ESSAY REVIEW

THE GREAT DINOSAUR EXTINCTION CONTROVERSY AND THE K-T RESEARCH PROGRAM IN THE LATE 20TH CENTURY

The Great Dinosaur Extinction Controversy. *Charles B. Officer and Jake Page, Reading, MA: Addison-Wesley Longman, Inc, 1996. 209 pp. Hardcover, $25.00.*

The K-T Research Program. In the last two decades of the twentieth century, renewed research has tried to discover what happened on the surface of the earth 65 million years ago during the geologically brief, but somewhat indefinite time interval, K-T time, at the end of the Cretaceous period and the beginning of the Tertiary sub-era. To a remarkable degree, this renewed K-T research program relies on or reacts to interpretations of physical and chemical data by Alvarez, et al.[1] These interpretations have been less easily integrated into the body of established geological work than interpretations of data from more traditional twentieth-century research programs applying physics to geology.[2]

Custom provides the abbreviation, K-T, as an adjective[3] referring to that brief indefinite time interval during which rocks including the boundary between Cretaceous and Tertiary times[4] were deposited. K, from Kreide, the German word for chalk, and T are the accepted abbreviations among geologists for Cretaceous and Tertiary.

When their science was evolving into recognizably modern form during the opening decades of the nineteenth century, many geologists had their professional lives centered around London or Paris, where the London and Paris sedimentary basins exhibit exceptional lithologic and paleontologic discontinuities at the K-T boundary.[5,6] The Cretaceous chalk formed the basement in both basins, and its weathered surface impressed the early geologists with the gap in time between the chalk and the later formations, including K-T time in present terms. Initially, the best-documented paleontologic change was in bivalves[6], rather than in the ammonites, calcareous nannofossils, or dinosaurs that now are noted. Dinosaurs came to light piecemeal in the second quarter of the nineteenth century, and it was found during the last half of nineteenth century that fossils of the latest dinosaurs occur in Cretaceous but not in Tertiary rocks. It has always been the case that such large vertebrates were very much less abundant than invertebrates, as for example, the number of elephants now alive is insignificant compared to the number of clams in the sea. For this reason, shells are much more useful than bones in dating geologic events.

The K-T research program has been concerned primarily with the cause of extinctions at the K-T boundary. The supposed cause of extinctions has usually reflected ideas of the society from which the cause emanated. Cuvier advocated catastrophic sea level changes as a cause of extinction at a time when Noah's flood was a viable geologic hypothesis and the idea of extinction was new. Jameson's elaboration of Cuvier[7] may have been influenced by the location of Edinburgh, off the North Sea, and by Deluc's description of catastrophic storm surges along North Sea coasts. Senescence of the biologic races was an option when social Darwinism diffused into geology[8]. Greenhouse effects leading to sterility[9] entered consideration shortly after the energy crisis of the early 1970s led to increased concern over excess CO_2. Extinction from genetic defects induced by increased ultraviolet light[10] made the list contemporary with concern about Freon damaging the ozone layer. The prevailing theory of an 'extraterrestrial cause' was published one decade after the peak in the American public's interest in space exploration, and one year after the Pioneer Venus and Voyager Jupiter results became available.[11] It may be more of a stretch, but Mt. St. Helens exploded on 18 May 1980, making a significant impression on the U.S. population, and subsequent eruptions and lahars continued up to the 6 June 1980 publication of Alvarez, et al[1]. Volcanism has been the only alternative to the impact theory since that time, and Mt. St. Helens is mentioned (p 171) when Officer and Page[12] present their volcanic theory.

The Book. The occasion of this essay is the recent book, *The Great Dinosaur Extinction Controversy,* by Officer and Page,[12] who question the conduct of K-T science from their perspective as proponents of the volcano theory for explaining K-T time. Officer is a Dartmouth research professor in geology, and Page is a science writer[13]. Officer and Page are spokesmen for the dwindling band that favors a volcanic explanation for the iridium anomaly, and a combination of causes, traceable to volcanism, for the extinctions. The remainder of this essay is divided into five parts: a review of the book, philosophy of science as applied in the book to the K-T program, professional opinion concerning the K-T program during late 1996, the historical background of the paleontologists' view of K-T extinctions, and possible incompleteness of the K-T program to date. The bibliography in Officer and Page[12] is assumed, and otherwise unidentified page numbers in this text refer to their book. Harland et al[4] is here the standard for stratigraphic terminology and absolute ages. This essay emphasizes the K-T research program and perceptions of it, rather than the results of the program. The terms 'geology' and 'geologists' are used in a broad sense encompassing all of earth science.

The epigraph that starts the book is from H. L. Mencken: *For every complex problem there is a solution which is simple, neat, and wrong.* Given the book's title, the "complex problem" of the epigraph is the extinction of dinosaurs, but the book shows that the scientific problem also includes the occurrence of iridium and shocked quartz in fine-grained sediments having K-T age, and the apparently abrupt disappearance or change in abundance and species composition of ammonites, inoceramids, foraminifera, and phytoplankton at the same time. The "dinosaur extinction controversy" of the title is perhaps more accurately called the "impact controversy".

The "simple, neat, and wrong" solution of the epigraph is the idea embodied in the paper[1], "Extraterrestrial cause for the Cretaceous–Tertiary extinction", published in *Science,* 6 June 1980. The authors of that paper include a Nobel Prize-winning physicist, the late Luis Alvarez, his geologist son, Walter, and chemists Frank Asaro and Helen Michel, all associated with the University of California at Berkeley. Their 1980 solution implied that an asteroid about 10 km in diameter impacted the earth's surface. According to their 1980 paper, the dust raised by this impact "effectively prevented sunlight from reaching the surface for a period of several years", suppressing photosynthesis, thus causing the extinctions observed in the paleontological record.

The impact hypothesis raised new physical and paleontological problems. The principal physical problem is, where on the earth's surface did this impact occur? A 10-km asteroid would make a crater 150 to 200-km in diameter[14]. Because of the implied involvement of shocked quartz,[14] the impact site probably should be on a continent rather than on the two thirds[14] of the earth's surface that is ocean. Most of the proposed impact sites have been in North America, and the current candidate is the Chicxulub crater,[15] in and near Yucatan, Mexico, whose qualifications the authors examine critically in Chapter 10. Paleontologic problems discussed in Chapters 3, 4, and 5 include the extinction trajectories of whole classes of life that show significant decline well before the proposed impact, or whole classes of life that swam through the impact relatively unscathed.[16] Another problem is the apparently inadequate physical basis for a worldwide stratospheric dust layer with magnitude and duration sufficient to affect life on earth.

There is an associated attitude problem. All fields of science are not equivalent, and an intuitively reasonable ranking of the sciences will place physics and chemistry above geology and paleontology. Given a few observations of a comet, the physics of Newton can provide, *a priori,* positions of the comet in the past and the future, but given many observations of an existing animal, the biology of Darwin can provide, *a priori,* only an approximate framework on which to hang descriptions of its probable ancestors, and less about its probable descendants. In physics, experiments are repeatable, but in paleontology, experiments are often unknowable. The authors point out (p 12, p 49) that, for 150 years, this implicit ranking has afflicted famous physicists with hubris when considering paleontological questions. When a Nobel Prize and tenure at a well-known university are added to this ranking, it probably was difficult for the 1980 authors to view paleontologists as other than "stamp–collectors" (p 78).

It is consistent with this attitude that the original paper[1] uses the abbreviation, C-T, rather than K-T, for the Cretaceous–Tertiary boundary[17] even though the letter C has long been the accepted abbreviation among geologists and paleontologists for the Carboniferous time interval, which ended 221 million years before K-T time. Over the 18-month period ending four months after Alvarez, et al[1], *Science* published one other article and one report that required abbreviations for the Cretaceous; both[18] appeared with K, not C, as the symbol. Geologists from English-speaking countries have letter symbols for geological time intervals that have a currency approaching that of the letter symbols for elements in the periodic table used by chemists.[19] There are three geologic periods whose names in English begin with the letter 'C': Cambrian, Carboniferous, and Cretaceous. It is relatively rare that a local geologic map shows rocks of all three ages because there is about a half billion years between the start of the Cambrian and the end of the Cretaceous. The first Folio of the U.S. Geological Survey does show all three[19], and they are mapped even at that date (1893) with the symbols used today: Ꞓ, C, and K.

The book is a clearly-written, loosely-organized argument, but not a polemic. It presents conventional science accurately, science history somewhat uncertainly, philosophy of science inaccurately, and current scientific opinion wishfully. There are eleven numbered chapters, plus a Preface and Afterword that are not essentially different from the chapters. Most of these thirteen sections could be read separately or in random order as self-contained units. The book is appropriately illustrated, concludes with a contemporary bibliography keyed to the separate sections, and has an inadequate index.

Publication date is June 1996. Of 223 entries in the bibliography (including duplications), almost all are since 1970, and none is more recent than 1994. The impacts of comet Shoemaker–Levy on Jupiter in July 1994 receive slight mention (none in the index), primarily in one paragraph on page 22 referenced only to the 26 July 1994 issue of the New York Times in the bibliography. The bibliography contains one 19th century reference,[20] and the next oldest is a 1936 publication of the American Association of Petroleum Geologists. The authors do document recent publications and the ephemeral scene, and they have avoided loading the text with ritual mention of older work.

Here is a thumbnail sketch of the 13 sections. The Preface identifies several newsworthy aspects of the impact controversy that relate it to popular culture. Chapter One (The Day of the Meteorite) reviews public reaction to the 1980 paper, physicists' excursions into geology and paleontology, the pecking order among sciences, an outline of dinosaur studies, and the geologic time scale. Chapter Two (Meteorites and Comets) defines comets, meteors, and meteorites, gives an eclectic history of their study, and discusses the probability of a large meteorite impact on an American city. Chapter Three (A Brief History of Dinosaurdom) is aptly named. Chapter Four (Paleonecrology) is not aptly named, being mostly a history of geologic dating, although it shows the continuous decline of ammonites prior to the end of the Cretaceous and the stratigraphic ranges of ammonites in upper Cretaceous rocks of France. Chapter Five (Early Dissent) discusses the eruptions of Krakatoa, Tambora, and Toba, paleontology of dinosaurs, invertebrate extinctions in Antarctica, foram work by Keller, phytoplankton, sea level, volcanism, and selected quotations. Chapter Six (Science and Politics) reports gossip, the effect of the impact idea on academic promotions, extension of the impact idea to nuclear winter and antinuclear causes, and its consistency with punctuated equilibrium. Chapter Seven (Media Science) outlines publication practices of scientists, describes the polywater delusion, and analyzes reporting by *Science* to support the authors' opinion that *Science* was biased in reporting the impact controversy. Chapter Eight (Iridium and Shocked Quartz) reviews iridium data, compares them with arsenic and antimony concentrations, and describes quartz deformation data. Chapter Nine (The Silly Season) discusses a 26 million year periodicity in extinction, Planet X, multiple impacts, global wildfires, amino acids, and impact triggering of Deccan volcanism. Chapter Ten (The Missing Crater) reviews critically a recent sequence of papers on the impact location (27 of 34 entries in their bibliography for this chapter have dates from 1990 to 1994). Chapter Eleven (What Did Happen) discusses plate tectonics, hot spots, the similarity between geologic events that occurred near the times of the K-T and the Permian-Triassic boundaries, postulated environmental effects of volcanism, and locoweed. The Afterword (Pathologic Science) concerns the philosophy of science, as applied to the impact controversy.

The authors spend most of their book arguing against the impact idea of Alvarez, et al[1], and relatively little space promoting their own explanation. In fact, it is hard to extract a concise statement of the authors' theory from their book, but the following abstract from an earlier paper[21] that has Officer as first author seems a fair summary: "The various geological signatures at Cretaceous/Tertiary time including iridium and other associated elements, microspherules, and shock deformation features are compatible with the suggestion that the transition is marked by a period of intense volcanism. The volatile emissions from this volcanism would lead to acid rain, reduction in the alkalinity and pH of the surface ocean, global atmospheric temperature changes, and ozone layer depletion. These environmental effects coupled with those related to the major sea level regression of the late Cretaceous provide the framework for an explanation of the selective nature of the observed extinction record."

In the opinion of Officer and Page, the Alvarez impact was a solution to a problem that did not exist, because pre-existing extinction trends, changes in sea level and climate, disease, and volcanism were adequate processes to account for the K-T events. They particularly favor the environmental effects induced by intense volcanism. Their volcanos were within the Deccan Traps of India and to a lesser extent along the Laramide magmatic trend of North America and on the Walvis Ridge of the south Atlantic (pp 165–168). They concede a large K-T extinction, but claim that the most newsworthy animals to go extinct had already been on a downward trajectory toward extinction before the end of the Cretaceous, and that the 50% species extinction of K-T time (p 8, p 168) was significantly less than the approximately 70% to 80% species extinction at the end of Permian time (p 168) when impacts did not occur.[22] Officer and Page discuss their volcano idea only briefly, mainly in part of Chapter Five (pages 73–76) and especially in Chapter Eleven (pages 158–177), but many of those pages describe conventional plate tectonics, even though their argument depends only secondarily on plate tectonics.

The authors credit (p 75) the early development of their favored idea to McLean[9] and Keith[10], the first being a review article without a figure or table, and the second being a one-paragraph abstract. McLean concludes that the late Cretaceous climate was cooling when the temperature trend changed to produce a brief greenhouse warming at the end of the period. He suggests that the large dinosaurs could not adopt to the sudden warming. Moreover, the warming indirectly made eggshells thinner and, as a separate disaster, induced sterility, all contributing to the extinctions. McLean's paleontologic and chemical assumptions were quickly questioned.[23] Keith[10] is an abstract of a presentation for the spring 1980 meeting of the American Geophysical Union, held a few days before Alvarez, et al[1, 24] was published. Keith[10] identifies damage from increased ultraviolet radiation as the cause of extinction. The ultraviolet hazard resulted from an increase of chlorine in the stratosphere because of increased volcanic activity. The resulting increase in ultraviolet light killed the remaining bareskinned dinosaurs, but did not harm feathered birds and furred mammals. Feathers and fur may have been adaptations to the increased ultraviolet light, as well as to climate, in this view.

The authors suggest (p 171) that during peak volcanism around K-T time, chlorine emissions were about 110 times greater than present values, causing an 8% reduction in the ozone layer[25]. Sulfur dioxide emission was fourteen times what arises from present-day fossil fuel emissions (p 171). They leave open whether the CO_2 increase from volcanism caused a greenhouse warming or the accompanying sulfur dioxide aerosols caused a cooling (p 172). (Thus, the authors believe that their environmental theory works whether the climate warms or cools.) Either way, the Cl and SO_2 from the volcanism did lower the pH of surface waters of the ocean, which they say fits the observed extinctions of planktonic forams (they had carbonate tests) and survival of planktonic dinoflagellates (they had cellulose reinforcement).

Philosophy of Science. The authors argue their case in this book with scientific and non-scientific evidence, and they conclude with an Afterword that introduces a new line of evidence, philosophy of science, combining both. Lawyers, whose business it is to argue cases, are cautious about introducing new lines of evidence because they know that new evidence can expose their own argument to new attack. The authors attempt to capture the high ground of Philosophy of Science in the Afterword, but they should have waited until the fog around that high ground had lifted. Lakatos[26] is their choice for a philosopher guide, a good choice where geological convention would select Popper[27], but they selectively ignore Lakatos' directions. They quote (pp 179–182) extensive passages from Lakatos[28], italicizing those sentences that they consider important: all theories at any stage of development have problems, good theories form a progressive research program that predicts hitherto unknown facts, and bad theories form a degenerating research program that fabricates theories only to accommodate known facts. By these author-selected criteria, the choice still does not favor their idea.

To support their opinion, the authors disregard the sequence of published findings when they say (p 183) "Both discoveries—iridium and PDFs [shocked quartz]—fall under the category of novel and undreamed-of findings that contradict common wisdom and both were the result of the volcanic hypothesis". This sentence follows two paragraphs wherein the volcano theory is assumed, requiring the iridium layer and shocked quartz to be the result of volcanoes. By any reconstruction of recent history (including the authors' own statements on pages 110, 124 and 125), it was the impact theory that made both the iridium layer and shocked quartz grains relevant to K-T research and brought both to wide notice. The volcano theory reacted, after the fact, to these findings.

