugitive slave law into a short novel Thrice Through the Furnace. Wilkins dissects other early influences to show the development of King's sensitivities and regard of women. How it came to be that at his death in 1901, he left a clandestine black wife--who had been the only recipient of his deepest love--and five unacknowledged children, is a moving epilogue in a fascinating story.

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NELSON, CLIFFORD, RABBITT, Mary, & FRYXELL, F.M., 1981, Ferdinand Vandeveer Hayden--the U. S. Geological Survey years: *Proc. Amer. Philosoph. Soc.*, v. 125, no. 3, p. 238-243

PIONEERS OF MALTESE GEOLOGY. *George Zammit Maempel. 1989. 302 p. Hardcover, $35.00; softcover, $28.00. (For sale by author, 53 Main St., Birkirkara, Malta).*

Malta's strategic military and political positions in the Mediterranean have long been acknowledged. It is less well known, however, that Malta and the associated islands of Gozo, Comino, and Filfla also occupy a critical geological position between North Africa and southern Europe. We are only now beginning to recognize the extent to which the geology of this region is critical in resolving problems of Tertiary micro-block paleotectonics and paleogeographic evolution in the central Mediterranean. In fact, the geology of the islands is quite straight-forward, and comprises fairly-well defined sedimentary units of late Oligocene to Quaternary age that are offset by faults. Knowledge of the Maltese islands, the submerged bank on which they are positioned, and Sicily to the north provides essential clues for interpreting conditions prior to and including the Messinian, the time of the so-called Mediterranean "salinity crisis." The islands also shed information on the more recent Quaternary, a time of much lowered sea level stand, when this region was part of a larger subaerially exposed southern Europe province, and thus more readily accessible to faunal and floral exchanges with circum-Mediterranean land masses. It is useful to be aware of these regionally important ramifications of Maltese geology as one reads George Zammit Maempel's book, Pioneers of Maltese Geology.

Maempel provides a focused view of those four persons he considers to have been the most instrumental in promoting the study of Maltese geological sciences during the mid- to late Nineteenth Century. As was typical at that time, none were geologists by training. Each was a civil servant and spent only a part of his career in Malta before returning from the colony to Britain. The time span considered, from 1842 to 1894, was that of the naturalist era par excellence. T.A.B. Spratt was a British naval officer employed with the Surveying Service of the Royal Navy in the Mediterranean, and was stationed in Malta from 1842 to 1857. Sir William Reid was Governor of Malta and its dependencies from 1851 to 1858. A.L. Adams was stationed in Malta as a military surgeon from 1860 to 1866. J.H. Cooke was a teacher of English in Malta from 1887 to 1894. These gentlemen were influential, to varying degrees, in that they compiled or modified geological maps, collected and described paleontological specimens, helped organize geological collections in museums on Malta and in England, fostered interest in the natural sciences of the islands, and facilitated the study of natural history of the islands for others. Designation of Spratt as "father of Maltese geology" (although he was not the first) is substantiated.

The author has compiled a fairly comprehensive essay-review for each of these active and imaginative individuals, placing their geological contributions in context of their personal and professional lives and of the times in which they lived. The book does not want for lack of biographical information, chronological lists of publications, ample background information, and footnotes annotating historical events, geology, and other details. The Crimean War by Britain, Turkey, and France against Russia (1853-1856) was a pertinent, if indirect, influence on geological research of the islands: at this time Malta would become strategically and commercially important. As one would expect, the discoveries of precious paleontological remains, including the famous pigmy elephants of Malta found in Quaternary-filled fault zones and caves, are discussed. Attention is called to the tragic loss of so many original Maltese collections, perhaps due even more to poor curation than to the destructive bombardments in World War II. Historians will also be interested in some of the asides, such as T.A.B. Spratt's politically delicate warning in 1859 that siltation by wave currents would seriously affect the operation of a future Suez Canal to be constructed in Egypt. This prediction has proved to be correct (and costly) since the opening of the Canal ten years later, in 1869.

One would agree with Maempel's premise "that an adequate knowledge of the history and development of a subject, as well as information on the life and works of the people who contributed to such a development, are absolutely essential for the proper understanding of the subject." The book does accomplish this goal. It is of note in this respect that the author has followed in the footsteps of those earlier geological patrons he describes--he was trained in pharmacy and medicine and subsequently gave up his active medical practice to devote his efforts toward a museum curatorship and paleontological study of Malta.

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HISTORY OF GEOLOGY OF WESTERNMOST NEW YORK STATE *(Niagara, Erie, Chautauqua and Cattarauus Counties) (1604-1899). Irving H. Tesmer. 1989. Buffalo Museum of Science, Buffalo, New York. 214 p. Softcover. $19.95.*

Tesmer has compiled a thorough and impressive bibliography of the geological investigations of western New York through the end of the 19th century. The eight chapters--Geomorphology, Lakes and Rivers, Glacial and Surficial Geology, Stratigraphy, Paleontology, Mineralogy and Economic Geology, Structural Geology, and Historical Geology--terminate with copious references. The work is further strengthened by an all-important author index at the end of the book. The volume was well researched and provides a good foundation and point of departure for further detailed investigations. As such, this work would make a good addition to the libraries of most historians, especially those interested in New York and Great Lakes geology.

Although a good chronicle, compendium, or resource guide, it is disappointing because there is no interpretive scheme, and little or no commentary or evaluation. Paragraphs frequently succeed one another without a connecting bridge. For example, a paragraph on the rate of Niagara Falls' migration is followed by one dealing with a marine invasion into the Lake Ontario Basin, succeeded by another that returns to the topic of falls recession.

The reader gets the impression that certain segments of each chapter could just as well be in table form, with three columns entitled, from left to right, year, author, contribution. Another approach could have been to annotate excerpts of the original authors' paragraphs or sentences. However, a portion of this oversight may stem from the work itself. Attempting to treat the historical development of so many diverse geological disciplines with an interpretive and evaluative focus would tax the expertise of the most arduous and skillful of workers. One may be intellectually and historically adept at paleontology and stratigraphy but less so in Pleistocene geology.

I, being predisposed to relate the forest to the trees, thought the work suffered somewhat because the text does not couch events within a "big picture" framework, a clustering of contributions about a central theme. For example, the early period of random and often haphazard notices of early observers beginning with Samuel de Champlain in 1604, was eventually replaced by the "Eatonian Period"--the most organized and methodical (although stratigraphically erroneous) investigations of Amos Eaton in the 1820's and early 1830's, which was largely stimulated by the construction of the Erie Canal (1817-1825). Eaton's seminal work eventually resulted in the 1836 Natural History Survey of the state, ushering in the era of James Hall and the explosion of stratigraphic and paleontologic endeavors that were to mark the remainder of the century. Although many investigators were involved, the contributions of Amadeus Grabau probably culminate, but certainly terminate, this period. A final episode might be the Agassiz or Newberry Period, when the Quaternary and drift deposits came to be recognized as glacial in origin, the works of Grove, Karl, Gilbert and Joseph W.W. Spencer being especially significant. However, this treatment, outlined above, may reveal more about me than it does to Tesmer's approach to the history of western New York geology.

