Vol. 3, No. 3 March 1993

GSA TODAY

A Publication of the Geological Society of America

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INSIDE

Deep Drilling of the Oceanic Crust and Upper Mantle

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ABSTRACT

From the inception of the Deep Sea Drilling Program (DSDP) through to the present investigations by the Ocean Drilling Program (ODP), understanding the processes of generation and aging of the oceanic crust and upper mantle has been a primary objective. Time, technology, and logistics have somewhat restricted the success of these investigations, and it has been necessary until now to rely heavily upon the ophiolite model of ocean crust to provide information on crust-building processes. However, we now realize that many ophiolites, particularly the best exposed and studied, were not generated at major ocean-basin spreading centers but above subducting oceanic plates at convergent margins. In this article, the degree to which ophiolites can help us understand the first order processes of crustal accretion and spreading is reviewed, and a strategy for investigating the lower ocean crust and upper mantle through deep drilling is outlined. This strategy was first tested on Leg 118 in the Indian Ocean, where excellent recovery of gabbroic rocks gave us a preliminary look at in situ layer 3. It was also the focus of drilling on Leg 147 in the Pacific Ocean at Hess Deep in late 1992.

INTRODUCTION

Sixty percent of the present surface of Earth was created at ocean ridges, as magmas generated within the mantle cool to form ocean crust. Over the past two decades, increasingly detailed geological and geophysical investigations of spreading centers, together with field mapping and drilling of a few well-preserved ophiolite complexes, have led to a basic conceptual model for the complex interrelated magmatic, tectonic, and hydrothermal processes involved in the formation of oceanic crust.

The processes whereby ocean crust is created, modified, and destroyed have fundamental implications for the chemical and thermal evolution of the mantle, the buffering of seawater chemistry, and the generation of continental crust. The chemistry and mineralogy of the mantle are influenced by the removal of magmas beneath spreading ridges. Hydrothermal fluxing at the ocean ridges and lower temperature reactions between basement and seawater in older ocean crust control many aspects of seawater composition. Altered crustal rocks play a major part in the nature of volcanism at convergent plate margins, and the net fluxes at these margins have significantly influenced both mantle chemistry and the composition of the continental crust.

Large areas of the sea floor can now be imaged accurately with swath-mapping tools, which have already revealed how the floor of the major ocean basins is segmented. MacDonald (1991) has remarked on how the segmentation is apparent on a variety of scales. First-order segments, usually bounded by transform faults, are generally hundreds of kilometres long and persist for periods of the order of millions of years. They may be subdivided into second-order and even third- and fourth-order segments that become increasingly more transient. Such observations indicate a very orderly spatial and temporal pattern to most sea-floor-building processes, including magmatism, tectonism, hydrothermal activity, and later alteration.

ORIGIN OF OCEANIC CRUST

The oceanic crust of the major ocean basins is formed at oceanic spreading centers. These centers are expressed as the ocean-ridge system, below which the asthenosphere rises to fill the gap between separating tectonic plates. The ocean crust formed in this manner and in this environment has historically been the best studied. However, ocean crust is also formed in marginal basins, peripheral to the major oceans. Although such crust and the processes that formed it are less well studied, we do know that these are somewhat different from those in the major ocean basins. Indeed, it is clear that the production of ocean crust in general is quite variable from one tectonic environment to another and can be produced at spreading rates that may vary by an order of magnitude. Thus, although the development of a general model of the processes that form and alter ocean lithosphere is one priority, in the long term, wide sampling of the observed variability will be required to truly understand the role that ocean crust plays in geochemical cycling.