If philosophy of science is now in evidence, the Mencken epigraph identifies another advantage that the impact theory holds over the volcano theory: the impact theory is simple and neat. Mencken states, and the authors imply, that simple and neat is the mark of a bad theory, but Popper[29] suggests that a simple theory is a better theory because it is the more easily falsifiable—Ockham's razor imbedded in Popper's logic. Compared to the impact theory, the authors' volcano theory is harder to pin down. The volcano theory is indifferent to heating or cooling of the climate, and has many variables linked by uncertain cause and effect relations. The authors invoke sea level change as a cause of extinction, an idea that goes back at least to the beginning of the nineteenth century[7]. Old ideas are not necessarily worn-out ideas, but exactly how does that happen? By far, the best documented sea level and climate changes are those of the Pleistocene, but it remains unclear why extinctions occurred then.[30] How are the proper sea level changes related to volcanism? The authors mention three conjectures (p 170). In Popper's sense, the volcano theory has less empirical content, which makes it less easily falsifiable, and thus the poorer theory[31].

In Lakatos' sense, the impact theory prevails because it has not been replaced by a better theory. Lakatos goes beyond Popper in allowing for the existence of theories that were already falsified in the after-the-fact judgement of history. Included in the passages that the authors quote from Lakatos (p 181), but not italicized by the authors, is this statement: "there is no refutation without a better theory." In his work, Lakatos repeatedly emphasizes this point, paraphrasing it in many ways, with or without his own italics. In the same source[26] used by the authors, Lakatos' statement, or its equivalent, reappears on his pages 34, 35 (in italics), 36, and 37 (twice), and in other places. The paper of Alvarez et al[1] is chock full of new analyses, and it presents a new viewpoint. To replace Alvarez et al, the authors must propose a 'better' theory, but they present few new data characteristic of a progressive research program in Lakatos' sense. On the evidence of their book, the volcano theory is a collection of diffuse ideas that relies on speculative review work and hypotheses that have not been found adequate in the past. The authors introduce Lakatos to support their argument, but inadvertently they offer an example to support Lakatos' ideas on how science works.

The authors then go on to misapply the characteristics of 'pathological science', a term Irving Langmuir used to categorize cases of self-deception in physics. R. N. Hall transcribed Langmuir's 1953 talk on this subject, and it appeared in edited form in *Physics Today*[32], October 1989, at a time when cold fusion was news. The authors identify (pp 184–6) six characteristics of pathological science that, word-for-word, appeared in a brief note in *Scientific American,* September 1989, attributed to "T.M.B."[33], but the authors refer these six to Rousseau[34] who actually gave only three characteristics, one of them being his own. Even more peculiar, the T.M.B. list of September 1989 seems to be an edited version of six characteristics that appeared one month afterwards in a box in the October 1989 *Physics Today*[32] article. Whatever their origin, most of the six characteristics do not fit the impact controversy. To show this, I list the authors' (T.M.B.) version of Langmuir's six characteristics of pathological science (in italics) and my comments on their applicability:

The maximum effect is produced by a causative agent of barely detectable intensity. If it existed, an asteroid 10 km in diameter is certainly a detectable causative agent.

The effect is of a magnitude that remains close to the limit of detectability. If dinosaur extinction and an iridium anomaly are the effects, both are within the limits of detectability, although the time of extinction is always subject to amendment if later fossils are found.

The investigators claim great accuracy of measure-

ment. The accuracy and precision in measuring iridium concentrations claimed in 1980 have been justified well enough by subsequent work, including by analyses of Crocket[35] and others used to support the authors' ideas. The accuracy is sufficient to identify iridium anomalies.

Fantastic theories that contradict experience are presented. Given the authors' definition of experience (no one has yet been killed by a meteor), the impact theory may fit this characteristic of pathological science.

Criticisms are met with ad hoc explanations presented on the spur of the moment. If geological latitude is allowed to "spur of the moment", this seems correct, but Lakatos emphasizes that a ruling theory invoking ad hoc explanations is not replaced until a better theory comes along[36].

The ratio of supporters to critics rises to somewhere near 50 percent and then falls gradually to oblivion. The authors define impact supporters (let us call them supporters in the first sense) as those who believe that the impact caused the K-T extinctions, but the existence of a large K-T impact can be favored (by supporters in the second sense) without believing that impact caused extinction. At least in the US, the sum of impact supporters in the first and second senses account for far more than 50 percent of critical geologists[33], and there is no indication that the proportion is falling. Impact supporters in the first sense may number around the 50 percent of Langmuir, but there are almost no supporters for volcanos as the primary cause of extinction.

Opinion Polls. This last criterion of pathological science requires an opinion poll to verify. How widely accepted is the impact theory? The answer to this question may depend on the audience to whom it is addressed. The authors' statements imply three audiences: the public at large, the non-specialist scientists, and the specialists. The authors concede that the public accepts the impact theory, but they claim that some part of the scientific audiences rejects it. The authors' opinion is best represented by the following quotation: "The Alvarezes' original assertion still reigns popularly as largely unchallenged, in spite of the fact that it does not stand up to any of the actual scientific scrutiny to which it has been subjected" (p. 123). There are a number of similar statements from start to finish in their book, such as "the impact theory has not stood up at all to expert scrutiny" (p xiii), and "The Alvarez hypothesis has collapsed under the weight of accumulated geologic and other evidence to the contrary, as well as from an increasingly obvious absence of scientific evidence proffered in its support." (p 178).

These opinions[37] seem to diverge from those actually held by a significant fraction of geologists in 1996. For example, they diverge from opinions evident at the 1996 Annual Meeting of the Geological Society of America (GSA). The evidence from that meeting[15] suggests that the impact controversy is not a controversy any more in the minds of most of those working on the K-T research program. Impact is the prevailing and the ruling idea, and the open questions now concern such details as how large was the Chicxulub crater. There was even a bandwagon fervor to GSA publications, which featured at least six 'impact' books at that meeting. The few dissenters at the GSA meeting proposed their ideas as oblique statements on the subject.

The authors identify opinion polls taken in 1984 (p 102) and 1985 (p 77) concerning the validity of the impact idea that suggest a substantial proportion of doubters existed then, with significant differences in opinion among scientists from different specialties or continents[38]. If so, a sequel to those opinion polls might serve as a test of the last of Langmuir's six characteristics of pathological science, as well as historical purposes. A questionnaire[39] was prepared and distributed to people attending meetings of four groups concerned with geology (broadly construed) in the Washington, D.C., area: the Geological Society of Washington (est. 1893)[40], the weekly seminar (est. before 1950) of the Geophysical Laboratory of the Carnegie Institute of Washington, the Paleontological Society of Washington (est. 1933), and the Potomac Geophysical Society (est. 1973). These groups include geologists from the National Headquarters of the US Geological Survey, the US National Museum of Natural History, the Carnegie Institution of Washington, seismological businesses, and three local universities.

At four meetings of these local groups in November and December 1996, a total of 72 questionnaires were returned with answers from a potential 159 respondents (45% participation). The questionnaire[39] has fourteen mostly multiple-choice questions and a fifteenth optional essay-type question. The 72 respondents answered most applicable questions. This essay examines opinions evident in the answers to the first six multiple-choice questions. In the summaries that follow, numbers indicate single-answer responses, that is, answers where the respondent selected only one of the options offered. Numbers do not add up to 72 because they omit multiple answers, and some respondents did not answer some questions. Respondents giving multiple answers were never more than six, and usually less than three, for any given question. The six questions and their options, in italics, and the number of respondents selecting single options follow:

1. *What is the present status of the impact controversy? Already settled, Still an open question,* or *No opinion.* The distribution of opinions: 20, 49, 2, respectively.

2. *Was there a geologically significant impact, or series of impacts, by a large extraterrestrial object, or objects, on the earth's surface at the end of Cretaceous time? Yes, No,* or *No opinion.* The distribution of opinions: 66, 1, 5, respectively.

3. *Did the impact(s) cause a worldwide iridium layer with the age of the K-T boundary? Yes, No but iridium layer does exist, There is no worldwide iridium*

layer, or *No opinion.* The distribution of opinions: 57, 6, 3, 6, respectively.

4. *Did the impact(s) cause the extinction of dinosaurs? Yes, No,* or *No opinion.* The distribution of opinions that selected one answer only: 37, 11, 18, respectively.

5. *The iridium layer was caused by Impact(s), Vulcanism, Other* ______________, or *Does not exist.* The distribution of single-answer opinions: 59, 1, 5, 0, respectively.

6. *Dinosaurs went extinct because of Impact(s), Vulcanism,* or *Other* ______________. The distribution of single-answer opinions: 27, 2, 30, respectively.

Of these six questions, Questions Five and Six explicitly list volcanism, the authors' theory, as a possible answer, meaning that there were 144 opportunities to choose volcanism explicitly (2 questions × 72 responses), but the respondents selected volcanism only three times (3/144) as a single answer. This outcome strongly suggests that the authors' volcano theory has essentially no constituency among an important subset of the professional geological population.

However, the respondents did partially support two of the authors' opinions: the controversy is not closed, and impacts did not kill off dinosaurs. Of 69 respondents who expressed an opinion on Question One, 20 thought the controversy was already settled and 49 thought it was still open to debate. Possibly, the wording of Question One and the title of the questionnaire, "Opinion on Impact Controversy", and the existence of the authors' book influenced some respondents to favor the 'still open' answer, when they otherwise might have chosen 'already settled'. But four respondents (all of whom selected 'already settled' in Question One) commented (in Question 15) that, as far as they were concerned, the controversy was settled, although they acknowledged one or the other reason to think that it still continued for others. Even with some bias introduced by the title of the questionnaire, the observed 20:49 ratio of opinions is one-sided enough to suggest that a majority of professional geologists in the sampled population did partially agree with the authors that some aspect of the impact controversy remains open.

However, in the minds of those responding, there was no dispute that a K-T impact occurred, and that it caused the iridium layer: of 67 who expressed an opinion on Question Two, 66 said that there was a significant K-T impact; of 66 who expressed an opinion on Question Three, 57 said that the impact caused a worldwide K-T iridium layer, and of 65 who selected a single answer to Question Five, 59 said that impacts rather than volcanism or some unspecified other cause produced the iridium layer. (Questions Three and Five ask essentially the same question, and get closely similar responses.) An overwhelming majority of respondents clearly believed that there was a significant K-T impact, and that it caused a worldwide iridium layer.

The responses to Questions Four and Six concerning dinosaur extinction are less one-sided, and less consistent. Probably this is what the respondents had in mind when the majority indicated under Question One that the controversy is still open. In Question Four, of 66 respondents providing a single answer, 37 said impacts caused the extinction of dinosaurs, 11 said they did not, and 18 had no opinion (by far the most 'no opinions' received for any question). Question Six offers impacts, volcanism, and other (unspecified) causes as three choices for dinosaur extinctions. Only 59 of the 72 respondents gave a single answer, and among the 59, 'other' led 'impacts' by 30 to 27, with 2 votes for volcanoes. The 13 respondents who did not choose 'other' or 'impacts' or 'volcano' as a single answer include 7 who did not answer this question, and 6 who selected more than one answer, the most 'no-answer' and 'multiple-answer' responses to any question.

This analysis of the first six questions clearly shows that, in late 1996, among the Washington, D.C., area population of responding geologists, almost everyone was an impact supporter in the first or second sense. On the other hand, there were mixed and partly inconsistent opinions on the cause of dinosaur extinction. This uncertainty is further emphasized by the 33% (24/72) who listed 'no opinion', or selected more than one answer, or did not answer Question Four, and the 18% (13/72) who either did not answer, or selected more than one answer for Question Six. Considering the differing responses to Questions Four and Six, and comments as written as answers to Question 15, it is reasonable to conclude that approximately half of the respondents were also impact supporters in the first sense, i.e., approximately half believe that the impact caused dinosaur extinction.

If the respondents as a whole were somewhat uncertain concerning the causes of dinosaur extinction (Questions Four and Six), the 12 paleontologists[41] among the 72 respondents were not. Question Four asks whether the impact caused extinction of the dinosaurs. Of the 66 respondents giving a single answer, 11 said 'no'. A higher fraction of paleontologists (4/10) said 'no' than non-paleontologists (7/56). Of 18 who answered 'no opinion' to Question Four, all were non-paleontologists (0/10 single-answer paleontologists, but 18/56 single-answer non-paleontologists had 'no opinion').

Of 59 single-answer responses to Question Six, a much larger fraction of non-paleontologists (24/48) than of paleontologists (3/11) blamed the impact for dinosaur extinction. In addition to these 59 single-answer responses, there were 7 who did not answer Question Six. All 7 were non-paleontologists. On a single-answer plus no-answer basis, 0/11 paleontologists and 7/55 non-paleontologists did not answer.

Because Questions Four and Six more closely concern their fields of expertise, it is expected that paleontologists would have more certain opinions on the subject, and that is what the data show (1 no-answer, 0 no-opinions). In exercising their expertise, the paleontologists differed from the non-paleontologists:

paleontologists were less likely to believe that an impact caused dinosaur extinction. However, for questions outside their expertise (Questions One, Two, Three, Five on the existence of the impact and the cause of the iridium layer), the paleontologists' opinion was somewhat more pro-impact than the non-paleontologists. Refer to the options in the italicized questions above: the distributions of responses to Question One are (5, 7, 0) for paleontologists and (15, 42, 2) for non-paleontologists; to Question Two, they are (12, 0 ,0) and (54,1,5); to Question Three, they are (11, 1, 0, 0) and (46, 5, 3, 6); and to Question Five they are (10, 0, 0, 0) and (49, 1, 5, 0). In all four sets of responses, the numbers imply a somewhat greater 'pro-impact' opinion for the paleontologists than for the non-paleontologists. Because the average opinion of the responding population was already overwhelmingly pro-impact, the pro-impact deviation shown by the paleontologists could not be much larger. The small number of paleontologists makes any comparison uncertain, but the agreement in trends from responses to all four non-paleo questions lends more probability to the conclusion that the paleontologists tend to be somewhat more pro-impact than the non-paleontologist respondents on topics outside of paleontology.

These opinions on impacts and extinction are consistent with those found by Hoffman and Nitecki[42], who sampled an earlier stage (mid 1984) in the evolution of the controversy. They found that subscribers to *Paleobiology,* compared to North American geophysicists, were significantly less likely (in a ratio of 16:31) to agree that a K-T impact caused mass extinction, but significantly more likely (39:15) to agree that there was a K-T impact but other factors caused mass extinction. It was also consistent that the paleobiologists were less likely (6:13) to doubt a K-T impact.

Paleontologists. Many commentators on the K-T research program have noted that a substantial fraction of paleontologists are reluctant to concur in the extinction aspects of the impact theory.[43] Some supporters in the first sense of the impact theory, who include paleontologists, have treated the reluctant paleontologists harshly.[44] Yet the opinion poll data presented above and in Hoffman and Nitecki[38] suggest that paleontologists are perhaps even more willing than average to accept the existence of the K-T impact and its causal relation to the iridium layer. That is, the reluctant paleontologists are at least as likely to be impact supporters in the second sense as the average geologist. This section examines the background to the opinion of the reluctant paleontologists.

At least two explanations of the paleontologists' reluctance occur to impact supporters in the first sense: (1) the reluctant paleontologists are reacting unprofessionally to an invasion of their turf by the physicists and chemists represented by Alvarez et al[1]; or (2) the reluctant paleontologists are reactionaries trained in the old school who are unwilling to give up old beliefs. Let us provide the benefit of doubt for (1) and examine what in the old school training of (2) may inspire a cautious approach in some paleontologists. Because popular text books are repositories of conventional wisdom at the time they were published, this examination includes twentieth-century texts and reports that might have been familiar to these paleontologists and their teachers.

K-T extinctions, impacts, and to some extent, volcanism, are all supposed to have occurred over a geologically short time, making the exact (independent) determination of a synchronous K-T boundary important, if theories are to be evaluated. By definition, the exact K-T boundary is a synchronous surface separating older Cretaceous from younger Tertiary rocks. The naive reader expects radiometric dating to 'scientifically' identify which rocks are K and which are T, thus determining the synchronous surface, but radiometric dating does not have the accuracy to do that.[4] Some acceptable radiometric Tertiary ages are older than Tertiary and some acceptable radiometric Cretaceous ages are younger than Cretaceous, which ultimately puts the task of locating the K-T boundary back on paleontologists and biostratigraphers.