Tesmer could have ameliorated most of my concerns had he included an introductory chapter or preface stating the method of investigation, along with the intent, goals, objectives, and basic thrust of the work.

Lastly, the reader is left to ponder: Who published the book? No publisher is stated anywhere in the volume. I began to dimly comprehend that since the Buffalo Museum of Science is the only outlet for the volume, perhaps the museum is the publisher as well.

In summation, Tesmer's compendium deserves merit for its scope of coverage and thorough bibliography. However, those readers searching for a narrative, thematic, and interpretive history of western New York geology may be somewhat disappointed.

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THE HISTORY OF THE YORKSHIRE MUSEUM AND ITS GEOLOGICAL COLLECTIONS. *Barbara J. Pyrah. 1988. The Ebor Press, York, England. 165 p. Softcover, £4.50, plus £1.50 postage surface mail (order from Yorkshire Museum at Museum Gardens, York, YOl 2DR England; enclose sterling cheque for £6.00).*

Founded in 1822, The Yorkshire Museum built one of the biggest geological collections in the provinces of England. The fortunes of the museum's various collections waxed and waned, resulting, at times, in crowded galleries and storage areas with consequent friction between individual curators. In addition to its regular curators, honorary curators (Audubon and Strickland, among others) were associated with the museum. These honorary curators donated not only their time but also money and specimens, in support of the museum's growing collections.

The acquisition of most of the museum's material took place between 1826 and 1907, and was the work of just four people: John Phillips (1800-1874), who was the first Keeper; Edward Charlesworth (1813-1893); William Reed (1810-1892); and John Francis Walker (1839-1907). During the nineteenth century the Museum amassed over 120,000 specimens of geologic material. By the twentieth century, however, the geological collections had become a heritage from the past rather than an ongoing endeavor. In fact, by the turn of the century, the problems of cleaning, conserving, and storing, combined with a shortage of funds, had created a vicious cycle of neglect. The following quotation demonstrates this pattern:

> [the skeleton of the Irish elk] was one of the most famous of the Museum's geological specimens. This had been the first Magaceros skeleton displayed in an English museum, in 1836; mounted under John Phillip's supervision, it was acknowledged to be a particularly fine specimen. As its base was required for an aquarium display the skeleton was consigned, together with other, recent skeletal material, to totally inadequate storage in the changing huts surrounding the disused swimming pool. Decay and vandalism soon reduced both the fabric of these crude buildings and their contents into a heap of rubble and bones in the swimming pool, which was infilled and grassed over in the early 1970's (p. 129).

Not until the Museum had affiliated with the North Yorkshire Community Council, a municipal organization, did it become possible to obtain funds sufficient to retain professional staff and thereby tackle the basic problems of the collections.

Early members of the Museum's society included Sir Humphry Davy (1778-1829), Henry de la Beche (1796-1855), William H. Fitton (1780-1861), and the Reverend Adam Sedgwick (1785-1873). William Buckland (1784-1856) donated specimens, and William Smith (1769-1839) lectured at the Museum in 1824 and 1825.

The History of the Yorkshire Museum and its Geological Collections was both interesting and informative reading. The book provides a heartening example of thorough, scholarly research done on a local level.

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THE TITUSVILLE GUIDEBOOK--HISTORY OF PETROLEUM INDUSTRY SYMPOSIUM, SEPTEMBER 17-20, 1989. *Samuel T. Pees, editor. 1989. American Association of Petroleum Geologists, Tulsa, Oklahoma. 84 p. Softcover, $15.00. (order from the AAPG Bookstore)*

Samuel T. Pees and his colleagues have performed an invaluable service in preparing guides to three field trips in northwestern Pennsylvania, and a series of accompanying papers and vignettes. A great deal of the very earliest days of the oil industry are preserved in the sepia-toned photographs and text of these pages. Before reading this work, I had never heard of oil mining with pick and shovel, among many other facts.

Although the trips are short, where else can one go past the site of a well producing since 1861; or see the home of "Coal Oil" Johnny who spent $2,000 a day for a year and then, when the money ran out, went back to the farm; or see the site of the first commercial oil well (at 59 1/2 feet depth) in the United States? I liked the story of Ben Hogan, who anchored his boat of ill repute in the Allegheny River between two counties, where lawmen of neither one nor the other could arrest him; after all, oil workers need relaxation too.

If you want to know where the term "wildcatter" originated, buy this guidebook.

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EARTH FIRE. A HOPI LEGEND OF THE SUNSET CRATER ERUPTION. *Ekkehart Malotki and Michael Lomatuway'ma. 1987. Northland Press, Flagstaff, Arizona. 193 p. Softcover, $19.95.*

The significant part of this book is a bilingual translation of an oral Hopi legend, incorporating the eruption of Sunset Crater of Arizona, which began in 1064-65 A.D. The legend represents a unique, contemporary, literary confirmation of the eruption that has passed from generation to generation of storytellers over 600-900 years. This legend was related by an anonymous informer and has been retold and translated, by the authors, into English as well as its original Hopi.

Aliksa'i--this is a story about magic, about marriage between a kachina god and a mortal woman, about help from Old Spider Woman, about human betrayal and godly revenge. The gods take revenge through a severe famine and the culminating eruption of Sunset Crater. Unfortunately, the gods lose control over the eruption, and it continues until about 1250 A.D. The story is beautifully retold in a vivid and lively style that makes it delightful to read.

The book contains an extensive introduction, an archaeological and anthropological discussion of the ancient people of Sunset Crater (by P.J. Pilles, Jr.), and an overview of the geology, volcanology, and geomorphology of the region (by L. Middleton and R. Holm). To supplement the legend, the authors present a summary of the Hopi alphabet and a glossary in English and Hopi. These chapters form a well balanced background and are, except for a conspicuous lack of maps, very informative. Photos throughout the book are by S. Trimble.

The book is attractively produced and represents very high artistic and typographical standards. The authors and publisher are to be congratulated for this book. It should appeal to all geologists interested in the cultural impact of natural phenomena, or simply to all of us who like a good story and a beautiful book.

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COSMIC DEBRIS--METEORITES IN HISTORY. *John G. Burke. 1986. University of California Press, Berkeley. 445 p. Hardcover, $55.00.*

Cosmic Debris by John G. Burke is a detailed and exhaustive history of the development and growth of the science of meteoritics, the study of meteorites. The value of this book lies in its exploration of the roots and growth of meteoritics and as a "case study" of how prevailing scientific theories change.

