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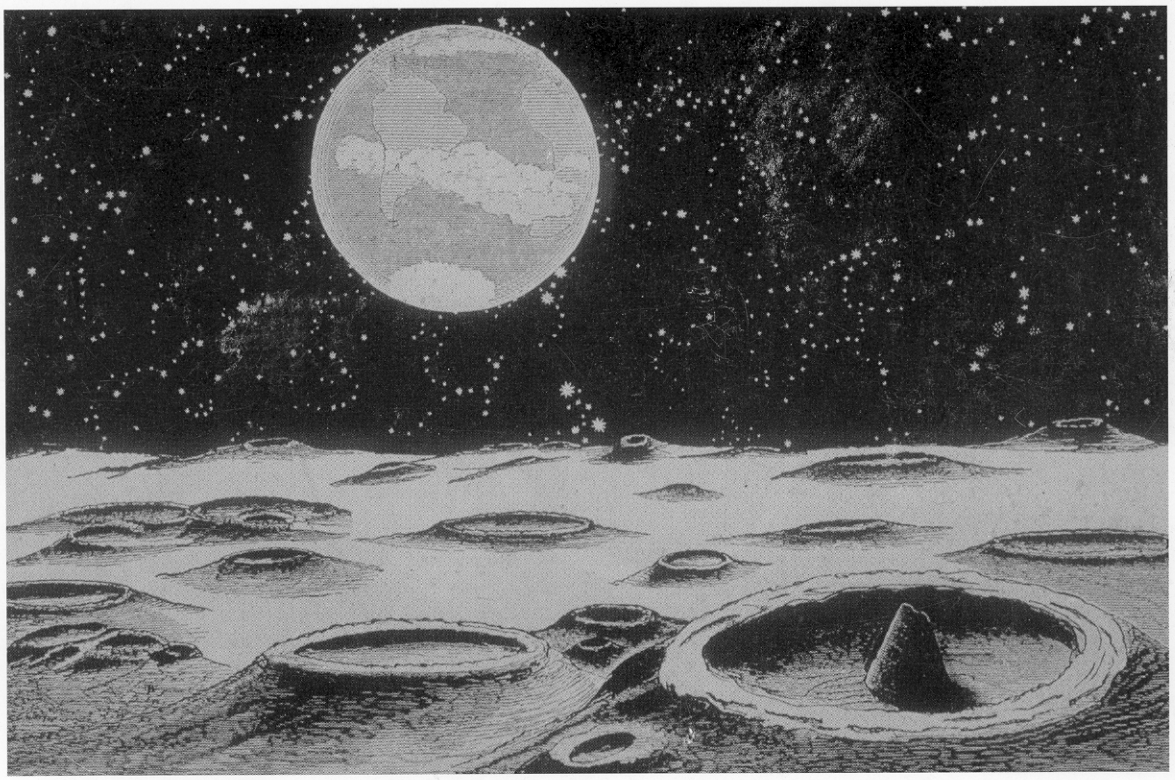

IMPACTS ISSUE:
ROCKS FROM SPACE?

In 1873, Richard A. Proctor portrayed Earth as if seen against a backdrop of stars, with lunar craters in the foreground. This illustration appeared in Proctor's The Moon. *See Peter Schultz's article, within.*

EDITORIAL
EVER SINCE COPERNICUS

GREGORY A. GOOD

After Copernicus, the boundary between Earth and the Cosmos blurs. In the Aristotelian worldview, a definite line demarcated two separate realms: the terrestrial and celestial. Copernicus erased the line. Ever since then, science and the human race have seen the Earth from a variety of different, sometimes conflicting, often unresolved perspectives. This special issue of *Earth Sciences History* focuses on one embodiment of this conflict and the resolution that continues nearly 500 years after Copernicus made Earth a planet.

CONTINUITY

This issue of the journal marks another transition, one of editorship. Mott Greene has shepherded the journal since volume 13 in 1994. Mott and guest editor Jody Bourgeois conceived this issue, volume 17, number 2, and saw it through the critical stages of review and revision. I have come along late in the process, to see it through the press. With volume 18, my responsibility will be to carry on in the tradition established by founding editor Gerald M. Friedman and carried on by Mott.