To test a theory about K-T extinctions, it is necessary to establish *both* the K-T boundary and the range of the species *independently* of that theory, yet, in present practice, the occurrence of the iridium layer seems to help define the K-T boundary, and fossil species have been assumed to range to the end of the time interval in which they are found.[45] The practice is consistent with earlier non-impact use of the fossil record.[46] This exercise in circular reasoning has been exposed repeatedly by geologists and paleontologists over the last 150 years for confusing homotaxis (Huxley's term) with synchrony. Homotaxis is similarity in sequence[47], which can be demonstrated by ordinary biostratigraphic techniques, and synchrony is identity of date, which cannot. Independently of any theory, at many outcrops, the definition of K-T rocks can be inaccurate merely because the K-T boundary depends on available fossils. The last appearance of a key Cretaceous fossil is almost certainly never synchronous at all outcrops[48], so the mapped K-T boundary derived from that fossil may vary in absolute age from region to region, or even from outcrop to outcrop. Before 1980, this time variation was less important, but the impact theory requires that an event happened on one day, so it matters in evaluating K-T impacts. Sometimes, a particular species defined to be Cretaceous actually survived into the Tertiary, or a species defined to be Tertiary actually originated during the Cretaceous.[48] A K-T boundary defined on the last appearance of the older or the first appearance of the younger of these two species is also the erroneous product of a circular definition.

If dinosaurs and ammonites are defined to be Cretaceous, and the impact caused their extinction, as supposed by impact supporters in the first sense, then the last day of Cretaceous time probably post-dated the impact. Thus, the K-T boundary would post-date the impact-induced sedimentation. Where non-impact sedimentation was continuous, the rock deposited on the last day of Cretaceous time should not look different from the rock deposited on the first day of Tertiary time, so here the K-T boundary will not be a visible feature in the rock. However, in almost all environments, sedimentation is intermittent at most. The time during which sediment is actually being deposited at a given location is a small fraction of elapsed time[49]. Thus, it is probable that, in most places, the time of the exact K-T boundary surface occurred during an interval of non-deposition. The duration of non-deposition, which includes time of erosion, is a hiatus that is potentially long compared to the duration of the proposed K-T extinctions, volcanism, and impacts. There is, in fact, no practical upper limit on the potential duration of this hiatus.[50]

Very long hiatuses are usually recognized from field evidence, but shorter hiatuses that are still long with respect to K-T time can be hard to identify, especially if the sedimentary environment before and after the hiatus remained the same. The K-T events may have taken only a small fraction of the hiatus. The longer the unappreciated hiatus, that is, the greater the absolute age difference between rocks in contact across the true K-T boundary, the more likely will evolutionary change in fossil species across the boundary appear to be a sudden extinction.

To the early geologists, the first-order distinction of the Cretaceous at the K-T boundary was the *creta,* the chalk. This was a rock-type distinction, not a fossil distinction, in their eyes. But the secondary characteristic of the Chalk is its fossils, and they are marine. The London and Paris marine Chalk is overlain by brackish marine and non-marine sediments.[5] Thus, whether or not there was a K-T extinction, non-marine rocks above the K-T boundary would differ in fossil species from those in the Chalk below because different environments produced the rocks on either side of the K-T boundary, and those different environments could not support the same life. Similar environmental changes are implied at many other localities that have K-T rocks.

The reluctant paleontologists may also have in mind the uneven level of effort characteristic of paleontology, resulting from accidents of geographic location, paleontologist's talent, rock exposure, and patronage. Until recently, North American paleontologists could not reach much of the Asian continent, but they have studied intensively the Cretaceous rocks of the western interior basin of North America for over a century. Recent unusual dinosaur discoveries have come from China and Argentina in areas previously little explored. Productive, ambitious paleontologists with access to well-preserved fossils can produce "monographic bursts"[51] that change apparent rates of speciation and extinction in particular taxa. If they increase the intensity of their search as the K-T boundary is neared, even species that are decreasing in abundance will be discovered near the boundary.

Paleontologists study only what they can sample. There is a high correlation ($r = 0.94$) between the number of vertebrate genera known from a given geologic period and the number of fossil localities sampled in that period.[52] The increased interest in dinosaurs has produced a burst of new finds in recent years. There were 265 dinosaur genera in 1987, of which forty percent had been described within the previous 18 years.[53] Yet dinosaur studies are still considered at an early stage:

> In comparison with the number of species of vertebrates known to be alive today (in 1969, Mayr estimated that there are 8,600 extant species of birds, 3,700 species of mammals, 6,300 species of reptiles, and 2,500 species of amphibians), the number of dinosaur species known for their 140 million year history is insignificant. Although most dinosaurs were relatively large animals that were probably long-lived, there could have been thousands of species alive at any one time throughout the world, and the discovery of new forms. . . is inevitably going to continue.[53]

Paleocene rocks, which include the lowest Tertiary at the K-T boundary, have limited exposure in Europe, but they are present in thick, mostly buried units along the western Gulf of Mexico coast. These rocks are known largely through samples from cores or drill cuttings. Foraminifera are small enough to be included whole in great numbers in such samples. Thus, forams are important in petroleum geology, and because of this, they have had wealthy patrons for nearly 80 years, insuring that this group of fossils is comparatively well known[54, 48]. Larger invertebrates contemporary with these forams, but too big to fit within a core barrel, are comparatively less well known.

Some reluctant paleontologists will want proof that extinction is not merely local extermination or ordinary evolution.[55] It is not possible to tell from one outcrop whether the disappearance of a fossil species is due merely to local *extermination,* possibly because of changes in the local paleoenvironment, or to worldwide *extinction.* 'Extinction is forever', as the contemporary bumper sticker states, but extermination can mean that the species continues to evolve elsewhere, and may later re-invade the locality as a new species. In quite recent geologic time, the once-abundant horse was exterminated in the Western Hemisphere: "When I found at La Plata the tooth of a horse embedded with the remains of Mastodon, Megatherium, Toxodon, and other extinct monsters, all of which coexisted with still living shells at a very late geological period, I was filled with astonishment; for seeing that the horse, since its introduction by the Spaniards into South America, has run wild over the whole country and has increased in numbers at an unparalleled rate, I asked myself what could so recently have exterminated the former horse under conditions of life apparently so favorable. . ."[56]. And again: "The horse and the camel and the elephant died out in North America late in Pleistocene times, but they survived in the Old World, and the horse especially returned to flourish here in historic time. These great mammals were merely exterminated here, but their contemporaries, the ground sloth and saber–toothed cats, are extinct."[57] (These statements, written a century apart, make the same distinction between extermination and extinction, although Darwin was not always so consistent.)

The horse that the Spaniards brought back to the pampas was by then a different species of horse than the horse whose tooth Darwin found at La Plata.[56] Evolutionary progression necessarily involves extinction when the paleontologists decide that specific characteristics changed enough to merit the establishment of a new species. Other things being equal, the timing of the taxonomic extinction will be fitted to the end of the geologic time interval during which the change occurred, biasing the record toward increased end-of-period extinction.

The extinction of ammonites is credited to the K-T impact by impact supporters in the first sense, but the reluctant paleontologists have in mind that this extinction very nearly happened two geologic periods (143 million years) earlier without an impact. Ammonites were abundant during each of the three Mesozoic periods: Triassic, Jurassic, and Cretaceous. However, "of more than 2,600 known Triassic ammonoid species, none continued into the Jurassic and the host of later species and genera, quite as numerous as those of the Triassic, were derived from a single surviving small group."[58] "That all post-Triassic ammonites are derivatives of a single Triassic family (Phylloceratidae) is amazing. . ."[59]

With all of the above in mind, and their own field and museum experience to guide them, many paleontologists are reluctant to join impact supporters in the first sense. Logically, there must have been a last day of Cretaceous time, but for many paleontologists, specifying even the last 100,000 years of an event 65 million years ago stretches present knowledge, and the associated accuracy in radiometric dating is not better.

None of the Above. In their book[12], Officer and Page try to persuade the reader of two opinions: (1) the impact theory of Alvarez, et al, is wrong, and (2) their own volcano theory is right. Based on this review, they have not succeeded in either effort.

The naive reader might wonder why no options other than impacts and volcanoes are considered. The book, in agreement with the implicit consensus of the scientific community, does not raise the possibility that 'none of the above' could be the correct answer. As a matter of logic, if the authors had succeeded in their first aim, proving the impact theory to be wrong, it does not follow that they would then have succeeded in their second aim, proving the volcano theory to be right. Other processes might explain K-T phenomena.

To suppose that solutions could not exist outside of impact or volcano theories is to suppose that geologists can prove a negative—there are no other processes acting on the surface of the earth that might produce the observed K-T effects. The existence of *Nature, Science,* and dozens of other journals that weekly and monthly report new discoveries about the earth demonstrate that we are far from proving such a negative.

A viable alternative theory would depend on facts that the impact theory could not assimilate. Given the split opinions on dinosaur extinction presented above in the discussion of Questions Four and Six, and the background knowledge of the Reluctant Paleontologists, it may seem that an alternative theory is more likely to arise from discoveries in paleontology, such as MacLeod and Keller[45] have recently provided. But paleontology, in the court of present scientific opinion, has the burden of proof. This burden is the greater because that same court generally does not recognize the near impossibility of establishing the timing of a worldwide event with a duration on the order of one day that happened 65 million years ago, using evolutionary change that typically requires more than 100,000 years. The K-T impact can be fitted easily within the uncertainty in time, and other data can be reinterpreted.

Here are examples of the difficulty. Paleontologists have occasionally published claims of Tertiary dinosaurs[60], i.e., species that survived K-T times, but if such claims come to be widely accepted, impact supporters in the first sense will interpret those Tertiary dinosaurs as the enfeebled remnant of a much larger group decimated by the K-T impact. Many paleontologists claim that dinosaurs and ammonites were declining in numbers long before K-T times, and thus their extinction must be independent of a K-T impact. The dinosaur decline at the end of the Cretaceous appears to resemble closely the decline in elephants at the end of the Cenozoic.[61] But there will be experts who dispute the taxonomy, timing, and statistical basis of such declines, and claim that the impact caused the final extinction. Some widespread species became extinct gradually, exterminated first in high latitudes and going extinct later in lower latitudes.[62] Such latitudinal extinction will be explained as delayed chemical effects induced by the dust layer from the impact. Thus, fossils are unlikely to falsify the impact theory, in the view of impact supporters in the first sense.

The impact theory has made empirically stronger claims on the physical sedimentary data, so impact supporters believe restricted interpretations of shocked quartz grains and iridium layers. Although quartz is very abundant at the earth's surface, the properties of SiO_2 are still a research subject, and new discoveries are being made[63], partly under the stimulus of the impact controversy. Much remains to be discovered. There are few, if any, relevant studies of deformation at metamorphic temperatures, pressures, and strain rates. The size distribution of naturally shocked grains (percentage of total grains per grain-size interval) is not well known from the literature. If an ordinary process could—even rarely—produce shock lamellae in quartz, the impact theory would be weakened because such lamellae now are believed to be due only to high energy shocks such as asteroid impacts. There have been few documented control studies that looked diligently for shocked grains away from known K-T rocks or impact areas. If shocked grains were found in rocks not connected with impacts, that finding could falsify the theory as understood by impact supporters in the second sense.

Iridium anomalies are the famous evidence for impact theory, but they are small. The Gubbio iridium peak found by Alvarez et al is about 9 parts per billion (ppb), which compares with 514 ppb in C1 chondrites (meteorites) and about 1000 ppb in some sulfide ores.[64] As a measure of the smallness of a ppb unit, one US citizen represents about 4 ppb of the entire US population. Sedimentary beds with iridium anomalies are correlated from site to site largely on physical stratigraphy—homotaxis assumed to imply synchrony. A potential falsifier would be the existence of more than one significant iridium anomaly in a vertical sequence. To show this would require good outcrops and close sampling.

The responses in the opinion poll to Questions Two, Three and Five show overwhelming support for the impact origin of a worldwide iridium layer. Is it reasonable, even conceptually, to retain the possibility that there is an alternative to the impact theory in the second sense? The history of geology suggests that it is quite reasonable. One reason for geologists to study the history of geology is to recognize, on a general level, that prevailing theories held by large fractions of the geologist population have been overturned often. It is proverbial that history repeats herself. Here are two examples of theories that prevailed in their day, but are nearly forgotten now.

Some of the more senior readers of this essay learned geology in the first half of the twentieth century when diastrophism was widely assumed to be the ultimate basis for dividing geologic history. That theory prevailed then, but it has nearly disappeared since. The theory had European roots in the works[65] of Élie de Beaumont, Eduard Suess, Émile Haug, and Hans Stille, and it crossed the Atlantic to become firmly imbedded in twentieth century American teaching. In popular texts of their day, Chamberlin and Salisbury[66] devoted a chapter to each geologic period, including in each chapter a subheading worded like, 'Close of the Period', that discussed the diastrophism ending the period. Schuchert[66] defined six major orogenies and several 'disturbances' that closed eras or periods of geologic time. The theory is implied in the title of Barrell's 1917 report.[49] This pervasive idea was often on the level of something taken for granted.

Diastrophism as a benchmark for geologic history had its critics. The theory was essentially bankrupt after mid-century reviews by Gilluly[67] and Henbest[68], Gilluly being more widely known, but Henbest more relevant to the K-T question. Henbest (an expert on the paleontology of foraminifera) pointed out with detail that paleontology lacked the precision to prove, or disprove, synchronous worldwide diastrophism, and this lack of precision implied circular reasoning in the use of paleontology to support the theory of diastrophic control. Henbest explicitly uses Huxley's argument[47], then 90 years old. More than 40 years after Henbest, a similar argument, applied to the K-T problem, is evident in the title of a recent paper, "No statistical support for sudden (or gradual) extinction of dinosaurs".[69]

In the nineteenth century, a major problem for geologists was explaining how erratic boulders came to be perched on rocky slopes, far from their outcrops of origin. It is generally known that the solution at the start of the century was a violent version of Noah's flood, and that the solution at the end of the century was the slow creep of Agassiz's glaciers.[70] Buckland wrote the authoritative compilation[71] of evidence for the flood in 1823, but he also sponsored Agassiz's visit[70] to introduce the evidence for glaciers to British geologists in 1840. This involved no paradigm shift in 1840, for when Agassiz arrived, Lyell's iceberg theory prevailed, icebergs having displaced the flood in the prior decade.[70] The iceberg theory required boulders to be frozen into ice, the boulder-laden ice broken off to form icebergs as sea level rose, and the boulders floated into place, with imaginative variations to fit specific facts.[72]

Today, glaciers are the self-evident explanation, and indeed around 1840, they briefly appeared that way to Lyell[73], Buckland[74], Darwin[75], and Hitchcock[76], under the direct or indirect influence of Agassiz. Each of these four (who were a minority among English-speaking geologists) reported a sudden appreciation of the glacial theory, and then each withdrew to their former iceberg faith, unable to believe in continental glaciation when out of sight of mountains. In published work, Lyell and Hitchcock questioned glaciation up to the time of their deaths (1875, 1864); Darwin did not abandon his iceberg explanations for boulders on the plains of southern Argentina until about 1860; and Buckland drifted back to his iceberg theory before withdrawing from active work in favor of his ecclesiastic duties.

Most senior geologists of English-speaking countries strongly favored the iceberg theory, in the face of the glacial theory, from about 1835 to about 1855. The iceberg theory required vertical changes in relative sea level of many hundreds of meters in geologically short intervals of time, a process that had never been observed. These icebergs on unobserved seas then held sway over glaciers whose work actually occurred in the Alps. But within 50 years, text book authors[77] found it necessary to remind readers that an iceberg theory once had existed, in order to demonstrate how much of an advance glacial theory represented.

That diastrophism is the deciding process in dividing geologic time and that icebergs are responsible for erratic boulders are only two of many theories that appeared invincible in their day only to disappear with the advance of science. There is no *a priori* guarantee that the K-T impact theory cannot go the same way, and thus it cannot be exempt from scrutiny.