Our present ideas of the stratigraphy of the ocean crust come mostly from the study of ophiolite complexes, which are fragments of ancient ocean lithosphere. However, for "in situ" ocean crust only a seismic stratigraphy is available, and the seismic data suggest a uniform, worldwide structure. The correlation between typical ophiolite stratigraphy and the seismic model of the ocean crust has been tested at only one location in the oceans, because only one hole (DSDP 504B) has successfully drilled oceanic layer 2B (Becker, Sakai, et al., 1989). Drilling at DSDP Hole 504B has confirmed part

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Ocean Drilling Project Hard-rock guide base, used for drilling on bare-rock exposures such as at the mid-oceanic ridge, being readied for deployment on the *JOIDES Resolution.* Bare-rock sites lack the sediment cover of other holes which is used as a guide to hold the drill string and bit steady as it begins drilling into the hard rock. The hard-rock guide base is first placed firmly on the rock surface, and then the drill is guided to the rock by the conical structure.

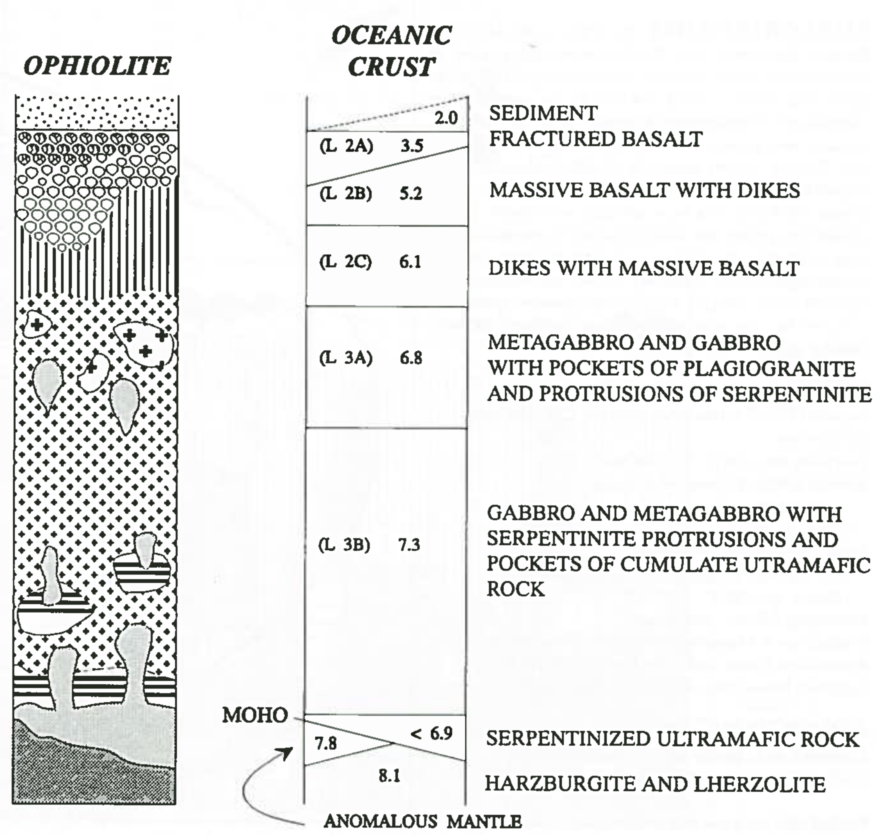

Figure 1. The correlation between seismic stratigraphy of the upper oceanic lithosphere and lithologies observed in ophiolites. L = layer deduced from seismology.

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GSA TODAY *March*
Vol. 3, No. 3 *1993*

GSA TODAY (ISSN 1052-5173) is published monthly by The Geological Society of America, Inc., with offices at 3300 Penrose Place, Boulder, Colorado. Mailing address: P.O. Box 9140, Boulder, CO 80301-9140, U.S.A. Second-class postage paid at Boulder, Colorado, and at additional mailing offices. **Postmaster:** Send address changes to *GSA Today*, Membership Services, P.O. Box 9140, Boulder, CO 80301-9140.

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of the ophiolite analogue in the form of a pillow-lava carapace overlying a sheeted dike unit (Fig. 1). The correlation of deeper structures of the oceanic crust with that perceived for ophiolites has yet to be verified by deep drilling.