Mott has done a fine job of bringing new authors and new readers to *Earth Sciences History*. This is important to the History of the Earth Sciences Society, since the Society's health hinges on the health of the journal. My goals as I pick up the editor's green pen are to continue to serve the journal's current readers, while bringing new ones to increase the ranks of HESS. Ultimately, I want the history of the geosciences to be as familiar a field of history of science as the history of biology and the history of physics are now.

As many of you will know from my recently published work *Sciences of the Earth: An Encyclopedia of Events, People, and Phenomena* (New York and London: Garland Publishing, 1998), I advocate an inclusive history of the earth sciences. Sciences of the Earth include geology, geography, geophysics, oceanography, meteorology, climatology, aeronomy, and those parts of space physics and planetary science related to the Earth. And this is not an inclusive list. The geosciences also include matters from earlier periods when twentieth-century categories (subjects, disciplines, etc.) did not hold. To put it simply, *Earth Sciences History* must reflect this breadth. Its very title demands that it do so. Certainly both previous editors had this goal, too. Let me assure readers and potential contributors that I will continue in this vein. I strongly encourage writers interested in the histories of all the earth sciences to consider this journal their journal of choice.

Let me assure you, too, that the journal will continue to draw on and serve both earth scientists and historians of science. HESS needs to reach out ever more to both scientists and humanists and not to limit itself. Articles in *Earth Sciences History* need to be firmly grounded in science. The stories they tell should be interesting and one hopes compelling. Likewise, the journal must continue to be open to topics and authors worldwide. Africans, Asians, Americans north and south, Europeans, and others should all feel welcome in these pages. Someone might note that our journal has included mainly articles on European and North American topics. In volumes 14 through 16, European topics accounted for 17 articles, North American for 9, and there was only one that looked elsewhere. This distribution would change if we included more volumes. There have been special issues on polar research, oceanography, and now the Earth in the Cosmos. Nevertheless, we need to reach out more. Earth science is global and so too should its history be. I strongly encourage writers worldwide to contribute.

Potential contributors should also know that I will continue the same commitment previous editors have had to quality. Two referees selected by the editor will review all submissions. Authors will know who they are; they will know the author. Manuscripts will go through however many revisions are needed to reach a publishable standard. While this will sometimes be more work for the author, it will also be more work for the reviewers and me. The standard of the journal, though, is more important than easy publication. Our field will prosper when our standards are high.

If you have not written for the journal before, I recommend that you go back to Mott's first editorial, "Signing on—With A Vote of Thanks," and review his "Five Steps to Writing Good 'Earth Sciences History'."

1. Turn your Topic into a Problem
2. Make the Context of Your Work Explicit
3. Remember to Write What "Goes without Saying"
4. Present Sufficient Technical Detail, and
5. Follow the Professional Standards of the Field

Mott discusses these points in more detail in volume 13, number 1, 1994, pp. 1–2.

NEW DIRECTIONS: THE FOCUS BOOK

Any editor brings something new to a journal. Mott brought the new sections **Documentation** and **Letters to the Editor,** which I will continue. I will add a section of my own, beginning with volume 18: the **Focus Book.** In each issue, a recently published book of

broad interest in the history of geoscience will be highlighted. Two essay reviews—one by a scientist and one by a historian—will discuss the book from different perspectives. The book's author will then answer these essays, offer rebuttals perhaps, or refine a point. I intend **Focus Book** to emphasize that history of geoscience is a conversation among scholars. I also hope this section will begin to generate the common ground that will help us transcend our own tightly defined perspectives. Maybe we will also argue a little, but with proper control, this too will be productive.

WORDS OF THANKS

I close my first editorial with some explicit words of appreciation. First, to Ken Taylor, the other HESS officers, and the membership at large, I am deeply obliged and honored that you have offered me this opportunity. I will try hard to be a good steward of your trust. Second, my thanks to Mott Greene, who has set a high standard and has handed me a journal in such good condition. Third, I am glad to have Dorothy Sack helping as HESS treasurer and Gretchen Luepke continuing as book review editor. They have both helped immensely as I have struggled to take up my new tasks. Fourth, Heidi Raak at Allen Press oriented me during a tour in Lawrence, Kansas that went from the mailroom through every production step to binding and shipping. The only omission was Toto. It will be a joy working with her and with Allen Press. Lastly, I should thank West Virginia University, which is supporting my editorship in several material ways.