If the K-T impact theory is not exempt from scrutiny, are there examples of potential alternate explanations? A different, ordinary, and possibly correct explanation of some iridium anomalies was recently suggested by Bruns et al,[78] who note guardedly that "Ir concentrations in some slowly accumulating sediments are of the same order of magnitude as some of those reported for the Cretaceous–Tertiary boundary event." Although Officer and Page do not consider an option outside of impacts and volcanoes, and do not mention Bruns, et al[78], which was published one month before their book, they do enlighten the origin of this paper.[78] It is consistent with arguments by Officer and Page in their Chapters Six and Seven that Bruns and two of his coauthors are not Americans (although his work was done in the U.S.); that the only funding acknowledged in their paper was from Prof. Dr. J. Thiede and the Deutsche Forschungsgemeinschaft; and that their brief paper is published in the *Journal of Sedimentary Research* without attempting to publish it in *Science.*[78]

Conclusion. In the late twentieth century, the impact theory[1] drives research into what happened on the surface of the earth about 65 million years ago. As a result, there is now a much better idea of the paleontology of K-T time, and of extinction in general, than there was 20 years ago, and the distributions of iridium and shocked quartz in K-T sediments, unknown and unasked for 20 years ago, are now essential data.

Supporters of the impact theory[1] include *impact supporters in the first sense,* those who believe that a large extraterrestrial object impacted the earth's surface and caused large-scale extinction during K-T time, and *impact supporters in the second sense,* those who believe that there was an impact that created a worldwide diagnostic iridium anomaly, usually associated with shocked quartz, but who are either unsure of its causal relation to extinction or don't believe such a relation exists. Polling among geologists in the Washington, D.C., area suggests that nearly all respondents are impact supporters. Perhaps half of the respondents are impact supporters in the first sense. The available information suggests that only a very small number of geologists support the volcano theory of Officer and Page[12] as the primary cause of K-T events in either first or second senses.

There are compelling paleontologic objections to the impact theory in the first sense, most fundamental of which is the satisfactory demonstration of extinction.[55] These objections will not be accepted as disproof because the brief duration of K-T time postulated by the impact theory is not resolvable either by evolutionary change[47,54,79] or by radiometric analysis[4], and in the prevailing opinion, the burden of disproof is with the paleontologists. There is a fundamental indeterminacy[79] in telling geologic time by fossils, at the precision and accuracy needed to discriminate K-T events. Telling time with fossils requires evolutionary change in a particular environment, but during the brief K-T time interval required by impact theory, time was too short for evolutionary change, but not too short for local environmental change, for example, from fluvial deltaic to littoral environments. Differences in local environment, and thus in sedimentation, are more important than evolution in determining those K-T fossils left for paleontologists to find.[79] This suggests, paradoxically, that the potentially most vulnerable aspect of the impact theory is its supporting sedimentary evidence, which is now the most widely accepted.

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NOTES

1. L. W. ALVAREZ, W. ALVAREZ, F. ASARO, and H.V. MICHEL, "Extraterrestrial cause for the Cretaceous–Tertiary extinction", *Science,* 1980, *208*:1095–108.
2. Three such physics-based research programs in geology that effectively began with this century and continue: dating of rocks by measuring products of radioactive decay (Arthur Holmes, "Radioactivity and geological time", *Bulletin of the National Research Council,* Number 80 (Washington, D.C., National Research Council, National Academy of Sciences, 1931), 124–459); estimating pressures and temperatures at formation of silicate minerals and rocks (N. L. Bowen, *The evolution of the igneous rocks* (New York, Dover reprint 1956 of Princeton University Press, 1928), 334 pp.); and discovering the layered structure of the earth from seismic waves refracted or reflected by the layers (Harold Jeffreys, *The Earth, its origin, history and physical constitution* (Cambridge, Cambridge University Press, 1952, 3rd ed), 392 pp. 1st ed 1924, 6th ed 1976). A history of one organization that contributed to and is essentially coextensive with these three programs is given in Gregory Good, ed., *The earth, the heavens and the Carnegie Institution of Washington* (Washington, D.C., American Geophysical Union, 1994), History of Geophysics, vol 5, 252 pp.
3. The adjective is also written K/T, but typographically this appears to imply an inverted stratigraphic order, Cretaceous overlying younger Tertiary.
4. W. B. HARLAND, R. L. ARMSTRONG, A. V. COX, L. E. CRAIG, A. G. SMITH, and D. G. SMITH, *A geologic time scale 1989* (Cambridge, Cambridge University Press, 1990). 263 pp. In this thorough work, Harland, et al, accept 65.0 million years as the age of the K-T boundary, although their chronogram technique for deriving the number gives 66.0 million years (p 112, p 199). Their acceptable radiometric dates include 15 Danian (Tertiary) dates, of which 7 have ages between 65.4 and 67.1 million years, that is, older than their Tertiary. They also accept 20 Maastrichtian (Cretaceous) dates, of which 4 are between 62.6 and 63.5 million years, that is, younger than their Cretaceous (p 87, column 6). Such uncertainties in radiometric age require that the final boundary be established by fossils, 'Absolute' geologic age determination is as much an art as a science.
5. CUVIER and ALEXANDER BROGNIART, "Essai sur la géographie minéralogique des environs de Paris", *Memoires de la Classe des Sciences Mathématiques et Physiques de l'Institut Imperial de France,* 1811, translated in part in Kirtley L. Mather and Shirley L. Mason, *A Source Book in Geology,* 1400–1900

(Cambridge MA, Harvard University Press, 1939), 702 pp, p. 196–197.

6. Charles Lyell, *Principles of Geology, being an attempt to explain the former changes of the earth's surface, by reference to causes now in operation,* vol III (London, 1833) 398 pp + appendixes and front matter, on p 325. See Cyril Galvin, *Earth Sciences History,* 1993, *12*:70–73, for a review of vol III. Lyell overestimated the duration of K-T time, certainly because of the change in fossil species, but possibly in part because he underestimated the power of fluvial erosion (Lyell, *Principles,* vol III, Chap XXII).
7. Cuvier, *Essay on the Theory of the Earth, translated by Robert Kerr with mineralogical notes by Professor Jameson* (Edinburgh, 1813), republished 1971, Gregg International, Westmead, 265 pp. Cuvier's statement on sudden extinction appears on p. 17 of first and fourth editions. In the 1822 fourth edition, Robert Kerr does not appear on the title page or in Jameson's Preface, although the translation of the first edition appears unchanged in the fourth. I have not seen the second or third eds. Robert Bakewell, *An introduction to geology comprising the elements of the science in its present advanced state and all the recent discoveries; with an outline of the geology of England and Wales,* 3rd ed (London, 1828), 540 pp, on p 305, continues the tradition of Cuvier's floods. A resident of Great Britain living near the coast, i. e., Jameson, would grow up sensitive to the effects of storms at sea. The North Sea storm surges appear in a translation from Deluc, identical in both editions (Note E pp 200–216,1813, and Note F pp 302–317, 1822). David Brewster (?) described a later North Sea storm surge, in "Great inundation in Sweden and at St. Petersburg", *Edinburgh Journal of Science,* 1825, *2*:367–8. The *Memoirs of* (Jameson's) *Wernerian Natural History Society,* published at Edinburgh frequently included tables of meteorological data, as did other journals of the period. Robert Stevenson, "Observations upon the Alveus or General Bed of the German Ocean and British Channel," *Memoirs,* 1818, *2*:464–490, describes what is now called beach erosion and sea level rise.
8. Thomas C. Chamberlin and Rollin D. Salisbury, *A college text-book of geology* (New York, Henry Holt, 1909), 978 pp, on p 771. Raymond C. Moore, *Historical geology* (New York, McGraw-Hill, 1933), 673 pp, on pp 485–487. Moore (p 487) is dramatically explicit on the extinction of ammonites at the end of the Cretaceous: "This seems to be one of the lessons of life — adolescence, adult virility, senility and death. Climax is followed by swift denouement and the play is ended. The fall of the curtain is preceded, on the one hand, by the appearance of overspecialization and exaggeration that produce freaks and caricatures; on the other, by degeneracy that reverts to primitive characters." (Text books are useful sources of conventional wisdom around the time they were published.)
9. Dewey M. McLean, "A Terminal Mesozoic "Greenhouse": Lessons from the Past", *Science,* 1978, *201*:401–406, p 403.
10. M. L. Keith, "Cretaceous volcanism and the disappearance of the dinosaurs", *EOS,* 1980, *61*:401.
11. Mark E. Byrnes, *Politics and Space: Image making by NASA* (Westport, CT, Praeger, 1994), 212 pp, on pp 75, 85, 174. American popular support for space exploration reached a peak in 1965 and revived around the time of the first moon-walk (20 July 1969). In the year preceding Alvarez et al[1] *Science* was full of the Voyager I encounter with Jupiter (*204*; Apr-June 1979) and the Pioneer encounter with Venus (*205*; 6 Jul 79 issue is almost entirely on this subject), and in the 16 Nov 79 issue, *Science* even noticed a movie, *Meteor,* about an asteroid 8 km in diameter on a collision course with Earth (*206*:803–4). The 8-km movie asteroid is within the size range of the K-T asteroid estimated by Alvarez, et al[1]. Results in Alvarez, et al[1] were advertized at two meetings[24] in 1979, and their manuscript was submitted to *Science* in late November 1979, according to Frank Asaro in: W. Peter Trower, ed, *Remembering Alvarez* (Chicago, U of Chicago Press, 1987), 272 pp, on p 242.
12. Charles Officer and Jake Page, *The great dinosaur extinction controversy* (Reading MA, Addison–Wesley, 1996), 209 pp.
13. This journal reviewed a previous collaboration of these authors: Gretchen Luepke, Review of "Tales of the Earth", *Earth Sciences History,* 1994, *13*:197.
14. Richard A. F. Grieve, "Impact cratering on the Earth," *Scientific American,* Apr 1990, *262(4)*:66–73, p 73. Ocean occupies 71% of the earth's surface, or 65% if continental shelves along the ocean margins are subtracted: Richard J. Chorley, Stanley A. Schumm, and David E. Sugden, *Geomorphology* (London, Methuen, 1984), 605 pp, on pp 98–100.
15. On 28 Oct 96 at the Annual Meeting of the Geological Society of America, there was a "Popular Scientific Debate" on "Chicxulub: How did it do it?—The K-T boundary, mass extinction, and the post–Chicxulub era" at which no one disagreed openly with the assumptions that there was a K-T impact, and that it was located at the Chixculub site. Officer and Drake, *Extinction controversy,* pp 200–202, has a good recent bibliography on efforts to locate the impact site.
16. V. L. Sharpton and P. D. Ward, eds., "Global catastrophes in earth history: an interdisciplinary conference on impacts, volcanism, and mass mortality", *Geological Society of America Special Paper,* 1990, *247,* 631 pp. This contains many articles on how different taxa changed immediately before and across K-T time, e.g., pp 417, 425, 433, 445, 457, 509, 519, 531, 549.
17. When I asked two senior paleontologists, one an American and another a Russian, what they thought of this use of C-T for Cretaceous–Tertiary, they at first thought that I misspoke, then that I was mistaken, and then that it was an incomprehensible departure from custom.
18. Andrew H. Knoll, Karl J. Niklas, and Bruce H. Tiffney, "Phanerozoic land-plant diversity in North America", *Science,* 1979, *206*:1400–2, on p 1401, and M. D. Zoback, R. M. Hamilton, A. J. Crone, D. P. Russ, F. A. McKeown, and S. R. Brockman, "Recurrent intraplate tectonism in the New Madrid seismic zone", *Science,* 1980, *209*:971–6, on p 973. The 18-month period includes the time Alvarez et al[1] was under review, as well as the time contemporary articles under review by *Science* advanced to publication.
19. The standardization of geologic nomenclature has long been the concern of the International Geologic Congress, and the letter C for Cretaceous was suggested first (presumably an abbreviation for craie): *Rapports des Commissions Internationales pour l'unification de la Nomenclature,* (Bologne, 1881), 144 pp, on pp 63 and 96. C did not reappear in the English-language reports of the same committee at the 3rd and 4th Congresses in Berlin and London (1885, 1888), and by 1890, the US Geological Survey had adopted K after a review of European and North American practice (J. W. Powell, *Tenth Annual Report of the United States Geological Survey, 1888-'89,* (Washington, 1890), *Part I,* p 71.). Arnold Hague, Joseph Paxson Iddings, and Walter Harvey Weed, *Geological Atlas of the United States, Livingston Folio, Montana, Folio 1* (Washington, U S Geological Survey, 1894). A table and explanation appears inside front and back covers of *Folio 1* over the name of J. W. Powell, Director, which assigns K, C, and Ꞓ their present usage. The maps using these symbols are dated 1893. That this use of K is not internationally universal is evident from Otto H. Walliser, ed., "Global Bio-Events, a Critical Approach", *Lecture Notes in Earth Sciences* (Berlin, Springer, 1986), *8,* 442 p. wherein Americans use K, and Danes (p 391), Germans (p 397), and a French author (p 411), use C/T, and Kauffman, an American, uses C–T for Cen-

omanian-Turonian and K-T for Cretaceous-Tertiary on the same page 322.
20. JOHN PHILLIPS, *Life on earth, its origin and succession* (Cambridge, 1860), 224 pp.
21. CHARLES B. OFFICER, ANTHONY HALLAM, CHARLES L. DRAKE, and JOSEPH D. DEVINE, "Late Cretaceous and paroxysmal Cretaceous/Tertiary extinctions", *Nature,* 1987, *326*:143–149, p 143.
22. DOUGLAS H. ERWIN, *The great Paleozoic crisis, life and death in the Permian* (New York, Columbia University Press, 1993), 327 pp. p 235. This book is not mentioned by Officer and Page.[12]
23. STEFAN GARTNER and JAMES P. MCGUIRK, "Terminal Cretaceous extinction scenario for a catastrophe", *Science,* 1979, *206*:1272–1276, in notes 4 and 5.
24. ANTONI HOFFMAN and MATTHEW H. NITECKI, "Reception of the astroid hypothesis of terminal Cretaceous extinctions," *Geology,* 1985, *13*:884–887, p 884 summarizes the prepublication history of Alvarez et al.[1]
25. OFFICER, HALLAM, DRAKE and DEVINE, "Extinctions", p 146.
26. IMRE LAKATOS, *The methodology of scientific research programmes, Philosophical Papers, Volume I,* John Worrall and Gregory Currie, eds. (Cambridge, Cambridge University Press, 1980), 250 pp.
27. KARL R. POPPER, *The logic of scientific discovery* (London, Routledge, 1959); New York reprint, 1992, 480 pp.
28. LAKATOS, *Methodology,* pp 5, 6, 35, 68.
29. POPPER, *Logic,* pp 140–142.
30. PAUL S. MARTIN and RICHARD G. KLEIN, eds. *Quaternary extinctions: a prehistoric revolution* (Tucson, U of Arizona Press, 1984), 3rd printing, 1995, 892 pp. The many papers in Martin and Klein hardly mention sea level change. David M. Raup, *Extinction: Bad genes or bad luck?* (New York, W. W. Norton, 1991), 210 pp, on p 147 for the non-importance of sea level change and p 150 for the "spotty and idiosyncratic" Pleistocene extinctions.
31. POPPER, *Logic,* p 113.
32. IRVING LANGMUIR, "Pathological science", *Physics Today,* Oct 1989, 36–48. Transcribed and edited by Robert N. Hall.
33. T.M.B., "Chilling out, Shades of Langmuir: a panel suspects cold fusion isn't so", *Scientific American,* Sep 1989, *261,* p 20. T.M.B. presumably is Timothy M. Beardsley, Associate Editor. In the fifth characteristic of T.M.B., the authors replace the word 'with' by 'by', but otherwise there is no difference between T.M.B. and pp 184–6 of Officer and Page. The literal meaning of the sixth characteristic requires that the maximum support rises only to one third of all scientists before falling (supporters/critics = 0.50→supporters/(supporters + critics) = 0.33), but it seems to be interpreted as supporters/(supporters + critics) = 0.50.
34. DENNIS L. ROUSSEAU, "Case studies in pathological science", *American Scientist,* 1992, *80*:54–63, 54
35. J. H. CROCKET, C. B. OFFICER, F. C. WEZEL, and G. D. JOHNSON, "Distribution of noble metals across the Cretaceous/Tertiary boundary at Gubbio, Italy: Iridium variation as a constraint on the duration and nature of the Cretaceous/Tertiary boundary events", *Geology,* 1988, *16,* 77–80.
36. LAKATOS, *Methodology,* p 35.
37. Of course, science cannot, in any way, be done by opinion poll. However, the history of science could be better understood, and the philosophy of science better formulated, were data from opinion polls available.
38. HOFFMAN and NITECKI, "Reception", Tables 1 and 2.
39. A copy of the questionnaire and summary of responses is available from the author.
40. EUGENE C. ROBERTSON, ed, *Centennial History of the Geological Society of Washington, 1893–1993* (Washington, Geological Society of Washington, 1993), 165 pp.
41. Question 10 asked the respondents to circle one of six occupations, including "Other __________". The 12 'paleontologists' included nine who circled 'paleontologist', one who wrote in 'Biologist', one who circled three options including 'paleontologist' and wrote the number 1 next to paleontologist, and one who circled both geologist and paleontologist.
42. HOFFMAN and NITECKI, "Reception", Table 2. The two tables summarize the answers, but the questions are not supplied. Dr. Nitecki kindly supplied a copy of their questions (3 Feb 97). Their questions are not directly comparable to mine, but the intent is similar.
43. NORMAN MCLEOD, "K/T redux", *Paleobiology,* 1996, *22,* pp 311–317, on p 312. Clark R. Chapman, "Snowbird II: Global Catastrophes", *EOS,* 4 Apr 89, 217–8, on p 218. Hoffman and Nitecki, "Reception", p 887. Peter Ward, "The sky isn't falling" (book reviews), *Nature,* 2 Jan 97, *385*:36.
44. ERWIN, *Paleozoic crisis,* p 41. Officer and Page, *Extinction Controversy,* p 77.
45. For recent summary, see Norman MacLeod and Gerta Keller, eds., *Cretaceous–Tertiary Mass Extinctions: Biotic and Environmental Changes* (New York, Norton, 1996), 575 pp, on pp 2, 3.
46. LLOYD G. HENBEST, "Significance of evolutionary explosions for diastrophic division of earth history", *Journal of Paleontology,* 1952, *26*:299–318, pp 302, 304.
47. THOMAS H. HUXLEY, "Geological contemporaneity and persistent types of life", reprint of an 1862 address in *Collected Essays, Volume VIII,* (Appleton, New York, 1894), 388 pp, on pp 272–304.
48. GERTA KELLER, "Extended period of extinctions across the Cretaceous/Tertiary boundary in planktonic foraminifera of continental–shelf sections: Implications for impact and volcanism theories," *Bulletin of Geological Society of America,* 1989, *101*: 1408–1419 on p. 1416.
49. This is one of Darwin's imperfections of the geological record. Charles Darwin, *On the Origin of Species by means of Natural Selection* (London, 1859), 502 pp, pp 289–290. Large gaps in sedimentation were emphasized by Joseph Barrell, "Rhythms and the measurements of geologic time", *Geol. Soc. America Bull,* 1917, *28*: 745–904, on p 796; and were taught to stratigraphers and paleontologists by Carl O. Dunbar and John Rodgers, *Principles of Stratigraphy* (New York, John Wiley, 1957), 356 pp, on pp 128–134.
50. DUNBAR and RODGERS, *Stratigraphy,* pp 125–134.
51. G. ARTHUR COOPER and ALWYN WILLIAMS, "Significance of the stratigraphic distribution of brachiopods", *Journal of Paleontology,* 1952, *26*:326–337, on p 331.
52. JOSEPH T. GREGORY, "Vertebrates in the geologic time scale", *Geological Society of America Special Paper 62,* Arie Poldervaart, ed., 1955, 593–608, p 598.
53. KENNETH CARPENTER and PHILIP J. CURRIE, *Dinosaur Systematics: Approaches and Perspectives* (Cambridge, Cambridge University Press, 1990), 381 pp, on p 309.
54. HENBEST, "Evolutionary explosions", p 304.
55. That a fossil species suddenly went *extinct* at the K-T boundary is an inductive conclusion based on its local *extermination* across K-T boundaries in many sedimentary sections. To determine that this fossil species was exterminated at the K-T boundary in any one of these sedimentary sections, there are at least four requirements: (1) the K-T boundary within the section must be located by evidence that does not depend on the fossil species in question; (2) the extrapolated trend of species evolution and fossil abundance in latest Cretaceous rocks must be such as to carry the species into Tertiary rocks above the K-T boundary,