However, the problem of correlation is more profound than this! Recent detailed mapping and drilling programs in several well-exposed ophiolites have shown that the classic layered stratigraphy developed by Penrose Conference attendees in 1972 cannot be universally applied. The best example of this comes from the Cyprus Crustal Study Project of the International Crustal Research Drilling Group (see papers in Malpas et al., 1990), which investigated the Troodos ophiolite.

The Troodos ophiolite is now believed to have formed at spreading axes in a suprasubduction-zone environment resulting from the convergence of the African and Eurasian plates in the Late Cretaceous. Three major suites of volcanic rocks are recognized on Troodos, all of which display geochemical characteristics thought to be related to development from the mantle wedge immediately above a subducting oceanic plate. The rocks show varied degrees of depletion in certain trace elements and preferred enrichment in others, suggesting derivation from a source from which earlier melts had been removed but which had undergone selective addition from fluids emanating from the subducting slab (Fig. 2). The massive sulfide bodies so characteristic of the Troodos pillow-lava sequence are the products of high-temperature hydrothermal solutions that vented on the sea floor along the flanks of axial grabens. Except in the narrow subvertical zones beneath these ore bodies, the Troodos lavas have undergone only low-temperature interaction with seawater. Secondary mineral assemblages consist chiefly of clay minerals, zeolites, and carbonates and appear to have been controlled largely by variations in permeability, lithology, water/rock ratios, and proximity to intrusions. It is the presence of fresh glass throughout the extrusive sequence that has provided an ideal opportunity to study the primary compositional data. This strongly suggests, by comparison with basalt compositions in the present west Pacific, that the ophiolite was produced in a convergent plate margin setting by sea-floor spreading above a subduction zone (Fig. 3). Similar findings apply to most other major ophiolites, which raises the questions, If ophiolites are from environments of formation that are different from modern major ocean ridges, are they sufficient analogues of normal ocean crust, and are the processes that produced the rocks similar in both cases? We must weigh the fact that information we have obtained from ophiolites is still clouded with uncertainty regarding the influence of postformation events, including obduction.