Although meteorites have been falling to earth provably for the last million years, and plausibly throughout earth history, "Science" only began to recognize this phenomenon around the end of the 18th century. Before this time there were no lack of reliable witnesses to--and even examples of--meteorite falls and fireballs, but the prevailing theory, based on Aristotle, explained meteorites as products of the condensation of terrestrial "sulfurous exhalations." The book traces how this view changed with the spread of education, urbanism, and scientific knowledge (particularly developments in chemistry) in the Enlightenment of the 18th century.

After scientists accepted that stones could indeed fall from the heavens, there were no lack of theories to explain their origins. These theories tended to reflect the prevailing scientific orthodoxy and were often driven by advances in related fields such as chemistry and astronomy. The theories included proposals for atmospheric (a' la Aristotle), lunar, cometary, and cosmic origins.

During the 19th century, the development of more precise techniques and technology in astronomy, chemistry, and petrology produced a whole series of advances, including correlations between meteorite showers and comets, classification of meteorites based on their mineralogy, and the discovery of a number of minerals specific to meteorites. The 19th century also saw the beginnings and growth of the major meteorite collections in Paris, London, Vienna, Harvard, and the Smithsonian. The story of these collections is told along with the careers of their remarkable and often very idiosyncratic curators. The book follows meteorite research into the 20th century and ends with a chapter on contemporary research and current theories.

Perhaps the most enjoyable chapter reviewed the folklore and myths surrounding meteorites. These included everything from ancients Greeks worshipping stones in the temple of Artemis to Hungarians using a meteorite as a baking dish (since it made the bread taste better).

The strength of this book is in the phenomenal detail with which the history of meteoritics is documented. It exhaustively covers the development of ideas and theories and the contributions of the founders of meteorite science. This is an excellent book for the serious scientist in meteoritics who needs to understand the historical context of his science. It is also recommended for anyone interested in the evolution of scientific theories and the growth of science.

The book's weaknesses are in its treatment of current research, its answers to problems of 18th and 19th century science, and its detail. The book is too detailed and assumes too much mineralogical knowledge to be accessible to the general reader. The treatment of current research is by necessity abbreviated, but where the text covers the issues of 18th- or 19th-century science, the 20th-century view is often not given. The reader is often left wondering what the correct answer is to an earlier controversy.

In summary, this book is a tour de force of history on the science of meteoritics. It is excellent for anyone who has a strong background in meteoritics and wants insight into how scientific questions in that field have been formed, evolved, and answered over time.

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REVIEW ESSAY

How Did Impact Processes on Earth and Moon Become Respectable in Geological Thought?

A Review Essay Inspired by

Hoyt, W.G., 1987, *Coon Mountain Controversies*: University of Arizona Press, Tucson, 442 p.

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This is an important book by an extraordinary author, of interest to anyone fascinated by the ways in which unorthodox science becomes part of conventional wisdom. Coon Mountain is the original name for the feature between Flagstaff and Winslow, Arizona, that later became known as Crater Mound and is now called Meteor Crater; the book is subtitled "Meteor Crater and the development of the impact theory." William Graves Hoyt was Research Associate at Lowell Observatory in Flagstaff, until his death in 1985. He became interested in Meteor Crater and lunar craters in the 1960's, during a stint as managing editor of the *Arizona Daily Sun* of Flagstaff. His mentor was E.M. Shoemaker, who also wrote the foreword of this book. Academics who have reservations about journalists can relax. This is a thoroughly scholarly work, documented by 76 pages of notes. It deals with the reluctant acceptance by geologists and astronomers of the notion that impacts of meteorites, asteroids, and comets are legitimate geologic processes on Earth, the Moon, and other members of the solar system. The cast includes many famous scientists but centers on Daniel Moreau Barringer (1860-1929), a gifted and driven man who was ambitious, contentious, determined to the

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point of obstinacy, and eventually tragic.

Trained as a lawyer, re-educated (largely by himself) for a successful practice as geologist and mining engineer, Barringer considered himself a businessman and scientist but was, first and foremost, a promoter obsessed with a fixed idea: that a mass of meteoritic material with a mineable fortune in platinum, nickel, and diamonds lay buried some 1,200 to 1,500 feet below the south rim of Meteor Crater. In his mind's eye, the mass grew from >1 million tons in 1909 to >10 million tons in 1914; by 1919 he estimated its value at $700 million. From the time his Standard Iron Company located mining claims at Meteor Crater in 1903 until his death in 1929, Barringer spent a fortune (his own and other people's) on shafts and drill holes, all to no avail. During this period, he battled against a perceived conspiracy by the U.S. Geological Survey (USGS), first to deny or ignore his evidence for impact and later to rob him of the credit. In spite of his flaws, Barringer was a true pioneer in what is now called planetary geology or astrogeology. Hoyt shows him as a man whose mind was intuitive rather than analytical. It allowed him to visualize impact of a comet and to recognize the rudiments of shock metamorphism and lunar stratigraphy. It blinded him to any facts that threatened his emotional and financial investments.

Hoyt begins his tale on August 20, 1891, when A.E. Foote announced to the AAAS the discovery of diamonds in one of many iron meteorites that had been found near a crater-like feature in northern Arizona. One member of the audience lost little time: G.K. Gilbert, Chief Geologist of the USGS, promptly dispatched W.D. Johnson to investigate the crater and arrived there himself on October 31, 1891. Gilbert remained for 17 days, hoping to prove by the inductive method that the crater had been formed by impact of a "star". Failing to find a magnetic anomaly or volumetric evidence for a buried mass estimated at 500 million cubic feet, he rejected the impact hypothesis. Instead, he concurred with Johnson that Meteor Crater was the result of a steam explosion, induced by subsurface volcanism of the type now called phreatomagmatic. The Hopi and San Francisco volcanic fields, within sight of Meteor Crater, were evidence that northern Arizona had a long history of explosive volcanism. Coincidentally, it was also the site of a meteorite fall. In 1895, near the end of his only publication on Meteor Crater, Gilbert hinted that his conclusions were tentative.