after allowance for a hiatus (if any) at the K-T boundary; (3) the sedimentary rocks on both sides of the K-T boundary must be similar; and (4) the species must be absent from the Tertiary side of the boundary, after reasonable search in suitable Tertiary rocks. Requirement (3) for similar rocks on both sides of the boundary is a minimum requirement to insure that equally favorable environments existed, before and after K-T time, to grow the species and to preserve its fossils. Because the number of fossils found in sediments favorable for their preservation depends on the number of hours spent looking, both requirement (2) for abundance trends, and requirement (4) showing absence in Tertiary rocks must be the result of an equitable search strategy: equal number of search hours in suitable rocks per unit geologic time. Available reports suggest much more time spent searching Cretaceous rocks immediately adjacent to the K-T boundary, a search strategy that can only bias abundance statistics towards an extinction outcome. A weaker form of extermination would change (3) to allow differences in sediment across the K-T boundary, but that weakens any inductive conclusion on extinction.

56. DARWIN, *Origin,* p 318.
57. DUNBAR and RODGERS, *Stratigraphy,* p 153.
58. MOORE, *Historical Geology,* p 485.
59. R. C. MOORE, "Invertebrates and the geologic time scale", *Geological Society of America Special Paper 62,* Arie Poldervaart, ed., 1955, 547–574, on p 561. W. A. Cobban states that Moore's understanding of the Triassic–Jurassic change remains the present understanding (personal communication, 25 Nov 96).
60. LEIGH M. VAN VALEN, "Paleocene dinosaurs or Cretaceous ungulates in South America", *Evolutionary Monographs,* March 1988, *10.*
61. ROGER J. CUFFEY, "Proboscidean and dinosaur biodiversities—similar declines at the approach of final extinction", in Geological Society of America, *Abstracts with Programs,* (Boulder, 1996), 530+pp, on p 297.
62. WILLIAM J. ZINMEISTER and RODNEY M. FELDMAN, "Late Cretaceous faunal changes in the high southern latitudes: a harbinger of global biotic catastrophe?" in Norman MacLeod and Gerta Keller, *Cretaceous–Tertiary mass extinction,* 319.
63. P. J. HEANEY, C. T. PREWITT, and G. V. GIBBS, ed., "Silica: Physical behavior, geochemistry, and materials applications", *Reviews in Mineralogy,* 1994, *Volume 29,* 606 pp; see particularly Hemley, Prewitt, and Kingma, 41–81. K. J. Kingma, C. Meade, R. J. Hemley, H. K. Mao, and D. R. Veblen, "Microstructural observations of a-quartz amorphization", *Science,* 1993, *259.* 666–669. Alan R. Huffman and W. V. Reimold, "Experimental constraints on shock-induced microstructures in naturally deformed silicates", *Tectonophysics,* 1996, *256,* 165–217, 167.
64. ALVAREZ, et al, "Extraterrestrial cause", Table 1, p. 1100. A. J. Naldrett and J. M. Duke, "Platinum metals in magmatic sulfide ores", *Science,* 1980, *208*:1417–1424, Table 2, p. 1419.
65. GABRIEL GOHAU, *A history of Geology* (New Brunswick, Rutgers University Press, 1991) revised and translated by A. V. and M. Carozzi, 259 pp, on pp 175, 184, 211. A diastrophism holdover is on p 219.
66. CHAMBERLIN and SALISBURY, *Geology,* pp 490, 515–6, 545, 641, 668, etc, show close of Cambrian, Ordovician, Silurian, Pennsylvanian, and Permian, respectively. Charles Schuchert and Carl O Dunbar, *Outlines of historical geology* (New York, Wiley, 1937), 3rd ed, 268 pp, on p 32. The ideas in both these texts recrossed the Atlantic to form the background for the diastrophic "revolutions" in John Joly, *The Surface-History of the Earth* (London, Oxford University Press, 1925), 192 pp, pp 132, 133.
67. JAMES GILLULY, "Distribution of mountain-building in geologic time", *Bulletin of Geological Society of America,* 1949, *60*:561–590. In the years immediately following Gilluly, new texts tried to endorse both sides of the diastrophism question: Jean Goguel, *Tectonics* (San Francisco, Freeman, 1962), English Translation by H. E. Thalmann from the French edition of 1952, 384 pp, on pp 273–277. L. U. de Sitter, *Structural Geology* (New York, McGraw-Hill, 1956), 552 pp, on pp 327–330.
68. HENBEST, "Evolutionary explosions", pp 299, 311. This paper derives from a 1938 conference of the Paleontologic Society of Washington, one of the local Washington, D. C., groups that contributed to the opinion poll discussed in the text. Gilluly gave his paper in 1948, which reawakened interest in the 1938 meeting proceedings, resulting in a 1949 symposium at which Henbest's paper was presented. It was published in 1952 (information from the Foreward by Winifred Goldring that introduces the Henbest paper).
69. STUART H. HURLBERT and J. DAVID ARCHIBALD, "No statistical support for sudden (or gradual) extinction of dinosaurs", *Geology,* 1995, *23*:881–884. Comment by P. M. Sheehan, D. E. Fastovsky, R. G. Hoffman, and C. B. Barreto, and Reply by S. H. Hurlbert and J. D. Archibald, in *Geology,* 1996, *24*:957–959.
70. RICHARD J. CHORLEY, ANTONY J. DUNN, and ROBERT P. BECKINSALE, *The history of the study of landforms, volume one: Geomorphology before Davis* (London, Methuen, 1964), 678 pp, 191–234, 330–339. G. L. Davies, *The earth in decay, a history of British geomorphology 1578 to 1878* (New York, American Elsevier, 1969), 390 pp., 263–316.
71. WILLIAM BUCKLAND, *Reliquiae Diluvianae: Observations on the organic remains contained in caves, fissures, and diluvial gravel, and on other geological phenomena attesting to the action of an universal deluge* (London, John Murray, 1823), 303 pp, 224. "Thus far I have produced a various, and in my judgement, incontrovertible body of facts, to show that the whole earth has been subjected to a recent and universal inundation."
72. LYELL, *Principles III,* 149–150.
73. CHARLES LYELL, "On the geological evidence of the former existence of glaciers in Forfarshire", *Proceedings of the Geological Society of London,* 1840, *III,* part II, n. 72, 337–348. Several writers indicate that Lyell avoided later reference to these opinions of 1840, as if embarrassed by his impetuous embrace of a theory he rejected after reflection. See Note 70 and Edward Bailey, *Charles Lyell* (Garden City, Doubleday, 1963), 214 pp, 141. The incomplete biography by Wilson ends with the 1840 presentation on 'glaciers in Forfarshire'; and a hint of this ambivalence. Leonard G. Wilson, *Charles Lyell, The years to 1841: The revolution in geology* (New Haven, Yale University Press, 1972), 553 pp, 502.
74. WILLIAM BUCKLAND, "Evidences of glaciers in Scotland and the north of England," *Proceedings of the Geological Society of London,* 1840, *III,* part II, n. 72, 332–337.
75. CHARLES DARWIN, letter to W. H. Fitton, c. 28 June 1842, in Frederick Burkhardt and Sydney Smith, ed., *The Correspondence of Charles Darwin, Volume 2* (Cambridge, Cambridge University Press, 1986), 603 pp, 321–322. "Yesterday (and the previous days) I had some most interesting work in examining the marks left by extinct glaciers—I assure you no extinct volcano could hardly leave more evident traces of its activity and vast powers." ['extinct' appears twice in these opening lines of the letter, written soon after CD finished the 'pencil sketch of my species theory' (p. 435)]. "... it convinces me that my views, of the distribution of the boulders on the S. American *plains* having been affected by floating Ice, are correct." [perhaps CD means 'effected'].
76. ROBERT H. SILLIMAN, "Agassiz vs. Lyell: authority in the assessment of the diluvium—drift problem by North American geologists, with particular reference to Edward Hitchcock", *Earth Sciences History,* 1994, *13,* n. 2, 180–186, pp 183, 184.

Edward Hitchcock, "First anniversary address before the Association of American Geologists at their second annual meeting in Philadelphia, April 5, 1841", *American Journal of Science and Arts,* 1841, *vol XLI,* 232–275, p 253 [Hitchcock in footnote says that, since presenting the address, he has read the reports by Buckland and Lyell (notes 74, 73) as well as by Agassiz, and "A flood of light having thus been unexpectedly thrown on my mind, I am free to acknowledge that many of my difficulties in respect to this theory have been removed."]

77. JAMES D. DANA, *Revised Text-Book of Geology,* fifth ed., edited by William North Rice (New York, American Book Co., 1897), 482 pp, 409. Thomas C. Chamberlin and Rollin D. Salisbury, *Geology, Vol III, Earth History* (New York, Holt, 1906), 624 pp, 337.

78. PETER BRUNS, WOLF-CHRISTIAN DULLO, WILLIAM H. HAY, CHRISTOPHER NED WOLD, and ERNST PERNICKA, "Iridium concentration as an indicator of instantaneous sediment accumulation rates", *Journal of Sedimentary Research,* 1996, *66*:608–612. (Dr. Bruns discussed the origin of this paper with me on 25 Feb 97.)

79. W. C. KRUMBEIN and L. L. SLOSS, *Stratigraphy and sedimentation* (San Francisco, Freeman, 1953), 497 pp. Fig. 10–8 on p 305 is a conceptual diagram qualitatively illustrating the basic indeterminacy in using fossils during short time intervals to tell time.

Cyril Galvin, *Coastal Engineer, Box 623, Springfield, Virginia 22150, USA*

BOOK REVIEWS

Gretchen Luepke, Book Review Editor

AUTOPSIE DELLA TERRA. **ILLUMINISMO E GEOLOGIA IN ALBERTO FORTIS (1741-1803).** *Luca Ciancio. 1995. Biblioteca di Nuncius: Studi e Testi XVIII. Leo S. Olschki, Firenze, Italy. 385 p. Softcover, 82,000 Lira.*

The figure of Abbot Alberto Fortis has been known for a long time by the historians of the Italian Enlightenment and late eighteenth-century scientific culture. As a geologist, traveller, journalist, antiquarian and man of letters, Fortis may be considered a real polymath who developed a wide variety of interests from the early 1760s until the year of his death in 1803. He became a prominent character within the active scientific and scholarly milieu that took shape in the Republic of Venice—that is to say, mostly in the Veneto region—during the second half of the eighteenth century. This "repubblica de' naturalisti" (republic of naturalists) has been recently studied by Luca Ciancio in an interesting paper published in 1993 in the periodical *Atti dell'Accademia Roveretana degli Agiati,* where also some useful prosopographic analyses are presented. Now, in this new book suggestively entitled "Autopsies of the Earth," Ciancio not only provides the first detailed and widely documented monograph on Alberto Fortis, but also intends to emphasize the important contribution made by the "scientific tradition of Veneto" (author's definition) to eighteenth-century geological research.

Consequently, in spite of the richness of Fortis's life and bibliography of published and unpublished writings, Ciancio does not adopt in this case the model of the "intellectual biography," but prefers to focus his research on some selected aspects of Fortis's scientific activities. These are considered by the author as tools for investigating—from a particular "Venetian perspective" related to the culture of the Enlightenment—the significant elements of the process of formation of geology as a science during the eighteenth century, through the interaction between natural philosophy, medical-naturalistic disciplines, and historical-antiquarian interests. For this reason the book does not strictly follow a series of biographical stages, but is based on four main chapters devoted to relevant topics: 1) The formation of a scientific style; 2) Theories of the Earth and research in the field; 3) Sciences of nature and "public good"; 4) Geology and reformation of natural history.

Several pages treat the role of significant figures of eighteenth-century geology in Veneto, such as Giovanni Arduino and John Strange, but also recall contemporary research and debates in Europe, for example, those related to the formulation of "theories of the Earth," to the discovery of extinct volcanoes, to the controversial question of rhabdomancy (dowsing), and to the concept of transformation of the species in zoology and paleontology.

Alberto Fortis acquired a specific scientific style during the years of his intellectual formation, also through contact with scientists such as Antonio Vallisneri junior, Giovanni Arduino, Giuseppe Toaldo, and Guido Vio. In particular, the relationship with Arduino was fundamental for the development of Fortis's interests in the geological structures and for the adoption of a research methodology based on accurate fieldwork. On the other hand, Ciancio points out the importance of the influence of both the medical tradition and the antiquarian studies on the development of the style of research undertaken by the geologists in eighteenth-century Veneto. The link between naturalistic research and historical-antiquarian methodology is particularly evident in Fortis, who left theological studies early and developed instead a strong interest for direct "archaeological" analysis of the monumenta of Antiquity. He was encouraged toward this scholarly direction by the important friendship with John Strange, who was the English Consul in Venice from 1774 and also a traveller, geologist, and collector of antiquities. Some pages of Ciancio's book clearly suggest that it is not possible to fully evaluate Fortis's scientific style without considering the role of Strange. In fact, the two scholars show the same kind of "philosophical erudition", as in the case of Fortis's writings on his travels in Dalmatia and to the Islands of Cherso and Osero, where archaeological, antiquarian, economic, anthropological and naturalistic elements are combined. Moreover, Ciancio underlines the link between architectonic visual representation and the early geological illustrations published in Veneto. This analogy is also suggested by Fortis's use of "architectonic concepts for facilitating the description of new natural morphologies" (p. 90).