SUGGESTIONS FOR CONTRIBUTORS TO EARTH SCIENCES HISTORY

1. *EARTH SCIENCES HISTORY* promotes and publishes historical work on all areas of the earth sciences—geology, geography, geophysics, oceanography, paleontology, meteorology and climatology. The journal honors and encourages a variety of approaches to historical study: biography, history of ideas, social history, and histories of institutions, organizations and techniques.

2. Submit **manuscripts** (original and two copies) to the Editor: Gregory A. Good, History Department, Room 202A, West Virginia University, Morgantown, West Virginia 26506-6303, U.S.A. Please include an **abstract** of approximately 150 words. Contributors should retain a copy for reference, and should include return postage or international reply coupons if they desire return of submitted material.

3. Manuscripts should be **typewritten** or processed on a **letter quality** printer and **double-spaced** throughout, including quotations and notes, on paper of standard size and weight. Margins should be wider than usual to allow space for instructions to the typesetter. All copy should be flush left, with the right hand margin left ragged (unjustified) to maintain even spacing and readability.

4. Revised manuscripts should be submitted in double-spaced hard copy and, whenever possible, on **3.25″ diskettes** identifying both the platform (Mac, PC or Other) and the word-processing program used (WordPerfect 3.0, Word 5.1 etc.). All diskette copy should have **formatting stripped out**: it should all be flush left, unjustified, with no special character formats other than underlining (italics).

5. Bibliographic information should be given in **endnotes** (not parenthetically in the text), typed separately from the main body of the manuscript, **double-** or even **triple-spaced,** numbered consecutively throughout the article, and keyed to reference numbers in the text.

   a. References to **books** should include author's full name; complete title of the book, underlined (italics); place of publication and publisher's name for books published after 1900; date of publication, including the original date when a reprint is being cited; page number cited. *Example*:

      Eduard Suess, *The Face of the Earth,* 5 vols., Vol. I (Oxford: Clarendon Press, 1904), p. 17.

   b. References to articles in **periodicals** should include author's name; title of article, in quotes; title of periodical, underlined (italics); year; volume number, Arabic and underlined (italics): number of issue if pagination requires it; page numbers of article; number of particular page cited. Journal titles are spelled out in full on first citation and abbreviated subsequently. Example:

      David R. Oldroyd, "The Archaean Controversy in Britain: Part I—The Rocks of St. David's," *Annals of Science,* 1991, *48*:407–452, on p. 434.

   c. Succeeding citations of books and periodicals should use an abbreviated version of the title with the author's last name. Example: Oldroyd, "Archaean," p. 446.

6. **Figures** are welcome in illustrating articles. Line drawings should be directly reproducible, glossy prints must be furnished for all halftone illustrations. Where authors elect not to make voluntary page contributions (see 8 below), there is a charge of US $15.00 for each figure in excess of two.

7. Manuscripts should be submitted to *EARTH SCIENCES HISTORY* with the understanding that upon publication, **copyright** will be transferred to the History of Earth Sciences Society. This understanding precludes *EARTH SCIENCES HISTORY* from considering material that is under consideration or accepted for publication elsewhere.

8. *EARTH SCIENCES HISTORY* requests **voluntary page contributions** from authors in the amount of $15.00 US per page, but acceptance of manuscripts and publication are not contingent on payment of page charges.

LETTER TO THE EDITOR

Remarks on a book review by Amos Nur (*Earth Sciences History* 1997, 16, no. 1, pp. 52–58) on the *Destruction of Sodom, Gomorrah and Jericho* by D. Neev and K. O. Emery (1995)

We wish to thank Dr. Nur for his review of our book.

Dr. Nur's criticism focussed on the following three points. It is (1) speculative; (2) confused; and (3) controversial.

First and foremost, we have to remember that ALL physical models in geology are inherently limited by various degrees of uncertainty due to the nature of the studied material. Limitations on our ability to probe both temporally and spatially the characteristics of the studied object—the earth—oblige us to use indirect observations and to rely on such diverse fields as archaeology, climatology, and historical, oral and written documents. Some of the latter, a few thousand years old, accounted for events that occurred several thousand years earlier. How could our book, which is an interdisciplinary study, be other than speculative? And yet Dr. Nur wrote (see his point 4) that he liked the book.

As we do not agree with some of his critical remarks, we would like to clarify herewith some of these points.