In 1906, the opposite view was expounded in two papers by, respectively, Barringer and his partner, B.C. Tilghman. They noted that ejecta from the crater was crushed, unsorted, and mixed with oxidized meteoritic material ("shale"). Tilghman appears to have been a more acute observer than Barringer; he even noted inverted stratigraphy among ejecta blocks. Later that year, S.L. Penfield of Yale identified a sample of dense slaty metamorphosed rock submitted by Barringer as Coconino Sandstone "absolutely ruined in situ." In a letter, Barringer described it as "an impactite," deformed by a shock wave that "passed through and shattered the particles without displacing them." These conclusions were soon confirmed H.L. Fairchild (University of Rochester) and G.P. Merrill (U.S. National Museum). In 1960, E.C.T. Chao, E.M. Shoemaker and B. Madsden (USGS) would identify coesite in samples of this material collected by Merrill in 1907 (a curious fact, confirmed verbally by E.M. Shoemaker and E.C.T. Chao). In 1962, Chao and several USGS colleagues also identified stishovite. These high-pressure polymorphs of SiO_2 became widely accepted as diagnostic for impact, if found in rocks of the upper crust. Another metamorphosed rock, superficially resembling pumice or tuff, was identified by Merrill in 1907 as vesicular glass, from melting of quartz sandstone. In 1927, A.F. Rogers (Stanford) identified the material as lechatelierite and concluded that its temperature of formation of about 1600°C precluded origin by steam explosion and pointed to impact of a meteorite.

Barringer amplified his interpretations in several subsequent papers and also applied the impact hypothesis to the Moon, after an evening at the Swathmore College Sproul Observatory in 1913. In 1914, he published a comparison of lunar craters and Meteor Crater and in 1924 he noted a succession of lunar events, based on superposition, that anticipated the lunar geological time scale proposed by E.M. Shoemaker and R.J. Hackman in 1962.

During his lifetime, Barringer convinced most geologists and other scientists of the scientific importance of Meteor Crater and of its origin by impact. The Swedish chemist Svante Arrhenius called Meteor Crater "the most interesting feature on the surface of our planet" and the astronomer W.W. Campbell (Director of Lick Observatory) "the eighth wonder of the world." Why, then, did Barringer die an angry and frustrated man? Hoyt recounts in detail his ceaseless struggles to raise capital for mining the meteorite, pursued with singular lack of scruples. He tried to use influential friends, such as Theodore Roosevelt (T.R. declined). He unsuccessfully approached Arrhenius, L.H. Baekeland (father of the age of plastics), the directors of General Electric, and the trustees of Princeton, MIT, and the American Museum of Natural History with promises of scientific fame as well as fabulous profits. He was more successful with U.S. Smelting, Refining, and Mining Company and other investors, but all efforts resulted in frustration and financial loss. Barringer blamed the broken and water-saturated ground and mistakes of his partner Tilghman, but the real reason was his own obstinate refusal to recognize that the gigantic meteoritic mass did not, and could not, exist. Ironically, neither he nor Gilbert had considered the effects of kinetic energy in melting, disrupting, and dispersing a high-velocity meteorite of much smaller mass than Barringer's assumed 10 million tons. As early as 1906, A.C. Lane, State Geologist of Michigan, pointed out that the meteorite would have been blown out of the crater. In 1908, Merrill suggested that much of the meteorite had been volatilized and the rest oxidized and ejected. Having reviewed a 1903 study of the Moon by N.S. Shaler, Merrill knew that the kinetic energy formula, $e = \frac{1}{2}mv^2$, would apply. The physicist M.F. Magie, a Dean at Princeton University and an eventual investor in Standard Iron Company, pointed out in 1910 that mass (m) could vary between 60,000 and 15 million tons, depending on velocity (v). He calculated the most reasonable estimate for m at 400,000 tons, for v = 18 to 20 mi/sec (30 to 33 km/sec). This figure is close to a modern calculation of 300,000 tons, by D.J. Roddy (USGS). Roddy's colleague, E.M. Shoemaker, tells me that he would now estimate the mass between 500,000 and 1 million tons, of which only one-third to one-half remains in the crater, dispersed as droplets in the shock-melted lining of the initial cavity.

Barringer was adverse and hostile to mathematical physics; he relied on practical experience with bullets and artillery projectiles. To be worth mining, the object buried beneath Meteor Crater must have large m; consequently v must be small. Over the years, he developed the notion that it was the head of a low-density comet, with metallic iron-nickel embedded in oxidized material, and with velocity retarded by the atmosphere. His ideas on comets found no favor with astronomers, such as his distinguished associate, H.N. Russell of Princeton.

On September 28, 1929, just as Barringer was making the final financial arrangement for a renewed mining operation, the astronomer F.R. Moulton (University of Chicago) upset his plans with calculations which considered the velocity, density, and orbit of a dense swarm of meteorites, as well as air resistance, depth of penetration, energy expended in crushing the target, and effect of compressed air at the leading edge. He concluded that the mass of meteoritic material was only around 300,000 tons. In a revised 127-page report, Moulton formulated 81 equations, carried out 1,300 computations, and finally set an upper limit of 500,000 tons for the meteoritic mass. Furthermore, he concluded that the fragmented meteorite and crushed rocks would have been ejected from the crater. His second report gave the *coup de grace*, not only to Barringer's hopes but to Barringer himself. It was received on November 23, 1929; a week later, Barringer died of coronary thrombosis. In his last days he was in the midst of a frantic debate, fulminating against impractical scientists and vainly trying to rally his supporters and investors. The stock market crash of October 1929 must have been an added burden. Shortly after Barringer's death, P.W. Bridgman, the Harvard pioneer in high-pressure experiments, confirmed Moulton's conclusions. In 1954, the meteoriticist H.H. Nininger estimated that as much as 100,000 tons of meteoritic material had been vaporized, condensed, and ejected from the crater as minute metallic spherules, many of them still preserved in soil.

Barringer's partners struggled on with inconclusive geophysical surveys and further drilling, but money ran out in 1932. In 1951, Standard Iron Company became Barringer Crater Company and successfully developed the crater as a tourist attraction. In the interim, a few hundred tons of impact-comminuted Coconino Sandstone from the crater rim were sold as silica sand, partly for use in a scouring powder called Star Dust. In 1948, during my senior year at the City College of New York, I spotted a can of the stuff in the Department office and used it to wash my grimy hands; it removed dirt and a good bit of epidermis. It also earned me a rebuke from a horrified Department Chairman, D.T. O'Connell. It turned out that the can was Professor O'Connell's cherished souvenir from a visit to Meteor Crater in the 1930's.

There were other causes for Barringer's anger and frustration. Geologists were slow to accept his evidence for the impact origin of Meteor Crater. Hoyt writes of "the tendency of most scientists when faced with an unorthodoxy to follow conventional wisdom on matters on which they have no personal knowledge". In my opinion, some of this can be ascribed to simplistic interpretations of the history of geology (how the good-guy uniformitarians beat the bad-guy catastrophists). Barringer himself blamed the incompetence of scientists in general ("My experience is about 9 out of 10 people who are referred to by this appellation are not entitled to it, and I think 99 out of 100 would be nearer the mark") and the machinations of the USGS ("the enemies' forces") in particular.