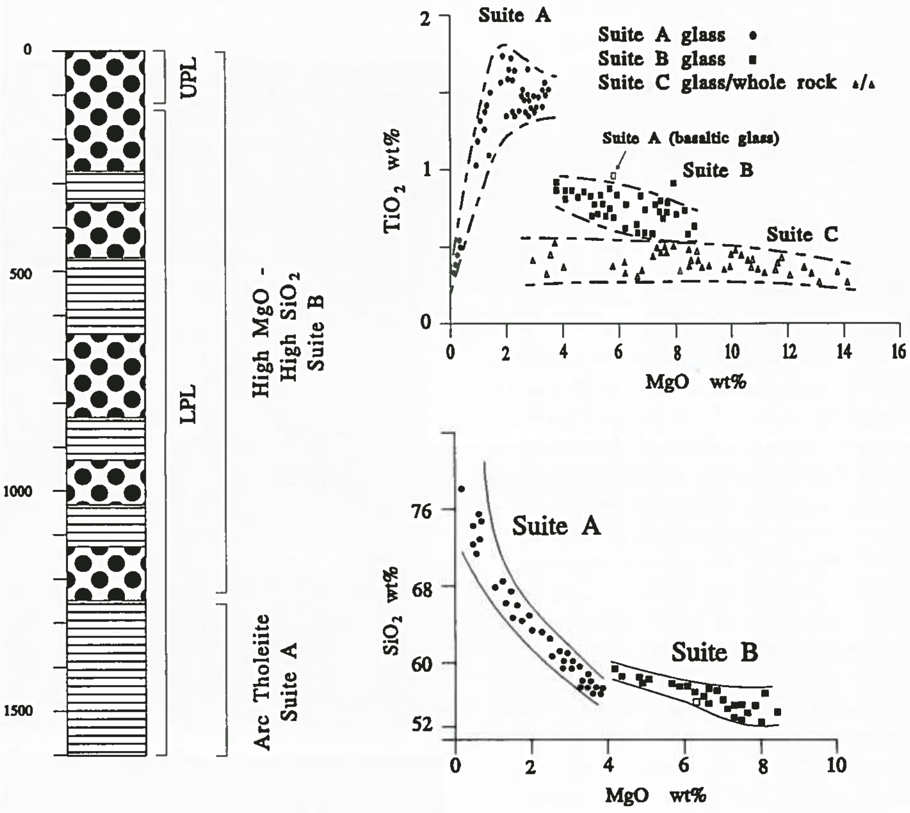

Figure 2. Geochemical stratigraphy of lavas from the Troodos ophiolite, Cyprus. Suite A consists of depleted lavas of island-arc tholeiitic affinity, suite B lavas are more depleted "boninitic" lavas, and suite C is the most depleted Arakapas Fault Zone lavas.

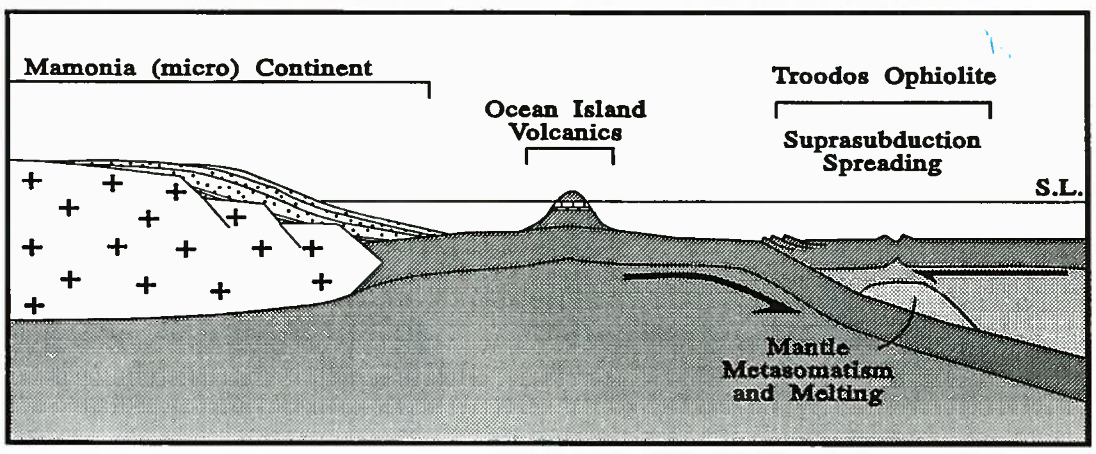

Figure 3. A model depicting the suprasubduction origin of the Troodos ophiolite, Cyprus.