(a) In his point no. 3 (controversies) Dr. Nur criticises us for the "... avoidance of any meaningful reference to plate tectonics, and especially to the Dead Sea transform as a strike-slip fault system". His words suggest that we repeatedly imposed throughout the book our personal preconceived idea that only vertical tectonics (i.e. normal faulting and uplifts, but not strike-slip horizontal differential movements) are "responsible for the ground and population upheavals" that have occurred along the Dead Sea Rift. A careful reading of the text (especially pp. 11–20 and figures 1.1 and 1.4) should convince the reader that the opposite is true.

We did not specifically refer to the general concept (or paradigm) of plate tectonics because this subject is not the main thrust of our book. We are also not sure if the use of the term "transform fault" is justified with respect to the Dead Sea strike-slip fault system (see Neev, Greenfield and Hall, 1985)*. Moreover, it cannot be expected that each of the postulations included in the plate-tectonics paradigm would instantaneously supply satisfactory explanations to all problems that come up in new geological studies. It seems to us a mistake, therefore, to block the way for the introduction of adjustments to some of the already-accepted concepts and definitions.

(b) In his point no. 1 (speculations) Dr. Nur raises some doubts regarding our ability to determine the dates, durations and frequencies of both earthquake's destruction or climatically-induced (mostly droughts) abandonment events.

Both Albright et al. (1944) and Rast (1987) suggest an association of the Sodom and Gomorrah tradition with the Early Bronze III settlements in the Dead Sea south basin. An interdisciplinary team of archaeologists (Rast and Shaub, 1982; Rast, 1987) and a geologist (Donahue, 1985) report two phases of destruction of the two Early Bronze III cities situated at the north-eastern corner of the Dead Sea south basin (i.e. a sequence of two earthquakes). The first one occurred in about 2400 B.C.E. and the second in 2350 B.C.E. Other Early Bronze III cities along the Dead Sea rift, and elsewhere in Canaan, were also damaged by these earthquakes.

Consequently, the cultural pattern of most of these sites was changed from intensive (irrigated) farming to grazing (semi-nomad) and occupational density dwindled. Some of these sites were totally abandoned. The epicenter of that second earthquake could have been located outside in the Dead Sea pull-apart basin, somewhere along the strike-slip fault, either north or south of the Dead Sea. Vibrations transmitted from that epicenter could have triggered a sudden phase of compaction within the very thick (8–10 km) and relatively loosely packed sedimentary sequences of the Dead Sea basin.

Dr. Nur mistakenly quotes us saying that "tectonic movements (are) ... responsible for both ground and population upheavals ...." We believe that climatic changes (drought within the subtropical zone and the advancing ice sheets in the north) are the dominant factors responsible for population upheavals. On the other hand, present-day experience indicates that damages caused by earthquakes, no matter how severe, do not deter people from returning to their old sites and routine practices, provided food production is more affluent there than in other areas.

Dr. Nur doubts whether we were really able to identify small climatic changes and to determine their durations and magnitudes. A most efficient tool for the study of that subject is provided by the sedimentary sequence of alternating rock-salt and laminated marl layers that underlies the Dead Sea bottom. These rocks, if accurately sampled, indicate most reliably the occurrences of local as well as global climatic changes, even if they were very short (less than 200 years). Their interpretation enables the drawing of more unequivocal conclusions as compared with results of isotopic studies of foraminifera recovered from deep-sea cores, where more ambiguous factors are involved. These rock-salt and marl studies, when combined with the results of the Mt. Sodom caves study (Frumkin et al., 1991), yielded information on the dropping and rising paleo-elevations

* Full references are to be found in our book.

of the Dead Sea levels. The Mt. Sodom caves were formed because of the dissolution of the rock-salt by the percolation process of meteoric water through fissures within this rising-diapiric mountain. These data enabled us to reliably portray the curve of the Dead Sea-level fluctuations during the Holocene.

Nevertheless, there is an Achilles heel to our study, which is in the miscorrelation noticed between the curves of climatic events dated according to the archaeological chronology, and the radiocarbon analyses determined on organic matter disseminated within the rock-salt–marl sedimentary sequence, as well as within the detrital marl fragments found in the Mt. Sodom caves. Whenever possible, the archaeologist would attempt to corroborate the time-scale of the archaeological stratigraphic column within the site he is studying by analysing the radiocarbon ages of the organic matter samples picked up from the same vertical sedimentary sequence. That procedure is recommended by Weinstein (1984), who warns the users not to rely upon results of radiocarbon datings if and where they are not corroborated by archaeological chronology. Gilead (1993) efficiently employed that recommendation in his study of the Chalcolithic sites of Beersheba. Apparently, that approach was also adopted in the Early Bronze III sequences of Bab edh-Dhr'a, Jericho, and Beth Yerah. On the other hand, our efforts to establish a consistent and reliable chronostratigraphy for the Holocene sedimentary sequence of the Dead Sea south basin, have failed (Neev and Emery, 1967, 1995, Figs 3.2 and 3.5) because the only tool we could use there for dating these sediments was the radiocarbon analyses of the disseminated organic carbon.