The second chapter examines the meaning of Fortis's "theory of the Earth" and his significant contribution to the study of the extinct volcanoes undertaken by the Venetian geologists during the 1760s and 1770s. According to Fortis's theory, which was published as a "geological poem" in 1768 and later in 1786, a series of cataclysms—caused by the modification of the inclination of the terrestrial axis—had produced a cycle of destructions and reconstructions on the Earth's surface. Moreover, the results of geological explorations in the field, started in the early 1760s under the guidance of Arduino, showed the evidence of these catastrophical and periodical events, due to an alternation of ancient volcanic phenomena and partial floods. Ciancio (p. 97) rightly points out that Fortis, like Arduino and Strange, did not separate his theory of the Earth from his field work, which also provided the basis for a remarkable extension of the chronology of the geological changes (the idea of "deep time"). However, the field work undertaken by Fortis and by the geologists of Veneto was mainly focused on the study of volcanic phenomena and produced important conclusions on the origin of igneous rocks. Ciancio's excellent discussion of the role of these scientists within the eighteenth-century European debate on the extinct volcanoes, as well as on the origin of basalt and crystalline rocks, has produced some of the best pages of the book (p. 119-166). Fortis, after having recognized with Arduino and Strange several evidences of ancient volcanoes in the Veneto region, stated before 1768 the igneous origin of basalt and at the end of the 1770s claimed the same origin for granite. According to Ciancio it is not correct to simply define the Venetian geologists as "vulcanists," because they also carefully considered the action of water in the lithogenetical and geomorphological processes. On the other hand, their ideas on the igneous origin of basalt and other crystalline rocks were criticized by scholars who referred to the contemporary chemical-mineralogical studies of Wallerius, Lehmann, Cronstedt, and Bergman, well before the diffusion in Italy of the Wernerian neptunistic theory.

In the third chapter, Ciancio investigates with clarity and effectiveness Fortis's role in the debate on rhabdomancy (dowsing), his activities as anti-clerical scholar and journalist, as well as his involvement in supporting economic reforms linked to technical-scientific culture both in the Venetian Republic and in the Kingdom of Naples. According to Ciancio, Fortis was not nominated professor of natural history at the University of Padua because of his anti-clericalism, but he contributed to the diffusion of the ideas of the Enlightenment in the Venetian Republic, together with other scholars such as Griselini, Toaldo, and Arduino.

The paleontological and geological studies of the last years of Fortis's life are finally presented in the fourth chapter, which emphasizes the importance of the long stay in Paris after 1796, where the Italian scientist started to prepare the *Mémoires pour servir à l'histoire naturelle de l'Italie* (1802). In Paris Fortis worked in the Museum of Natural History and met Cuvier, Delamétherie, Faujas de Saint-Fond, and Lamarck. According to Ciancio, Fortis's transformistic ideas in zoology can be often considered similar to those of Lamarck, for example in the case of the importance given to the environmental influences as causes of the changes of the organisms. Fortis's contribution to the paleontological research of the 1790s was also significant, in particular within the controversy on the origin of the fossils of Bolca. On the other hand he reinforced his conclusions on the importance of the volcanic phenomena in Veneto, referred to the theories of James Hutton and James Hall and supported Faujas de Saint-Fond's vulcanistic ideas in particular against the growing diffusion of Wernerian geology.

Autopsie della terra provides a documented reconstruction of the scientific and cultural itinerary of Alberto Fortis: in addition the impressive bibliography of primary sources, it is important to recall the findings of new Fortis's manuscripts in particular at the British Library in London, as in the case of the "oryctological travel" in the Apennines (1775).

Although Ciancio provides many answers on Fortis's place in eighteenth-century science, some general questions arise from the reading of the book. The numerous references to the positions of the "geologists of Veneto" (often too much restricted to the writings of Fortis, Strange and Arduino) suggest the existence of a specific group of scholars within the Venetian "republic of naturalists." A complete definition of this geological community and the detailed analysis of its scientific production will represent a stimulating task for further research, possibly also focused on the so-called "provincial" figures and institutions. Ciancio himself, in the section "Science in province" (p. 231-237), recognizes the question of the distinction between expert naturalists and provincial amateurs in Veneto. Consequently, within this historiographical context of partial knowledge on the formation and the extent of the "geological" community in eighteenth-century Veneto, Ciancio's statement that the anatomical investigation proposed by Giambattista Morgagni was taken as "a suggestive model of research by the geologists" (p. 54) seems to be too strong generalization and would need to be better documented with a wide range of primary sources. It is true, as Ciancio writes (p. 92), that the process which determined the birth of modern geology was extremely complex in the eighteenth century. But for this reason the development of the geological research in Veneto did not follow a single model of research, but was influenced and supported by a combination of elements. These included suggestions from the medical language and from the archaeological visual representation, but also fundamental indications from the mining experience and, especially in the second half of the century, a solid chemical-mineralogical knowledge.

Therefore, within this context it will be important to verify another crucial question: was, as suggested by Ciancio, the lack of centers of professional formation and geo-mineralogical research (such as the mining academies established in Germany and in France) really the main cause which initially shaped the original characters of this community of Venetian geologists and later determined its decline at the end of the eighteenth century? This interpretation is stimulating but perhaps, before drawing a conclusion, it would be necessary to have a comparative analysis of the real impact of the mining academies and similar institutions on the geological research in Europe.

Ciancio's book represents an important step of the recent historiographical trend that has produced new studies on several figures of eighteenth-century science in the Venetian Republic, such as Anton Lazzaro Moro, Giovanni Arduino, Antonio Vallisneri junior, Girolamo Festari, Giuseppe Olivi, Anton Maria Lorgna and others. Alberto Fortis's scientific and geological works cannot be considered of secondary importance any more, because their analysis has opened some significant historiographical problems that will be certainly treated again in further researches on the history of Italian geology.

Ezio Vaccari, Via Marsala 18, Verona VR Italy 37128

MACMILLAN ENCYCLOPEDIA OF EARTH SCIENCES. *E. Julius Dasch, Editor-in Chief. 1996. Simon and Schuster, New York. 2 vols., 1193 p. Hardcover, $165.00.*

The publication of the two-volume Macmillan Encyclopedia of Earth Sciences is a most welcome event. The work is designed to serve the advanced high-school and university-undergraduate student. It will also be valuable to the lay public, which is the primary aim of any good encyclopedia, as well as among members of the working scientific community.

An encyclopedia should not only feature the latest information on the subjects it covers but also focus on their history, because this is where most people will get their introductions to the subjects. Not all encyclopedias live up to this mandate. This one does. The list of contributors is a veritable Who's Who in earth sciences, including many who are active in earth sciences history. The Preface, written by editor-in-chief E. Julius Dasch of NASA, is worth reading for itself.

Among the over 300 articles, there are 61 biographical entrees of individual famous scientists, from astronomers to zoologists. More scientists are discussed in entries such as "The Herschel Family" (written by Nadine Barlow), "Minorities in the Earth Sciences" (Marilyn Suiter), and "Women in the Earth Sciences" (H. Catherine W. Skinner). Nineteen articles specifically discuss the history of their subjects–as in "Paleontology, History of" (Ronald Rainger), and others cover several historical subjects, e.g. "Famous Controversies in Geology" (Anthony Hallam).

In a review of an encyclopedia, only a cursory examination is possible. As a test, I tried looking up subjects both familiar and unfamiliar to me. I found the writing to be consistently clear and well-organized.

Misprints are an unavoidable fact of life in a large undertaking such as this, and the *Encyclopedia of Earth Sciences* is no exception. Bruce Heezen's name, spelled properly on p. 295, is misspelled "Heizen" on p. 296; Ida Ogilvie's name is misspelled "Olgivie" (pp. 1187, 1245). Ursula B. Marvin's name is reversed as "B. M. Ursula" in a bibliographic entry on p. 1189. A most curious entry in the Index (p. 1268) is "Unanium, as heat source, 259". A careful reading of p. 259 shows no misspelling of the element "uranium." All these are details that do not detract from the overall excellence of the writing.

The volumes are beautifully produced. A total of 46 beautiful color plates, almost evenly divided between the two volumes, are collected in their centers–a necessary compromise from having them juxtaposed with the article to which they belong. As a mineralogy enthusiast, I was pleased to see that 12 of these plates are devoted to exquisite mineral specimens. Images of planets taken from the Hubble Space Telescope, *Voyager 2,* and *Viking I Orbiter,* and the first photograph of the Earth from outer space are simply breath-taking.

The book's high cost will no doubt preclude its purchase by many individuals, but it definitely belongs in all school and public libraries. I predict this reference will be used widely well into the coming new century.

Gretchen Luepke, 650 Alamo Court, #6, Mountain View, California 94043; Gluepke@aol.com

GLOBAL CHANGE AND HISTORY OF GEOPHYSICS. *Wilfried Schröder and Michele Colacino, editors. 1996. Interdivisional Commission on History of International Association of Geomagnetism and Aeronomy (IAGA), Bremen-Roennebeck, Germany. 293 p. Softcover, 30 DM, US$20.00.*

This book is a collection of 16 papers selected from the symposium of the International Commission on History of IAGA during the IUGG/IAGA Assembly held in Boulder, Colorado (USA) in 1995. Two papers are in German with English abstracts; the remaining papers are all in English. The subjects of the papers range from a study of ideas and models of volcanoes to viewpoints on archaeoastronomy.

Giovanni P. Gregori and Wenjie Dong, in "The prime mover of volcanoes—History of a Concept," present a detailed spreadsheet of historic volcanic eruptions, supported by extensive comments on the ideas and models of volcanoes through time. References in this paper range in dates from 550 B.C. (Thales) to 1991 A.D. (Maurice Krafft). Another paper—"The historical observations of the geomagnetic field in Italy: the discovery of Father Denza Survey (1875–1883). A Preliminary Report" by L. Cafarella, A. Medoni, Anna De Santis, and M. Basso Ricci—must be in the record class of delayed publication at over a century.

Professor Rhodes W. Fairbridge examines the linkage among planetary, solar and lunar functions, and terrestrial climatic and geophysical phenomena in his paper entitled "Spectra of solar-Terrestrial Proxies (auroras, tree rings, varves, ice cores), and a proposed astrochronology of the last 15,000 years." He finds some vindication for the proposals of early scholars.

E. Piervitali, Michele Colacino, and M. Conte examine the possibility of use of processions of the Virgin to determine the frequency of droughts. Their paper, entitled "Drought in western Sicily in the period of 1565–1915 as seen through religious events," cautions that emotional and religious feelings may arise from situations other than droughts, so quantitative correlations are difficult. The same authors, in another paper entitled "The very cold summers in the century 1811–1910: Volcanic eruptions or blocking systems?" suggest that cold summers in the northeastern United States and Europe may have been caused by blocking of normal circulation by high pressure cells over Baffin Bay and the North Atlantic Ocean. They further postulate the cause to be possibly a result of (you guessed it) El Niño—Southern Oscillation, rather than aerosols released by volcanic eruptions.

In "Viewpoints in archaeoastronomy. An invitation to the archaeology of Earth's science," G. P. Gregori and Lucia G. Gregori consider ancient knowledge of 1) "Seasons or long-range time determination"; 2) "Sundials and calendars," which contains details of the Pantheon in Rome as a possible antiboreum (a type of sundial); 3) "Orientation of sailors and travellers"; 4) Latitude (and Earth's radius) determination"; and 5) "Equinox precession." They conclude that astronomical knowledge in prehistoric times was certainly well developed, and they further speculate on the many uses of this knowledge.

These selected articles give some feeling for the range of topics touched upon in this small (293 p.) book. It is an interesting collection of papers of variable quality in syntax, style, and format (camera-ready), and is suitable for widening the outlook of those interested in science and science history.

Richard E. Warrick, Geophysicist, U.S. Geological Survey (Retired), 345 Middlefield Road, Menlo Park, California 94025

FIELDWORK—A GEOLOGIST'S MEMOIR OF THE KALAHARI. *Christopher Scholz. 1997. Princeton University Press, Princeton, N.J. 190 p. Hardcover, $24.95.*

Geologists often cite the allure of fieldwork as the prime reason for choosing their profession. Being paid to go on safari, rather than paying handsomely for the adventure, is why many geologists consider themselves lucky. In this memoir of a long-ago field season in the African bush, Christopher Scholz has captured the essence of geologic fieldwork, revealing the trials, mishaps, and downright fun of the experience. The story itself is no more extraordinary than many geologists' yarns, but Scholz's insightful and sensitive narrative is exceptional. He explores the link between scientific discovery and adventure, defining his journey as "combining the most exalted and the most basic of human challenges. We had to solve a scientific problem, and, not to put too fine a point on it, we also had to survive."

The story begins in 1973, when Scholz, a newly-tenured professor, is asked by the United Nations to serve as an "earthquake consultant" for a rather undefined mission in Botswana. At first, knowing little about the tectonics of the Kalahari Desert, the author consults the data archives to develop a scientific hypothesis worthy of testing, then he hastily sets off for the field. The basic challenge is to conduct a microearthquake survey in the remote Okavango Delta to understand the source of large tremors in the region. Was the seismicity coming from a newly-forming southern extension of the great East African Rift System? On the way he encounters disinterested bureaucrats in Rome and a skeptical reception from the experienced field men of the Botswana Geological Survey. Just getting to the field area is hard enough, given maps annotated with dubious place names as "Shinamba Hills?" Making the seismometers work in a desert of sound-deadening sand and transistor-popping heat is doubly challenging. The field crew must find the sparse outcrops of solid rock needed for successful recording of microearthquakes. One critical site, the Kgwebe Hills, turns out to be guarded by a double phalanx of forty spear-carrying Bushmen. Sleepless nights result when the field party pitches camp on the waterhole trail used nightly by a herd of elephants in the Chobe Game Park.

We get a notion of working in the African outback of the early 1970's, before satellite systems provided navigation and communication. The field party was able to contact their base by radio, only to find out later that their transmissions were an amusement for an eavesdropping crew of Afrikaner drillers. Apparently, the drill crew had placed bets on how many days this New York professor and his team would last, camped amidst the elephants. We also get taken inside some edge-of-civilization beer pubs, and meet dusty exploration crews, expatriate bush pilots, and the "Groovy Girls." When Scholz takes an excursion to Victoria Falls, he must travel a road near the Rhodesian border, a road that is rumored to harbor land mines left by a guerrilla faction. An unpleasant reality of fieldwork in remote parts of Africa, then and now, is finding oneself quite by accident in war zone.

Scholz offers insight into what is needed for a successful scientific expedition. Regarding leadership: "You can't be very successful as a scientist without a heavy dose of stubbornness, or determination, whatever you want to call it. It's a competitive business in which you must be able to solve problems that others, sometimes many others, have failed to solve, or haven't seen in the first place. To be successful at that game time after time requires more than good scientific skills and a gift of cleverness. It also means having the will to not give up where others might throw in the towel." Another key ingredient for success is having a person who can improvise in the field to repair electronic equipment or Land Rovers, as exemplified by technician and companion Teddy Koczynski. Patience and the ability to win the cooperation of people, whether they are the hired crew, the local police chief, or a tribal leader, are also essential.

At the end of three months in the field, Scholz came home with enough data to publish a paper in support of his rifting hypothesis. In the "Epilogue" he acknowledges that his study in Botswana was not to become a scientific blockbuster, but it did establish that continental "rifts can propagate without the driving force of a mantle plume." Scientific knowlege was the goal, but the quest was the real story of this book.