Gilbert refused to engage in public controversy, which was in keeping with his character, but there are indications that new evidence changed his mind. In 1906, J.C. Branner, President of Stanford University, wrote to Barringer about a private conversation, in which Gilbert "considered you and Tilghman had brought forward evidence that entirely changed the conclusions he had drawn regarding the origin of the crater." H.L. Fairchild, who knew and respected both Gilbert and Barringer, wrote about Gilbert in 1929: "It is difficult to understand how he came to favor volcanism...Most certainly he later knew his mistake." However, Gilbert never retracted his steam-explosion hypothesis in public, to Barringer's fury. Some of Gilbert's USGS colleagues refused to acknowledge new evidence, probably out of respect for Gilbert and hurt professional pride. N.H. Darton argued against impact from 1905 until at least 1945; it was he who convinced the U.S. Board of Geographical Names in 1932 to adopt the name Crater Mound. Not until 1946 did Eliot Blackwelder (Stanford) persuade the Board to accept the popular name Meteor Crater. proposed by Fairchild in 1906 (purists of the Meteoritical Society correctly point out that meteors are mere light flashes in the sky; they prefer the name Barringer Meteorite Crater). Darton's reputation is not enhanced by his stand. He was a great reconnaissance geologist but neither here nor (in my experience) elsewhere did he demonstrate the attention to detail that could have solved the Meteor Crater problem. Much of what he wrote about the crater in 1910 either ignores the obvious (e.g., the abundance of meteoritic material) or is simply wrong (e.g., his identification of ejected material as steam-bleached redbeds from the Supai Formation rather than Coconino Sandstone). Darton was impressed by a superficial resemblance between Meteor Crater and the Zuni Salt Lake maar in western New Mexico, but not by obvious differences: there are two basalt cinder cones in Zuni Salt Lake, basalt ash and lapilli in its rim, and a basalt ring dike around its periphery. None of these statements can be made about Meteor Crater.

Was there, in fact, a conspiracy on the part of the USGS? Here one must distinguish between official policy and the tendency of all organizations to rally to the defense of a member as distinguished as G.K. Gilbert, especially if attacked by someone without formal credentials in the scientific establishment. In 1928, Barringer had reason to complain about the attitude of the Director, G.O. Smith, who credited Gilbert with the impact hypothesis and ignored Barringer's contribution. An earlier Director, C.D. Walcott, had in 1906 failed to reply to Barringer's challenge for a reinvestigation of the crater but, as Secretary of the Smithsonian Institution, had in 1907 approved Merrill's visit to the crater and subsequent investigations. In balance, the evidence suggests that individual USGS members were hostile to Barringer's theories and methods but falls short of proving an organized conspiracy.

Even outside the USGS, a minority of geologists continued to doubt the reality of meteorite impact. Between 1926 and 1953, the petroleum geologist Dorsey Hager argued for tectonic uplift along the axis of the Holbrook anticline, followed by slumping and collapse caused by solution of limestone and evaporites. By 1960, the question was settled in most people's minds by E.M. Shoemaker, who combined a thorough knowledge of shock mechanics, and experience with nuclear test craters, with accurate mapping of Meteor Crater. Although not mentioned by Hoyt, detailed knowledge of stratigraphy was the key to Shoemaker's structural interpretation; the subsequent discovery of coesite and stishovite clinched an already airtight case. What would Barringer have said, had he known that in 1959 his old USGS enemy would establish a Branch of Astrogeology at Flagstaff? With Shoemaker as its first Chief, it quickly became a leading center for studies of impact and other planetary processes.

The last notable skeptic was W.H. Bucher (Columbia), who questioned the impact interpretation in 1963 but was persuaded to the opposite view for Meteor Crater (but not for other proposed impact structures) in the following year. This occurred during a tour of the crater guided by Shoemaker. The incident, which I was privileged to witness, is not mentioned by Hoyt but is recounted in the 1987 book *Meteorite Craters* by Kathleen Mark. It is of historical interest, because Bucher was very influential among his students and colleagues. Some of the students who attended Bucher's marvelously persuasive lectures (C.L. Drake, C.B. Officer, K.K. Turekian) have been critical of certain aspects of impact processes into the present, especially as they relate to the Cretaceous-Tertiary boundary problem. Moreover, Bucher's Columbia colleague, Arie Poldervaart, was a staunch advocate of lunar volcanic cratering through defluidization. Poldervaart and his former student Jack Green (now Long Beach State University), published a defense of this concept in the Proceedings of the 21st International Geological Congress (1960), which also carried Shoemaker's classic interpretation of Meteor Crater. I also was among Bucher's and Poldervaart's students and was influenced by their views throughout the 1960's. It should be emphasized that neither was dogmatic and that both were more interested in stimulating thought and debate than in seeing their views prevail. They were, incidentally, on opposite sides of the contemporaneous debate on continental drift, Bucher against and Poldervaart passionately in favor. Unfortunately, both died just as hard evidence on both controversies began to appear, Poldervaart in late 1964 and Bucher in early 1965.

Whatever the attitude of skeptics during Barringer's lifetime, Hoyt leaves no doubt that Barringer caused many of his own troubles. Science and single-minded promotion of a speculative commercial enterprises do not mix. Barringer was aggressive and imbued with the robust business methods of his day. He simply could not understand gentlemanly scientists like Gilbert and Merrill, who looked at all sides of every question and avoided public confrontations. He regarded them as unmanly and with the suspicion (especially directed at Merrill) that they were plotting to rob him of credit for his discoveries. In their turn, scientists were wary of a promoter's claims. It is, in fact, difficult to draw the line between Barringer's science and promotion. He admitted submitting his 1924 paper on lunar craters to Scientific American "to draw the attention of people of wealth to the Crater." Even scientists who had an open mind on the origin of Meteor Crater rebuked him on occasion. "To urge a particular theory is the work of the advocate, not the scientist" wrote the meteoriticist O.C. Farrington (Field Museum) in 1916. In 1921, H.N. Russell admonished him that "you do not use the language common among men of science."

Many geologists of Barringer's day declined to become involved in the Meteor Crater controversy. Their silence may be as significant as that of the dog that failed to bark in the night, in Sherlock Holmes' estimation. The senior author of the principal introductory geology textbook of Barringer's day, T.C. Chamberlin (University of Chicago; associated with Moulton in formulating the planetesimal hypothesis), continued to cite Gilbert and declined Barringer's invitation to see for himself. The leading authority on the geology of northern Arizona was H.E. Gregory (Yale and USGS), but Hoyt does not mention him among his sources and I do not recall any discussion of Meteor Crater in Gregory's writings. One of my senior colleagues, the paleontologist S.A. Northrop, does not remember any mention of Meteor Crater in Gregory's lectures at Yale in 1920's. However, Northrop remembers lively interest in Meteor Crater, which induced him to pay a visit during his honeymoon in early 1930. He found Barringer's interpretation convincing and thinks most geologists of that day would have agreed.