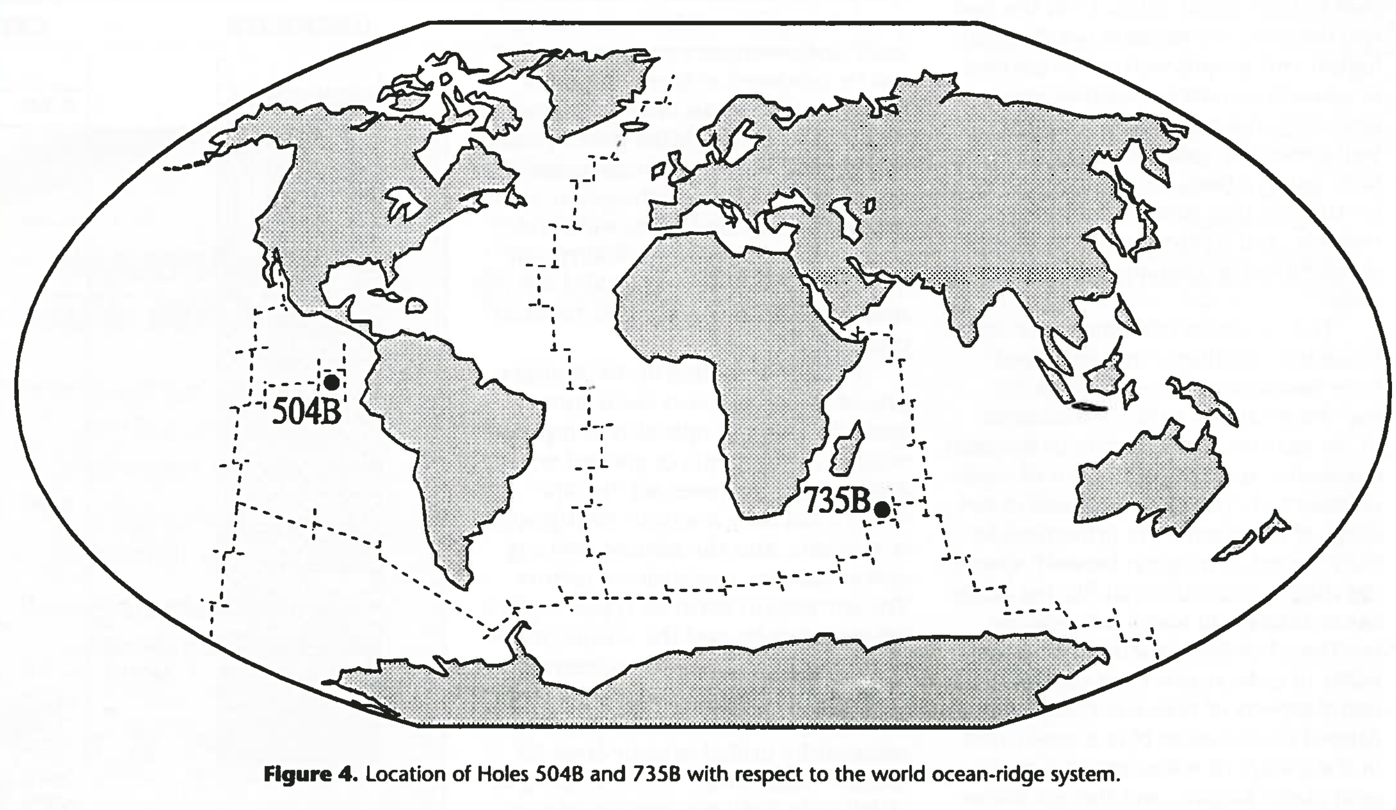

Figure 4. Location of Holes 504B and 735B with respect to the world ocean-ridge system.

INVESTIGATIONS OF THE OCEANIC CRUST BY DEEP SEA DRILLING

In situ igneous ocean crust has been drilled in a great many DSDP and ODP holes. However, in all but a few, the penetration is restricted to the upper few hundred metres of basaltic flows. Two of the more important sites that have been drilled deeper are Hole 504B in the eastern equatorial Pacific Ocean and Hole 735B in the Indian Ocean (Fig. 4).

Hole 504B is the deepest hole into igneous ocean crust. After six legs of drilling (DSDP Legs 69, 70, and 83 and ODP Legs 111, 137, and 139), a total penetration of 2000.4 m below the sea floor has been attained (Fig. 5). One of the initial reasons for drilling on the flanks of the Costa Rica Rift was to examine an area where the ocean crust appears to have reached conductive equilibrium with the young cooling lithosphere, about 5–6 m.y. old.