John Hillhouse, Geologist, U.S. Geological Survey, Menlo Park, CA, 94025

THE TUCSON METEORITES. THEIR HISTORY FROM FRONTIER ARIZONA TO THE SMITHSONIAN. *Richard R. Willey. 1997. The University of Arizona Press, Tucson. 48 p. Softcover, $15.95.*

Originally published by the Smithsonian Institution Press in 1987, *The Tucson Meteorites* has now been reprinted with a minor addition by the University of Arizona Press. Willey is the former director of the University of Arizona Grace H. Flandrau Planetarium. The book is divided into 6 chapters; the book includes a Preface, a "Recent Developments" (the new material in this edition), chapter notes, an appendix of facts and physical characteristics of the Tucson meteorites, and a short index, consisting mostly of names of people involved in the saga. Both historical illustrations and photographs of the meteorite fragments further enhance this book's appearance.

Unlike the fake Port Orford meteorite, the subject of another Smithsonian publication (reviewed in ESH, v. 13, no. 1, p. 77; 1994), the Tucson meteorites are quite real. They consist of two known fragments totaling more than a ton; one fragment has a unique ring-shape and was first used as an anvil at the Presidio of Tucson. Willey traces the fascinating path these meteorites took to get to the Smithsonian Institution, where they reside today in the distinguished Hall of Meteorites. The Ring, unusual is both shape and composition, has been displayed outside the Smithsonian only during the 1876 Centennial Exposition in Philadelphia and in 1976 at the opening of the Grace Flandrau Planetarium in Tucson—which coincided with the bicentennial of both the US and the founding of the presidio of Tucson.

The Santa Rita Mountains, located south of Tucson, are the purported locale of the original Tucson meteorite finds; the discovery of several very small meteoritic fragments in these mountains in 1991 (four years after the book's original publication) is strongly suggestive that this is true. The mystery of the location of the original finds, however, remains to this day.

As a native Tucsonian, I confess to being fascinated with a story heretofore known only vaguely to me. I have only one minor detail for which to quibble with the author. Willey describes the bed of the Santa Cruz River, which runs through Tucson, as "usually dry." It is quite true that the Santa Cruz River today is dry except for flash floods. This was not true at the time of the discovery of the meteorites; although the Santa Cruz was described little more than a creek by travelers in the 1850's, it was nonetheless a perennial stream up until the mid-1880's.

Tales of meteorite finds seem to be largely detective works, which make them fun to read. Anyone interested in meteorites will find this combination of science and history a delightful reading experience.

Gretchen Luepke, 650 Alamo Court #6, Mountain View, California 94043; Gluepke@aol.com

DANA'S NEW MINERALOGY: THE SYSTEM OF MINERALOGY OF JAMES DWIGHT DANA AND EDWARD SALISBURY. *Richard V. Gaines, H. Catherine W. Skinner, Eugene E. Foord†, Brian Mason, and Abraham Rosenzweig. 1997. John Wiley & Sons Inc., New York. 8th edition, 1819 p. Hardcover, $250.00.*

Interestingly, but rarely known, the origin of James D. Dana's (1813–1895) interests in mineralogy and geology date back to the influence of Amos Eaton, founder of the Rensselaer School, later to be renamed Rensselaer Polytechnic Institute. As early as the late 1820's, Eaton's student Fay Edgerton was Dana's science teacher at Utica High School, New York. Following Eaton's method's and his textbook and that of Rensselaer professor Ebenezer Emmons (1799–1863), Edgerton stimulated Dana's interest in science and provided a role model of what it meant to be a scientist. In a sense, this is how this textbook started.

A second Rensselaer relationship with Dana is the initial publication history of this book. The Annals of the New York Lyceum published Dana's first edition (1837). This book followed Rensselaer professor Emmon's book, entitled "Manual of Mineralogy and Geology" (1826), which, according to the book's cover, was "adopted as a textbook in the Rensselaer School." Dana's teacher Fay Edgerton showed this book to Dana at Utica High School. The second edition (1844) of Dana's "System of Mineralogy" was printed at the expense of the author. After 1844, John Wiley and Sons Inc. of New York City published the third and subsequent editions, including the current book. The Wiley sons were Rensselaer alumni. William Wiley was an alumnus of the class of 1866, and Edward P.

†In memoriam: Eugene E. Foord (November 20, 1946–January 8, 1998)

Hamilton, alumnus of the class of 1907, chairman of the Board of Wiley and later Honorary Chairman of the Board, served for 34 years as a member of the Board of Trustees of Rensselaer, established Rensselaer's Wiley endowment, and thereafter the Edward P. Hamilton Professorship. Although I have found new written records of how Dana and Wiley go together, their mutual common history of connection to Rensselaer may have been a factor.

Another significant relationship between Dana and Rensselaer involved Washington A Roebling (1837–1926) and Henry B. Nason (1831–1895). Nason, one of the founders of the Geological Society of America, was the *de facto* curator of the mineral collections of Rensselaer. Nason acted as agent for Rensselaer in acquiring specimens and arranged and labeled them. Nason's interest in mineralogy had a profound influence on the scientific advance of mineralogy. Roebling, of Brooklyn-Bridge fame, took Nason's course at Rensselaer. Inspired by Nason, he embarked on a study of systematic mineralogy, which led to a collection of minerals that included not only all known mineral species and subspecies, but also representatives of all the useless names with which some mineralogists have confused and confounded the science. The Roebling collection was donated to the National Museum of the Smithsonian Institution. The liberal terms of the gift and the generous endowment by Roebling's son John allowed for further acquisition of specimens and the preservation of the collection. Roebling's collection was a source of much of the work of Esper S. Larsen (1879–1961) and Harry Berman (1902–1944), who were engaged to complete Dana's Seventh Edition. Berman for a long time carried on alone, but was killed in a plane crash while on war work. With Berman and Larsen working on the Seventh Edition, once again Dana and the Rensselaer heritage overlap through Roebling and Nason.

When I taught descriptive mineralogy at the University of Cincinnati, I followed the Dana approach, which, as spelled out in the current book, starts with the elements and then proceeds to the other natural mineral compounds, such as sulfates, oxides, carbonates, silicates, and phosphates. One of my favorite examination questions for undergraduates related to the classification of minerals, in which I expected the students to follow the Dana System as expressed in Dana's books. The Dana classification system assigns a number consisting of 4 parts separated by periods, based on a combination of chemistry and crystal structure of the minerals.

I joined the faculty of Rensselaer Polytechnic Institute almost 70 years after the death of Henry Nason. Yet I felt like his immediate successor. The Department of Geology appointed me as chairman of a one-person museum committee. Despite the work of Nason, none of the 6,000 mineral specimens had been accessioned, although their dispersal through the collection and display cases had followed the Dana System. I accessioned all 6,000 samples, putting white paint on each mineral, on which an accession label was inscribed in India ink. The accession records are retained in a ledger.

The present eighth edition of Dana's System of Mineralogy has been labeled "Dana's New Mineralogy." Five co-authors have spent many years in its completion. During the years of preparation of this book, the number of minerals covered has about doubled. This book attempts to describe, catalog, and classify all known minerals through December 31, 1995. Some anthropogenic minerals have been included. For each mineral, its crystallography is described in terms of polytypes, following which its chemistry is expressed and its optical properties given. Finally, occurrences and localities are cited. Several references are listed at the end of each mineral citation, but in my opinion the reference list could have been broader.

Gerald M. Friedman, Department of Geology, Brooklyn College and Graduate School of the City University of New York, Brooklyn, NY 11210; and Northeastern Science Foundation affiliated with Brooklyn College, Rensselaer Center of Applied Geology, 15 Third Street, P.O. Box 746, Troy, NY 12181–0746; gmfriedman@juno.com

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GEORGE GORDON. AN ANNOTATED CATALOGUE OF HIS SCIENTIFIC CORRESPONDENCE. *Michael Collie and Susan Bennett. 1996. Scolar Press, Aldershot, Hants, England; Ashgate Publishing Co., Brookfield, VT, U.S.A. 328 p. Hardcover, $102.95.*

The Rev. George Gordon (1801–1893) of Birnie, Morayshire, Scotland is a sterling example in nineteenth-century Britain of the well-informed, dedicated, amateur naturalist whose local expertise is avidly solicited and extensively exploited by the professional scientists of the metropolis. Gordon's most memorable service lay in the assistance he provided Roderick I. Murchison and Thomas Henry Huxley in their efforts to establish the geological age and taxonomic status of the celebrated Elgin reptiles. But for a judicious assessment of the scope and significance of his achievements, one needs to look beyond the "big names" who sought him out.

Gordon's life in science was something like what Charles Darwin's might have been had the voyage of the *Beagle* not put an end to the latter's plans to become a country parson. From the beginning, to be sure, religious devotion was more constraining in the case of the Scot, as his father was a Church of Scotland minister and his mother, the daughter of one. After obtaining an M.A. at the University of Aberdeen, Gordon took up a lengthy residence in Edinburgh, where he studied theology and cultivated a developing passion for natural history by attending a multitude of science courses. In 1832 he entered upon a living as minister of the parish church in Birnie, near Elgin. Here, evidently without slighting his pastoral duties, he threw himself into science, transforming the manse into a museum, a laboratory, and a clearing-house of scientific information. Gordon played a prominent part in the founding of the Elgin and Morayshire Literary and Scientific Association (1836) and the Elgin Museum (1843) and in collaboration with other local scientific virtuosi embarked on an encyclopedic inquiry into the geology and natural history of the country around Moray Firth. Year in and year out he filled his leisure hours with field excursions, specimen collecting, and modest publishing on scientific topics. His reputation as a expert naturalist took off, and he consulted widely on scientific topics and hosted scientific celebrities visiting from the metropolitan centers.

There was no self-aggrandisement in Gordon's approach to science. Though capable of defensive measures when he felt that the London scientists were brazenly misappropriating his work or that of his friends, he generally gave little thought to personal scientific honors and generously shared his findings with anyone who solicited information from him. As early as Gordon's middle years, some of the more unseemly aspects of professional ambition that would become commonplace in the twentieth century had begun to manifest themselves in British science, but the minister of Birnie was without this competitive drive. Nor did he have a stomach for scientific controversy. Whether designed to avoid controverted issues or not, his methods as a naturalist largely shielded him from disputes. Eschewing theory, he confined himself to observing, describing and classifying. Prudently the Presbyterian pastor remained aloof from the debate on Darwinian evolution.

This catalogue of Gordon's correspondence, which inventories letters he received, as well as those he wrote (usually in draft versions), has been compiled with care and considerable thoroughness, though it is far from exhaustive. Toward the end of his life Gordon, in an act of self-censorship, went systematically through his private papers, selecting some for donation to the Elgin Museum and some, the great bulk, for suppression. Letters bearing on family matters and others that Gordon judged too personal for public exposure were likely destroyed. What has survived, 1,300 letters preserved in the museum and an additional 200 discovered in other British archives, is nonetheless a valuable record of scientific pursuits in nineteenth-century Britain. The correspondence ranges between 1829 and 1893 and includes exchanges Gordon had with scores of individuals, many locally prominent as scientific amateurs and some of wider reputation such as Robert Brown, Hugh Falconer, Archibald Geikie, William Hooker, John Lubbock, and Andrew Ramsay, in addition to Murchison, Huxley, and Darwin. The compilers have given brief indications of the contents of each of the letters and have bracketed the inventory of the correspondence with an informative introduction and helpful appendices, one giving full citations of works mentioned in, or related to, the letters; another, biographical particulars on Gordon's correspondents and acquaintances; and a third, a bibliography of Gordon's publications.

As a guide to Gordon's extant correspondence the catalogue leaves nothing to be desired, though lest the eager historical researcher be enticed by glittering visions of new and untapped archival sources to work with, it should be pointed out that Michael Collie and others have already drunk rather deeply at this well. In his *Huxley at Work* (1991), Collie transcribed the Huxley-Gordon correspondence and wove it effectively into a detailed analysis of Huxley's Scottish paleontology. Similarly Collie and John Diemer have published the Murchison-Gordon letters as an appendix to their splendid monograph *Murchison in Moray* (1995). And there is more exploitation of the Gordon correspondence in Ian Keillar and John S. Smith, eds., *George Gordon Man of Science* (1995), as well as in Diemer's article "Old or New Red Sandstone? Evolution of a Nineteenth Century Stratigraphic Debate, Northern Scotland," published recently in *Earth Sciences History.*

Robert H. Silliman, Department of History, Emory University, Atlanta, GA 30322

VOLCANOES: CRUCIBLES OF CHANGE. *Richard V. Fisher, Grant Heiken and Jeffrey B. Hulen. 1997. Princeton University Press, Princeton, New Jersey. 315 p. Hardcover, $35.00.*

Volcanoes: Crucibles of Change capitalizes on the recent upsurge of public interest and awareness of volcanoes, and although firmly rooted in the science of volcanology, is not bound by the rigors of science. Volcanoes are a wonderfully exciting topic, and through this book, the reader is taken on a journey that surveys the breadth of the subject of volcanology. The book contains a wonderful collection of anecdotes about volcanoes and human experiences with them and is interspersed with vignettes of the history of volcanology.

Volcanoes: Crucibles of Change focuses on human-volcano interactions, both ancient and modern. The book uses description and interpretation of examples of modern (e.g. Mount St. Helens, Nevado del Ruiz, El Chichon, Kilauea, Unzen, and Pinatubo) and classic (Krakatau, Mount Pelee, Vesuvius and Pompeii, Santorini, Valley of Ten Thousand Smokes) eruptions to illustrate the power, variety, and unpredictability of volcanoes. It is up to date and induces advances in volcanology learned from the most recent eruptions as well as recent reinterpretations of ancient eruptions. Encounters of modern jet aircraft with volcanic plumes, a significant but until recently unrecognized volcanic hazard, are discussed in depth. Climate impacts of volcanoes on human culture and the biosphere, both short and long term, are explored. Unlike most others, this book doesn't stop with the negative impacts of volcanoes on human culture; it also discusses the beneficial aspects of volcanoes—from building materials, fertile soils, and mineral deposits to the preservation of human fossils and culture by volcanic deposits.

The book is written primarily for nongeologists; complex concepts are simplified and jargon is minimal. Some professionals might consider certain concepts oversimplified, but the lack of rigor greatly enhances the interest and comprehension for the nonvolcanologist. The book is not a comprehensive survey of volcanology, but it does cover all the major types of volcanoes and volcanic eruptions, and discusses many related topics. Nevertheless, interested amateurs and experienced volcanologists alike will find it interesting and rewarding reading.

The text is drawn from the senior author's lifetime of experience working on volcanoes and volcanic deposits. The writing is clear and delightfully easy to read. The authors make liberal use of quotes from ship's logs, eyewitness descriptions, historical documents, and other sources that often add a poignant human element to the text. Each chapter concludes with a short prelude to the next subject, providing continuity and enticing the reader to continue to the next chapter. Many readers will whiz through this book and be disappointed when they are finished because there is no more. The book is adequately illustrated, although the half-tone printing process on uncoated paper does not convey the scientific details contained in many of the photographs. The computer generated graphics, simplified geologic maps, and especially the 3D block diagrams are extremely useful to assist the reader's understanding of complex volcanologic processes that are impossible to convey in words alone. References at the end of each chapter, mostly books but including relevant scientific journals, guide the interested reader in the proper direction to obtain additional or more rigorous information. However, these are far from complete and fail to cite important work.

Volcanoes: Crucibles of Change is organized into four parts. Part I, Volcanoes and Eruptions, describes the types of volcanoes and eruptions, and explains why volcanoes erupt. Chapter 1 focuses on the 1980 eruptions of Mount St. Helens. The progression of the eruption, its proximal and distal effects are artfully integrated with description of the problems created by governmental and public response to the growing unrest. Chapter 2 explains volcano distribution and behavior, beginning with the plate tectonic connection and explaining the physical processes that lead to the diversity of behavior in individual volcanoes. Chapter 3 describes the types of terrestrial volcanoes and their eruptive styles, illustrated with examples of real eruptions. Chapter 4 recognizes that the majority of the Earth's volcanoes are submarine and focuses on the interactions between water and volcanoes and its role in the formation of volcanic deposits.

Part II, The Hazards of Volcanoes, describes the hazards presented by volcanoes to human culture. Each chapter documents the hazard posed by a type of eruption, and illustrates it with examples from the historical record of volcanic events. Along the way, human attempts to mitigate volcanic hazards are explored. Chapter 5 describes the most frightening type of volcanic eruption, pyroclastic flows and falls. Chapter 6 investigates volcanic avalanches and various types of debris and mudflows. In Chapter 7 we are taken for a wild ride on a moving lava flow and discuss human attempts to divert lava flows from inhabited places. Chapter 8 describes the hazards of volcanic plumes by contrasting the harrowing encounters of a classic sailing ship and a modern jet aircraft with debilitating ash clouds. Chapter 9 discusses the origin and composition of volcanic gases, their local effects on the environment, and addresses both the short and long term effects of volcanic gases on the biosphere and global effects on weather patterns.