The Meteor Crater story is closely documented in 8 out of 14 chapters in Hoyt's book and in parts of 2 more. The rest is devoted to the development of ideas concerning lunar craters, the link between lunar craters and Meteor Crater, and the recognition of terrestrial impact structures other than Meteor Crater. Hoyt summarizes the early history of lunar studies, beginning with Galileo in 1609. By 1849, the astronomer Sir J. Herschel concluded that lunar craters were volcanic, an opinion shared by pioneer geologists like Elie de Beaumont and J.D. Dana. In the 1870's, the British astronomers James Nasmyth and James Carpenter popularized the volcanic explanation, whereas their countryman R.A. Proctor favored meteorite impact. The views of Nasmyth and Carpenter become widely, but not universally, accepted. In 1892-93, G.K. Gilbert again entered the picture, this time on the side of impacts by "moonlets", derived from a primordial Saturn-like ring around the Earth. His principal argument in favor of impact was the size of the largest lunar craters, in which he included Mare Imbrium, 600 mi (1,000 km) in diameter. The diameters of the 10 largest terrestrial volcanic craters known to Gilbert averaged only 11 mi (18 km). Critics of the impact hypothesis had cited the circularity of lunar craters, which is the reason why Gilbert postulated low-velocity "moonlets" striking the lunar surface at right angles rather than high-velocity meteorites or asteroids striking at random angles. Proctor had explained circularity by assuming impact into a plastic surface. As at Meteor Crater, Gilbert and his contemporaries ignored the effects of kinetic energy.

Another objection to lunar impacts was the absence of known impact craters on Earth. This is where Meteor Crater entered the picture. The connection was made in 1909 by two geologists, Franz Meineke in Germany and E.H.L. Schwarz (Rhodes University, South Africa), who both cited Barringer and Tilghman. In 1912, it was elaborated by the inventor Elihu Thomson, a founder and chief scientist of General Electric. Thomson became Barringer's close friend, scientific advisor, and an investor in Standard Iron Company. It was Thomson who directed Barringer's attention to the Moon.

The role of kinetic energy in creating lunar impact craters was recognized by the American astronomer T.J.J. See in 1909 but tied to an eccentric "capture" theory that was demolished in a review by Moulton. Kinetic energy was invoked by the New Zealander A.W. Bickerton in 1915 and worked out in detail by the well-known Estonian astronomer E.J. Öpik (later Armagh Observatory, Northern Ireland) in 1916. Öpik's paper, published in Russian during the height of World War I, attracted little notice. That catastrophic war directed the attention of the American physicist H.E. Ives to explosions and subsequently to the origin of lunar craters from the kinetic energy of impacts. Later, E.M. Shoemaker would point out that the excavation of a crater was not so much the result of an explosion (i.e. volatilization of a projectile) as the result of energy partitioned into shock waves.

Hoyt gives major credit for applying kinetic energy to a 1924 paper by Bickerton's student, the New Zealand astronomer A.C. Gifford. Gifford triggered a debate in that stronghold of lunar volcanism, the British Astronomical Association, in 1925-26. Hoyt rates the score as a draw: six participants favored lunar craters by impact, six by volcanism, and three by a preposterous glacial hypothesis. Prominent members of the British Astronomical Association, such as the popular writer Patrick Moore, continued to defend the volcanic origin of lunar craters into the Apollo period. In America, distinguished astronomers like G.E. Hale (Mount Wilson), W.H. Pickering (Harvard), and W.W. Campbell (Lick) continued to favor lunar volcanism in Barringer's day. H.N. Russell, who supported Barringer in all aspects of Meteor Crater until Moulton changed his mind, commented on Barringer's 1924 paper on lunar craters: "Several geologists will agree with him but the astronomers who have seen the moon under the most favorable circumstances are practically as a unit against him." Two years earlier, the apparent conflict between geologists and astronomers had prompted a famous comment (not cited by Hoyt) from William Morris Davis, made in a discussion of G.K. Gilbert's views on the Moon:

> "It has been remarked that the majority of astronomers explain the craters of the moon by volcanic eruption that is, by an essentially geological process -- while a considerable number of geologists are inclined to explain them by the impact of bodies falling upon the

moon that is, by an essentially astronomical process. This suggests that each group of scientists find the craters so difficult to explain by processes with which they are professionally familiar that they have recourse to a process belonging in another field than their own, with which they are probably imperfectly acquainted and with which they therefore feel freer to take liberties."

Davis was no stranger to Barringer. In 1912, Barringer had persuaded him to organize an excursion to Meteor Crater by a international group of leading geographers which (in Hoyt's words) "proved to be a promotional fiasco from Barringer's point of view."

In 1944, the American geologist J.E. Spurr defended lunar volcanism in two volumes and dismissed impact on the grounds that Earth lacked an impact record, aside from Meteor Crater. Actually, two more small terrestrial impact craters (Odessa, Texas, and Ösel, Estonia) were discovered during Barringer's lifetime; Barringer unsuccessfully tried to buy Odessa Crater for another mining venture. By the mid-1930's other small impact craters had been found. In 1936, J.D. Boon and C.C. Albritton reinterpreted much larger features as deeply eroded impact scars, later termed "astroblemes" by R.B. Dietz (now Arizona State University), and previously identified by Bucher and others as "cryptovolcanoes." Cryptovolcanism implied explosions of subsurface volcanic gases, somewhat along the lines of Gilbert's explanation for Meteor Crater. The impact and cryptovolcanic interpretations were both bold, because these mysterious structures yielded no traces of either meteorites or conventional volcanic rocks, although they may contain shock-induced melt rocks. Many contain shatter cones, proposed as criteria for impact by Dietz in 1946. In the same year, Dietz argued for impact origin of lunar craters and in 1949 the subject was treated in detail by R.B. Baldwin.

Hoyt treats the discovery of terrestrial impact craters other than Meteor Crater rather briefly. The tektite controversy is not mentioned. Of the many workers who developed petrographic criteria for recognizing impact structures, he mentions only N.L. Carter (now Texas A & M), whom he credits with first recognizing the significance of planar lamellae in quartz in 1965. In the authoritative symposium on Shock Metamorphism, held in 1966 and published in 1968, the editors (B.M French and N.M. Short, NASA) and Carter himself give priority to a 1962 abstract by D.B. McIntyre (Pomona College). Aside from those already mentioned, W. von Engelhardt (Tübingen) and his former students D. Stöffler (now Münster) and F. Hörz (now NASA); M.R. Dence (now Royal Society of Canada), T.E. Bunch (NASA), and their associates were establishing a spectrum of criteria for shock metamorphism in the early to mid-1960's. As Hoyt's book is incomplete on these topics, Kathleen Mark's book makes an excellent complement.