The same holds true with respect to the results of Frumkin et al. (1991) regarding the chronology of the Dead Sea fluctuating water levels that reflect climatic changes. These level changes were dated by radiocarbon ages of plant twigs that were buried within the detrital marl fragments accumulated at the bottom of the different Mt. Sodom caves. Apparently, these plant twigs reached the caves by floods fed by rains that fell on the Mt. Sodom catchment area at elevations higher than the opening of chimneys that led downward into the horizontal segments ("galleries") of the caves. The diagrams of elevations versus the radiocarbon ages of plant twigs in the Mount Sodom caves (Frumkin et al., 1991), as well as the stratigraphic profile of the rock-salt–marl sequence across the Dead Sea south basin and the composite logs there (Neev and Emery, 1995, Figs. 2.10, 3.2, 3.5), clearly reflect a climatically-induced process. The latter produced changes of both the Dead Sea levels as well as of the chemical and physical properties of the Dead Sea-water body.

Results of these studies also indicate the occurrence of the longest and coldest climatic phase of the Holocene, at least in our region, sometime between about 8000 YBP (the age of the cultural break between the pre-ceramic and ceramic Neolithic) and the present. The following physical events occurred within that cold-wet phase: (i) Levels of the Dead Sea rose by about 100 meters (from about −400 to about −300 m msl). (ii) Rock-salt ceased being deposited in the Dead Sea and instead an approximately 2 m thick marl layer accumulated on its bottom. (iii) The already abandoned Tell Beth Yerah was flooded because of the close to 20 m rise of the Sea of Galilee's level. It remained submerged for quite a long time. Sedimentological-archaeological data indicate that this wet phase was initiated in post Early Bronze IV times (or the synonymous term of the Middle Bronze I). That conclusion is in agreement with data from northwestern Mesopotamia, indicating that this wet phase had begun about 3900 YBP and declined close to the break between the Late Bronze and the Iron ages about 3200 YBP (Weiss et al., 1993).

New efforts are now being made by a joint team of scientists of the Hebrew University, Jerusalem and of the Max-Planck-Institute, Mainz-Germany in an attempt to reach more reliable and accurate age data for the Holocene–Late Pleistocene sedimentary sequence of the Dead Sea. Perhaps this study would help us to solve the above dilemma.

(c) In his point no. 2 (confusion) Dr. Nur asks us "What is a tectonic phase"? Based on definitions found in the Glossary of Geology (Gary et al., 1974), this term means the time interval of a sequence of structural events (such as faulting) that repeatedly occur during the development of a tectonic system. One example is the swarm of earthquakes that took place a few years ago within the Gulf of Elat (Aqaba) and lasted for more than a year. Another example could be inferred from the description of the cultural-demographic history of the Bab edh Dhr'a settlement (NE corner of the Dead Sea south basin) (Donahue, 1985, pp. 185–6). Relatively limited amount of human modifications occurred in that city during the close to 600 year long occupational stage beginning with the Early Bronze I culture, about 5000 YBP and terminating with its destruction by the great earthquake close to the end of the Early Bronze III settlement about 4350 YBP, when its occupation density reached its maximum. During Early Bronze IV (4350 to about 4100 YBP) the ruined site was subjected to increased erosion, perhaps because of a rejuvenated faulting phase (as inferred by Donahue from the increasing gradients of the streams). By that time the occupants of this site decided to settle at some distance away from the Early Bronze III ruined city and their irrigational agriculture way of living was changed to a grazing, semi-nomadic one. At that stage, their occupational density also appreciably dwindled. The 300-year-long time-lapse between 4400 and 4100 YBP should be considered as a "tectonic phase".

In conclusion, we believe that this note answers Dr. Nur's objections to our interpretation. Let the reader of our book decide for himself/herself.