Part III, Myths and Benefits of Volcanoes, takes the discussion of volcanoes away from the description of types and processes. Instead, we learn that volcanoes are not always destructive and focus on the beneficial aspects of volcanoes and human interaction with them. Chapter 10 discusses the history of the human perspective on volcanoes and their effects on human society from myths to modern volcanology. Chapter 11 discusses the direct beneficial effects of volcanoes on society, from health spas to kitty litter. Chapter 12 discusses resources provided by volcanoes, from diamonds to geothermal energy. Chapter 13 describes fertile volcanic soils, rebirth and recovery of the landscape from the effects of eruptions. Chapter 14 juxtaposes the destructive power of volcanic eruptions with the potential for their deposits to preserve the archeological record of human activity.

Part IV, Living near Volcanoes, explores how human society copes with the hazards of living with volcanoes and attempts to mitigate their effects. Chapter 15 discusses volcano hazard assessment, it application to erupting volcanoes, and recounts several recent eruptions and the lessons learned from them. An appendix, The Volcano Traveler, provides useful information for readers whose appetite for volcanoes has been whetted and wish to explore the wonders of volcanoes first hand.

In summary, I believe this book would be enjoyed by anyone desiring to learn more about volcanoes and their impact on society. Its breadth makes it useful to a wide range of readers from the uninitiated to the professional volcanologist, although I think the educated layman has the most to learn from reading it. It is probably the best popular book on volcanoes available today, and would make a wonderful selection for a scientific book club.

Michael A. Clynne, U.S. Geological Survey, 345 Middlefield Road MS910, Menlo Park, California 94025. mclynne@mojave.wr.usgs.gov

THE NATURE OF DIAMONDS. *George E. Barlow, ed. 1998. Cambridge University Press in association with The American Museum of Natural History. 278 p. Hardcover $74.95; Softcover $29.95.*

Designed to accompany an exhibition of the same name at the American Museum of Natural History, New York, this book provides a useful summary of current scientific, technological, and cultural knowledge of the diamond. Unlike the usual run of guidebooks that one reads as one examines exhibits, is not keyed directly to the exhibits but rather provides background reading on the topics which are featured in the exhibition. The latter is to run from November 1, 1997 to April 26, 1998, after which it is planned to send it on a nation-wide tour as far as California. Long after the exhibition is dismantled, it will continue to be read because of its eighteen articles contributed by twelve authorities. Thumbnail biographical sketches are provided for the authors. Each contribution is well illustrated with mostly color photographic reproductions, maps, and diagrams, and accompanied by notes, references, and bibliographies. On the whole, the book is very attractively designed both in respect to the selection and placement of illustrations and the typography. The binding of the paperback, the only copy examined, is cement-backed or "perfection" bound, which of course is far less desirable than regular sewing. An index is provided for the whole volume.

The text begins with a series of contributions that deal with the chemical and physical properties of the diamond (G.E. Harlow), color in diamond (E. Fritsch), origin (M.B. Kirkley), natural sources of diamond other than the earth's mantle (G.E. Harlow, V.S. Shatsky, N.V. Sobolev), and a particularly fine survey of deposits on a world-wide basis (A.A. Levinson), showing that we will not run out of natural diamonds for a long time as deposits in hitherto ignored regions are prospected.

Introducing the next series of articles is a large, unsigned article that briefly describes a select group of famous cut diamonds, including the Tiffany yellow which provides the front cover illustration. The first article in this group provides a short history of human knowledge of diamond which emphasizes its first recognition in India, B.C.E. (G.E. Harlow) and demonstrates the progress of this knowledge into Europe. Another short article discusses the development of diamond cutting designs (B. Zucker) and is followed by two articles on the employment of cut diamonds in regal ornaments and rings (D. Scarisbrick), and closing with an interesting dissertation on the mentions of diamond in English literature from Shakespeare to the present (C. Slade).

The final section is led off by a brief survey of some imperial treasures in the Armory of the Moscow Kremlin, apparently selected because of their lavish cut-diamond ornamentation (I.D. Kostina, G.E. Harlow). The next article, much fuller and handsomely illustrated, traces the recent history of the diamond in modern jewelry, with emphasis on designs, designers, and makers (J. Zapata). In the same popular vein, two other articles close the section, the first on Hollywood's fascination with diamond jewelry (M. Spiegel), and a general article on diamond in the Twentieth Century (G.E. Harlow). The above articles conclude what might be termed the purely cultural text and somewhat inexplicably are followed by two articles whose scientific and technological content suggests that they should have been incorporated in the first section of text. The first is a large, well illustrated and admirable tracing of the progress of the diamond from its place in the earth to its recovery, grading, and cutting, with remarks on industrial applications (G.E. Harlow), while the second article, equally interesting and valuable, treats the manufacture and applications of the synthetic diamond in science and industry (A.T. Collins).

This attractive, generally easy-to-read compilation is valuable mainly for bringing us up to date on advances in the knowledge of the diamond as a substance and on its natural occurrences which strongly suggest that this unique mineral is far more common than once supposed.

John Sinkankas, 5372 Van Nuys Court, San Diego, CA 92109. Phone 619-488-6904

THE BIOLOGICAL UNIVERSE: THE TWENTIETH-CENTURY EXTRATERRESTRIAL LIFE DEBATE AND THE LIMITS OF SCIENCE. *Steven J. Dick. 1996. Cambridge University Press, New York. ISBN 0 521 34326 7, 578 p. Hardcover, $54.95.*

Are we unique? Are we alone in the universe? Or do intelligent beings populate other planets, nearby or far away? In this book Steven Dick, an astronomer and historian of science at the U.S. Naval Observatory in Washington, traces the long history of ideas about life beyond the Earth as they have developed in the West from the time of Democritus and the Greek atomists of the 4th and 5th centuries B.C. to the mid-1990s. As indicated in the title, his focus is on the 20th century when Dick argues that the concept of a biological universe, replete with extraterrestrial intelligence, came into its own as a worldview legitimately seeking confirmation by scientific investigations analogous to those that have been applied to the physical universe since the time of Kepler, Galileo, and Newton.

This is the first book to explore the 20th century extraterrestrial life debate from the standpoint of the history of science. It is a clearly-written, fully-referenced work that should find a wide readership among scientists, students, and interested nonspecialists who are looking for authoritative information on any or all aspects of this subject. It also is a very timely book. By sheer chance, it appeared shortly before the announcement in August, 1996, of the finding of possible signs of ancient life in a Martian meteorite. Today, more than a year later, the presence of biogenic activity, past or present, remains unconfirmed. In the interim research on this problem has burgeoned, with both the NSF and NASA supporting proposals aimed at either positively confirming evidence for life on Mars or explaining the evidence in terms of a nonbiologic origin. With the outcome still in doubt, the absence of this episode is not a serious loss to the book. Too little time has passed to compose a well-balanced analysis of it. Meanwhile, in a period of heightened interest in this subject, Dick's book provides an instructive and sobering account of previous reports and speculations about extraterrestrial life.

The current flurry of research has brought the subject of life in other worlds decisively within the purview of science. Up until now, as a topic not amenable to observation, experiment, and falsification, it has commonly been relegated to the status of pseudoscience or science fiction. In the 1960s, for example, the evolutionist George Gaylord Simpson, spoke for many when he derided exobiology as a science looking for a subject. In the 1980s, the physicist Frank Tipler argued that the subject is a pseudoscience unfit for presentation at international scientific meetings. And in the early 1990s the distinguished evolutionist, Ernst Mayr, estimated the vanishingly small probability of intelligent life ever having evolved—even on the Earth—and expressed astonishment that, in a time of serious national debt, NASA was funding SETI (the Search for Extraterrestrial Intelligence). Within six months, Congress terminated the program, but SETI is carrying on with private support.

Regardless of the opinions of scientists, popular belief in life outside the Earth has soared in recent decades, propelled by a mass market of literature, movies, and television programs. Dick cites polls showing that in the 1990s over half of well-educated Americans believe implicitly in the existence of intelligent extraterrestrials. To some, the vast numbers of newly-discovered galaxies, each filled with billions of stars, virtually guarantees that some of the stars will be orbited by habitable planets. And, where life is possible, life is expected to exist. The popular belief also springs from what Dick calls deep psychological yearnings for cosmic companionship and a wish for the sky to be teeming with life.

In separate chapters, Dick explores numerous specific issues, among them "observations" of engineering works and vegetation on Mars, searches for evidence of life on Venus, the depictions of extraterrestrials in literature and the arts, the intricacies of the UFO controversy, and of SETI, Gaia, and the anthropic principle. However, many readers will find the heart of the book in Chapter 7, "The Origin and Evolution of Life in the Extraterrestrial Context." Here, Dick leads us through the swings of the pendulum that have characterized western thought about life from space.

One of the longer-lived but less familiar hypotheses originated with the ancient atomists who believed in an infinite number of worlds made fecund by innumerable seeds flying about through infinite space. This view was almost totally silenced, first by the Aristotelians and later by the Christians, both of whom insisted on a closed universe centered upon the Earth as the only abode of life. Not until the 17th and 18th centuries, after Copernicus, Kepler, and Galileo had displaced the Earth from its central position and redefined it as a planet, did speculations begin on the physical and mental characteristics of Martians, Jovians, and even inhabitants of comets.

When Darwin published his theory of evolution in 1859, he offered no scientific hypothesis to explain the origin of life itself. The prevailing view was that life must have sprung into existence by spontaneous generation, and in 1871 Darwin spoke, in passing, of a warm little pond as the most likely locale. Meanwhile, Louis Pasteur had performed a series of meticulous experiments that failed to prove the possibility of spontaneous generation. His results persuaded many scientists that life arises solely from life; therefore it could not have originated on the inorganic Earth and must have arrived here from space.

In the first decade of our own century, the Swedish chemist and physicist, Svante Arrhenius, addressed this problem by proposing his panspermia thesis, in which living seeds travel throughout space and implant life wherever they encounter favorable habitats. Other leading scientists, including Lord Kelvin, supported panspermia, at least for a time. The theory went into eclipse in the 1920s only to arise again in the 1960s as "neopanspermia," a hypothesis that not viable sperm but prebiotic chemical molecules drift through space and combine to form life when they encounter planets with favorable conditions. Support grew for this idea with findings of organic molecules in comets, interstellar clouds, and the atmospheres of the giant gaseous planets. By 1980 Fred Hoyle and his colleague Chandra Wickramasinghe, were arguing that frozen bacteria, viral particles, and algae travel through space; a year later they attributed the sudden bursts of new species on Earth to arrivals of genes from space. Today, explosive impacts of bodies from space, rather than extraterrestrial genes or living matter of any kind, are credited with altering the course of evolution by triggering extinctions followed by rise of new species.

The idea that meteorites may carry life to Earth dates back only as far as the 19th century after meteorites themselves were accepted as genuine natural phenomena. Dick traces the idea to 1834 when the Swedish chemist, Jöns Jacob Berzelius, analyzed carbon in a stony meteorite. Flurries of excitement about possible life in meteorites occurred in the 1870s and 1880s, in the 1930s, and again in the 1960s, when not only molecular hydrocarbons but also fossilized primitive organisms were reported in carbonaceous meteorites. Opponents savaged the evidence and brought an end to the debate at that time. Since then, no traces of life have been reported in the thousands of meteorites that originated as the debris of collided asteroids. The current search for signs of life is focused on one of only 12 meteorites judged to have been propelled to Earth by the force of a giant impact on Mars. Such an origin is based on their distinctive rock composition and content of noble gas isotopes that match those of the Martian soils and atmosphere analyzed *in situ* by the Viking landers in 1976.

Some asteroidal meteorites do, however, contain complex molecular building blocks of life. Dick cites the example of the carbonaceous meteorite that fell in 1969 at Murchison, Australia, containing 74 amino acids, 55 of which, including certain 'right-handed' forms, do not occur on the Earth. More recently, a large variety of amino acids has been analyzed in carbonaceous meteorites collected in Antarctica. These organic compounds are believed to be of astrophysical rather than biogenic origin, but their presence demonstrates that some of the major organic components of life do occur in other planetary bodies. Even so, we still have much to learn about the crucial combinations and linkages that lead from prebiotic compounds to living organisms.

To counterbalance the popular belief in intelligent beings with whom we might communicate some day, Dick presents arguments that we may be unique and utterly alone in the universe. That view currently is gaining support as more scientists contemplate the strictly contingent nature of each step in the long series of events that led to the formation of our own habitable planet 4.5 billion years ago, and the innumerable chance occurrences that have dominated the history of life over the past four billion years. Most early forms of life and many later ones became extinct, and each of the alterations that, quite by chance, ultimately resulted in *homo sapiens* was unpredictable and unrepeatable. Thus, if extraterrestrial intelligence exists it most likely will be in some nonhumanoid form unimaginable to us.

Dick's book is so engagingly written that, once opened to any page, it is difficult to put it down. Every person with an interest in the long history of ideas relating to life in the universe will feel grateful to Dick for placing between two covers this wealth of information that otherwise is available only in widely scattered sources.

Ursula B. Marvin, Harvard-Smithsonian Center for Astrophysics, Cambridge, Massachusetts

INTERESTING PUBLICATIONS

Gerald M. Friedman, Editor

Since the start of this journal, Founding Editor Gerald M. Friedman has prepared this column. Contributors wishing to list recent books and papers of interest to our members are requested to send them to: Gerald M. Friedman, Brooklyn College and Graduate School of the City University of New York, c/o Northeastern Science Foundation, Inc., Rensselaer Center of Applied Geology, P.O. Box 746, Troy, NY 12181-0746; gmfriedman@juno.com

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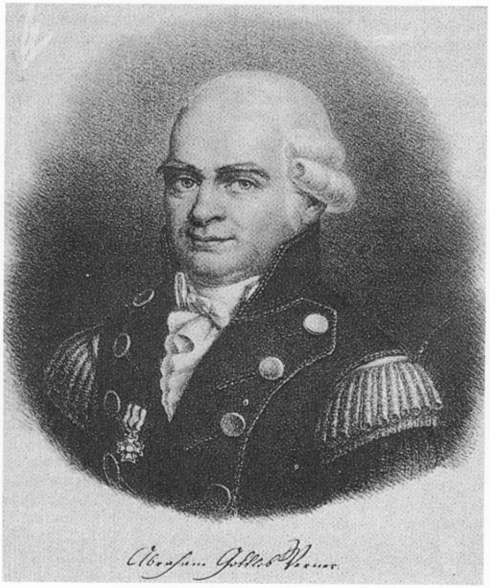
ANNOUNCEMENTS

International Symposium 19 to 24 September 1999



25 September 1999 marks the 250th Anniversary of the Birth of Abraham Gottlob Werner (1749–1817). The anniversary is the occasion for the following symposium:

"Abraham Gottlob Werner (1749–1817) and his times" is the subject of an International Symposium scheduled 19–24 September 1999 at Freiberg, Germany. Sponsored by the Technical University Bergakademie Freiberg and the International Commission on the History of Geological Sciences (INHIGEO), the symposium will focus on the history of geological sciences in Werner's times. Participants may offer papers addressing the following themes: (a) Knowledge of the earth from 1750 to 1820 and the geological ideas of A.G. Werner; (b) Developments and communication, theoretical concepts and academic controversies, research centres and influences in geological sciences in the time of Werner; (c) The relationships between geological knowledge and scientific, ideological and religious ideas during the Enlightenment and the early Industrial Revolution; (d) Werner and the technical disciplines related to mining ca. 1750–1820; (e) Werner and his natural history collections, private library and coin collection in relation to other contemporary coin collections and private libraries; (f) Werner's influence beyond the earth sciences and mining technology; (g) the history of the influence and the reception of Werner's work.

Copies of the first circular are available from:

Tagungsbüro "Werner-Symposium", Akademiestrasse 6, TU Bergakademie Freiberg, D-09599 Freiberg (Sachsen), Germany.

E-mail contacts are Dr. Peter Schmidt (pschmidt@ub.tu-freiberg.de) or Prof. Helmuth Albrecht (halbrecht@vwl.tu-freiberg.de).

Thanks very much for your attention to this.

Yours sincerely,
Kenneth L. Taylor