Hoyt ends his detailed account with Shoemaker's contributions to lunar and terrestrial impact problems and briefly summarizes the results of space exploration. The question has passed beyond impact vs. volcanism to the relative roles of each. In the post-Apollo era, the Moon is recognized as an igneous body cratered by impacts. On Mars, the impact record has been partly obscured by volcanism and eolian sedimentation. On Earth, only remnants of the impact record remain and on Io it has been totally obliterated by volcanism. The jury is still out on Venus. The debate over terrestrial cratering has now shifted to the validity of criteria for recognizing impacts and the problem of mass extinctions, to which Hoyt devotes one paragraph.

Hoyt tells the victor's side of the impact story; he might have mentioned that both sides of the debate made exaggerated claims. In the 1960's, for example, J.J. Gilvarry (Rand Corp.) and T. Gold (Cornell) represented extremes on the lunar impact side by rejecting all evidence for lunar volcanism, but for opposite reasons. For Gilvarry, Moon and Earth were so much alike that Earth oceans were interpreted as impact basins and lunar maria as impact basins filled with organic-rich sediments; for Gold, the Moon was so unearthly that geology could not be applied to its study. In Hoyt's account, the lunar impact vs. volcanism debate came to a head in a 1964 symposium, convened by Jack Green and sponsored by the New York Academy of Sciences (he gives the date as 1965, the publication year of the proceedings, edited by H.E. Whipple). I chaired two sessions and have vivid memories. The contributions ranged from serious reports on the state of science to wild speculations (one East European astronomer proved lunar volcanism mathematically in 1 1/2 pages and cited 10 references, all of them by himself). I tried to document the existence of large terrestrial volcano-tectonic structures to counter Gilbert's argument on the disparity in crater diameters. However, I also reminded the audience that no miracle could shield Earth and Moon from impacts by objects in Earth/Moon-crossing orbits. The debate was fun while it lasted. The symposium convened in May; in August of that year, Ranger 7 reached the Moon and hard scientific evidence began to accumulate. Luniks, Surveyors, and Orbiters soon followed, Apollo 11 flew in 1969, and the party was over. In Hoyt's words, lunar rocks and soils "left no doubt that impact had played a dominant role in bringing the moon to its present condition." Lunar volcanic rocks turned out to be depleted in water and other volatile components, which ruled out all volcanic crater models (including mine) based on massive lunar defluidization. However, the rocks "showed that vulcanism...has also been operative through lunar history."

Unlike most scholarly works, this book is written in plain English; its style follows the "who, what, where, when, how" format drilled into all good journalists. It can also be read for pleasure, like a Russian novel in which characters appear, disappear, and reappear in unexpected places. You will be amazed at the emotions aroused by a big hole in northern Arizona. Read it!

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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

A session on Science and Religion will be held at the biennial meeting of the International Society for the History, Philosophy & Social Studies of Biology will be held at Northwestern University, Evanston, Illinois, July 11-14, 1991.

For centuries, the practitioners of science and religion have been at loggerheads over such issues as a flat or spheroidal earth; the geocentric or heliocentric model for planet distribution; use of anesthetics for childbirth; use of cadavers in anatomy; the body injection of vaccines, blood, serums, etc. Generally, agreement and conciliation has been reached over these issues for most people. Ways and means must exist for resolving the current issue of creation and biological evolution, which has erupted periodically over the last 13 decades since Darwin's *Origin of Species* was published. A repetition similar to the disruption during the last two decades must be prevented. Confrontation, debates, a multitude of books and papers, and legal action up to the highest court have not accomplished resolution. So what can be done??

Papers are invited from experts in either science or religion, or with expertise in both, who share a common objective of--

Broadening the Appreciation and Understanding and, more importantly, Initiating Resolution However Long it Takes.

Some of the subjects which can be addressed include --

1. The science/religion issue is indigenous to Christianity. The contention seems to be absent from Buddhism, Hindu, Islam, Zoroastrian, Baha'i, etc. What can they teach with respect to resolution??

2. With respect to issues of the past, how was the rationalization and conciliation accomplished?? or was it?

3. Some of the theologians of the past who advocated biblical inerrancy, such as B.B.Warfield and A.H. Strong, accepted biological evolution and perceived harmony rather than contradiction. Who are the present day theologians holding similar views and how do they rationalize the issue?

4. Many eminent scientists who lived before Darwin, had a positive appreciation of theology? Who are the modern counterparts, including biologists, with similar views on theology?? How do they rationalize and compromise the two views?

5. What has been the actual cost in resources, money, and time for one institution, organization, etc., resources which could have been better spent on its designated goal??

6. What understandings, relationships, and compromises will establish mutual trust between antagonists and allow a forum for calm discussion to explore for mutually agreeable and acceptable compromise and reconciliation?

7. What methodology can be or is being used to initiate resolution of the current issue that is any means which will initiate resolution other than the antagonists just becoming weary of arguing and publishing, but little or nothing resolved?

Types of papers which will NOT be accepted include the strawman analysis, 'goring the other guy's ox', the 'impossible' scenario, "you are wrong, therefore I am right" scenario, the anathemic consequences of accepting a certain world view, etc. The format of the session will be determined by the response. There can be a succession of formal paper, panelists with short explanations followed by open dialogue, a workshop, whatever.

Abstracts from authors are due by January 31, 1991.

For information contact: C. Gordon Winder, U.W.O. Geology, London, Canada N6A-5B7, (519) 661-3198

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KUDOS

Kennard B. Bork, HESS secretary, is well known by the society membership to be genial, hard working, and extraordinarily efficient. These same characteristics are found in his work as a faculty member at Denison University, and this has not gone unnoticed. Several decades ago, the alumni of that institution established an Alumni Chair. Appointment to this Chair is for life, and has moved from department to department over the years to allow particularly distinguished and dynamic teachers to be honored. This year, Professor Bork became the first faculty member from Department of Geology and Geography to be appointed Alumni Chair. Congratulations, Ken, from your colleagues in the History of Earth Sciences Society!

Editor Gerald M. Friedman and Sue Friedman (whose unflagging enthusiasm, energy and efficiency make her an invaluable part of the process of publishing *Earth Sciences History*) were conferred the award of Honorary Alumni of the Geology Alumni Association of Brooklyn College of the City University of New York in recognition of "Outstanding Service to the Geology Alumni Association."