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INTRODUCTION

Dedication
Robert Sinclair Dietz (1914–1995)
Eugene Merle Shoemaker (1928–1997)

When a new concept emerges in a discipline—in this case, the recognition by geologists of the importance of impacts in Earth's history—it is fairly common to forget that many of the ideas already existed, sometimes long before acceptance. On the other hand, it is common to "discover" and overemphasize early "pioneering" papers, which may, in fact, have had little effect in their own time. Generally said, it is tempting, in the search for pedigree for current ideas, to see older expressions of similar ideas out of their historical contexts. Nevertheless, it may still be interesting to ask about the historical roots of a concept—what were prior explanations of observed phenomena? If certain ideas did exist, and were known to the community, what were their contexts? What prevented their acceptance, for example, and what led, ultimately, to their acceptance?

With the help of Mott Greene, in 1994 I was putting together a symposium for the November meeting of the Geological Society of America, on historical ideas about the influence of "extraterrestrial" forces in Earth history. [Gene Shoemaker rightly objected to this term, as something that falls to Earth is not "extraterrestrial."] In February of that year, I attended the third "Snowbird" conference (the first two were held in Snowbird, Utah, the third in Houston) on impacts and other catastrophes in Earth history. I used this occasion to solicit papers for the symposium.

The first of the Snowbird conferences, held in October of 1981, was triggered by the Alvarez, Alvarez, Asaro and Michel hypothesis (published in *Science* in 1980) that an asteroid struck the Earth 65 million years ago and brought about a mass extinction that closed the Mesozoic Era. Part of the format of these conferences has been a free-wheeling debate, and it is said that Snowbird I was very heated. I attended Snowbird II, in October of 1988, and the heat was still on, but the majority of attendees accepted some version of an impact hypothesis. Other mechanisms, and other crises in Earth's history (particularly mass extinctions) were discussed. By the time of Snowbird III, although a vocal minority still opposed the hypothesis, the discussions had moved on, for most workers, to mechanisms, to possible linking of processes (e.g., impact and volcanism), and to other events in Earth's history.

At the 1988 meeting, I heard a refutation of the hypothesis that the Vredefort structure in South Africa was an impact structure, as proposed by Reginald Aldworth Daly in 1947 and later championed by Robert S. Dietz in the early 1960s. In 1994, at Snowbird III, a paper was presented that accepted the impact interpretation for Vredefort, and Dietz was there to hear it. When Dietz had first addressed the topic of impact structures on Earth, not even "Meteor Crater" was accepted by many geologists as being impact generated. Eugene Shoemaker's landmark work on that crater helped turn the tide of opinion. By the 1990s, most structures—over 100 on Earth—that Dietz, Shoemaker and others had proposed as impact craters and astroblemes could be catalogued as such.

At Snowbird III, I found Bob Dietz out in the atrium, with Gene Shoemaker. I wanted to include, in our historical symposium, a description of the debate between Dietz and Walter Bucher over "cryptovolcanic" structures: How did Dietz develop his ideas about "astroblemes" (star wounds), who else was involved, and how and why were those ideas opposed by Bucher and others? I asked Bob Dietz to present a paper on this topic, and he agreed. Gene Shoemaker expressed enthusiasm for the symposium, and I convinced him also to make a contribution.

In November in Seattle, the room was packed. Bob Dietz at 80 was famous in the geological community, both for his pioneering work on impact structures, and for his early contributions to marine geology and plate tectonics. And from his presentation in 1994 in Seattle, it seemed he still thought he had convincing to do with regard to astroblemes. His talk was primarily a world tour of shatter cones, which Dietz had originally proposed as evidence for impact in 1947, and had championed in several later papers.

Another giant in the field of geology and impact structures, Gene Shoemaker's presentation at the 1994 symposium also drew a large crowd. He focused more on early studies of comets, and on the conservative attitude of geologists such as Charles Lyell, with regard to the idea of cosmic influences in Earth history. Gene enjoyed the history of science, and clearly had fun presenting his story, he having played a major role in acceptance of ideas about impact in the geological community.

Bob Dietz and Gene Shoemaker didn't live to write the papers we had hoped for. We have tried to put together a volume that will honor their names. We hope it contributes to a historical understanding of this recent experience of the introduction and reception of a "new" concept.

Joanne Bourgeois
University of Washington
Seattle, November, 1998