CALENDAR

1990

Oct. 25-28 - History of Science Society Annual Meeting, Seattle, Washington U.S.A. Contact Peter Galison or Timothy Lenoir, Program in the History of Science, Building 200-33, Stanford University, Stanford, California, U.S.A. 94305. Telephone: 415/725-0714.

Oct. 25-31 - INHIGEO Symposium/Meeting in conjunction with the Seventh Symposium of the Committee of the History of Geology, Geological Society of China. Beijing, People's Republic of China. General Topic: Interaction of geological thought between East and West. Four of the main components of the scientific program are: 1) History of interchange of geoscience ideas among China, Europe and America; 2) important events which promoted communication of geologic thought between East and West; 3) history of interchange of ideas in the main branches of geology, including seismogeology, stratigraphy, tectonics, petroleum geology, etc.; 4) biographical notes of geologists who made great contributions to the interaction of geological sciences and geological undertakings between East and West. To obtain a copy of the second circular write: Prof. Tao Shilong, China University of Geosciences, 29 Xueyuan Road, Beijing 100083, People's Republic of China.

Oct. 29-Nov. 1 - Geological Society of America Annual Meeting, Dallas, Texas U.S.A. General Chairman: Dr. David E. Dunn, Dean of Science, University of Texas at Dallas. Abstracts due July 11, 1990. For Abstract Forms and information, contact 303/447-2020; 800/472-1988.

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 Committee. Activities include geo-turistic visit of Capri, scientific sessions and post-symposium excursions to classic algal localities in the surroundings of Naples. Official Languages: English, French and Italian. Contact Dr. Maria Carmela del Re, 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 Languages: Spanish and English. Contact Dr. Fermin Villarroya, Chairman, Congress Organizing Committee, Departamento de Geodinamica, Facultad de Ciencias Geologicas, Universidad Complutense, 28040 MADRID Spain. Telephone: (34-1)449-73-91; Telex: 41798 UCGEO; Telefax: (34-1)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 or 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.

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

OFFICER REPORTS

REPORT OF THE SECRETARY FOR 1989

The Society finished out the 1980s with another strong year.Memberships and institutional subscriptions continued to climb, the Albritton Memorial Issue was published (Vol. 8, no. 2), and a Membership Directory was distributed to Society members in 25 countries around the world. The 28th International Geological Congress (Washington, D.C., July '89) featured several sessions relating to the history of the geosciences, and a number of IGC participants joined the Society.

President Ellis L. Yochelson was an active leader, corralling new members, working to expand our communications with the History of Geology Division of GSA and the International Commission on the History of Geology, and investigating ways to improve services to everyone in the Society. At the Washington IGC meeting he was instrumental in arranging a very enjoyable and informative afternoon session at the Dibner Rare Book Room of the Smithsonian Institution.

Our 1989 election yielded a high return of ballots. Ursula B. Marvin (Harvard-Smithsonian Center for Astrophysics) is our President-Elect, and Gordon Y. Craig (Edinburgh) is Councilor. Dr. Marvin will take over from 1990 President Robert H. Dott, Jr., and Prof. Craig will serve as Councilor through 1991. Also elected, to continue in their present capacities, are Treasurer Kenneth L. Taylor (Oklahoma) and Program Officer James E. Brooks (S.M.U.-Institute for the Study of Earth and Man).

The April 1989 mailing was our biggest to date. It included the Membership Directory, dues notice, ballot, and a few enclosures about books and events of potential interest. Also enclosed was a H.E.S.S. membership brochure, so that it would be easy for current members to invite others to join the Society. We still are anxious to build the Society's strength, especially for institutional subscriptions, so feel free to contact me if you need more brochures.

Society officers are sorry to report the deaths of three colleagues. Carol Faul died in May of 1989, John G. Dennis in September, and Walter H. Wheeler in November.

Earth Sciences History is another year old, and is now entering the '90s as a robust 8-year old. All of us owe a large vote of thanks to Gerald M. Friedman, Sue Friedman, and the staff at the Northeastern Science Foundation, for their continued efforts on behalf of the Society.

It is not strictly 1989 news, but you might want to be aware that, as of early 1990, distribution of back issues will be handled by Thomas E. Pickett (Delaware Geological Survey, Newark, DE 19716). Overstocked numbers will be offered at a discounted price -- please check your 1990 Dues Notice for details.

Respectfully submitted,
Kennard B. Bork

REPORT OF THE TREASURER FOR 1989

In summer 1989 Thomas E. Pickett assumed the post of Associate Treasurer, with responsibility primarily for subscriptions and sale of back issues. Dr. Pickett has contributed significantly to the increased efficiency of the Society's fiscal management.

The Treasurer and Associate Treasurer accounted for Society revenues of just over $17,000 in 1989 (from membership dues, subscriptions, sale of back issues, interest, and contributions). This figure does not include the generous contribution of the Institute for the Study of Earth and Man, Southern Methodist University, toward publication of volume 8 (2) of *Earth Sciences History*.

As always, HESS income in 1989 was spent largely on production and distribution of *Earth Sciences History*. The other expenditure of note was for the Society's new membership directory. The 1989 dues increase (to $20) has strengthened HESS finances somewhat despite rising costs. The Society's accounts as of 31 December 1989 held $8,189.24.

On behalf of the Society, the Treasurer acknowledges with gratitude the generous contributions received in 1989 from the following individuals:

Duncan Carr Agnew, Michele Aldrich, Kennard B. Bork, Arthur L. Bowsher, Stephen G. Brush, Albert V. Carozzi, Marguerite Carozzi, Gilbert Corwin, Allen Debus, A. G. Doré, Robert H. Dott, Jr., Bruce Francis Elchison, W. von Engelhardt, Robert H. Fakundiny, Mike Foster, David H. Geiser, R. N. Ginsberg, W. Dean Grafton, Henry G. Healy, David H. Hight, Arthur F. Krueger, Walter O. Kupsch, Rachel Laudan, Alan Leviton, Joel J. Lloyd, Gary E. Melickian, E. D. Menkes, Robert C. Milici, Anne Millbrooke, Ellen J. Moore, G. B. Morey, Barbara L. Narendra, Sally Newcomb, Herbert P. Obodda, William A. Oliver, Jr., Thomas R. Osberg, Alexander M. Ospovat, George Rapp, Jr., A. W. A. Rushton, Robert R. Shrock, Marie Siegrist, Brian J. Skinner, Jirí Strnad, Rudolf Truempy, Carmina Virgili, Peter M. Whelan, Karin L. Willoughby, Stephen S. Winters, Ellis Yochelson.

Grateful acknowledgement is also made of a contribution toward page costs from the United States Geological Survey.

Respectfully submitted,
Kenneth L. Taylor