As of December 1992, Hole 504B extended through 274.5 m of sediment and 1725.9 m into basement. This includes 571.5 m of pillow lavas and minor sheet flows, underlain by a 209 m transition zone of breccia, pillow lava, thin flows, and dikes, and 845.4 m of sheeted dikes. The mineralogy and chemistry of the dikes in the deeper section are nearly identical to those of the overlying lavas and imply crystallization from a magma produced by partial melting of a slightly depleted mantle source. The paucity of glassy, chilled margins deeper in the section and the increase in coarser grain sizes at the bottom of the hole suggest that the sheeted-dike–gabbro boundary lies not far beneath the depth drilled, if the ophiolite model is to be believed. The lower dike section in Hole 504B has been altered as a result of interaction with seawater, and the secondary mineralogy is consistent with the changes that take place toward the base of the sheeted-dike complex in several ophiolites. Many of the lithologic units described from the core at this level contain a variety of gabbroic "clots" that are visible in hand specimen as well as in thin section. Some of these clots contain up to 20% Fe-Ti oxide minerals and are interpreted to be crystallized pockets of trapped Fe-Ti–rich magma. Accompanied by changes in physical properties, including a marked increase in density, these lithologies may herald the transition between layers 2 and 3.

A vertical seismic profile experiment conducted in Hole 504B during Leg 111 records a weak reflector at depths of 1660 to 1860 m beneath sea floor (mbsf). This reflector had earlier been interpreted as the seismic layer 2-3 boundary. However, drilling on Leg 140 penetrated this depth interval, and the reflector is clearly not the transition from dikes to gabbros at Site 504B. It is more likely that the observed changes in intensity of alteration and in physical properties may have caused an impedance difference at about 1750 mbsf which resulted in the observed reflector. Hole 504B has reached the lower part of the sheeted-dike section.

Hole 735B on the southwest Indian Ridge was successful in penetrating unaltered gabbros from presumably the upper part of oceanic layer 3 (Fig. 6). Most of the 500 m section sampled by Hole 735B consists of a single olivine gabbro intrusion that exhibits only minor cryptic variation (Von Herzen, Robinson, et al., 1991). This body appears to have intruded a coarse gabbro-norite at the top of the section and has itself been intruded at the bottom of the section by troctolites and troctolitic gabbros. Numerous crosscutting microgabbros represent melts that migrated through the olivine gabbro prior to its complete solidification. Textural and mineralogical variations in the gabbro section are consistent with its formation in the mush zone that might have surrounded a small crustal magma chamber. The southwest Indian Ridge is spreading at the rate of 0.8 cm/yr, comparable to the slow-spreading Mid-Atlantic Ridge. It is predicted that, where present, the magma chambers underlying such ridges are very small, perhaps less than 2 km wide. Generation of the plutonic rocks sampled at Hole 735B in a small magma chamber might explain their poor layering, limited mixing of magma types, and considerable fractionation. The 735B section has probably sampled the upper parts of oceanic layer 3, although there is no absolute way to ascertain this. Thus, one fundamental problem that remains to be addressed by ocean drilling is the nature of the lowermost oceanic crust.

MAGMA CHAMBER MODELS

Crustal accretion by magmatic means requires the presence of a molten zone or magma chamber(s) beneath the spreading axis. In such chambers the magmas undergo a variety of differentiation processes before their eruption onto the sea floor. In this way, Earth's temperature is controlled, because the magmas provide the energy source driving the hydrothermal circulation that cools the crust.

One outcome of ophiolite research in the past decade is that we can now determine some constraints on the physical nature of the magma chambers that formed these complexes. For example, results from the Cy-4 core of the Cyprus Crustal Study Project show that the layered rocks of this part of the plutonic complex were not produced in a single, large, well-mixed magma body, but from a series of poorly mixed magma pulses injected into a chamber. Additional field evidence demonstrates that this was only one of many independent chambers from which the crust was formed (Fig. 7). This may indeed be the case for the Troodos ophiolite, but studies of other complexes have resulted in a variety of magma-chamber models. In most of these, the magma chamber is viewed as a relatively large molten body that undergoes periodical replenishment, fractionation, emplacement higher in the crustal structure, and eventual eruption. These models seem appealing in that they might be applied to "in situ" ocean crust to explain the relatively simple stratigraphy inferred from seismic studies. However, it is now apparent that they do not fit the most recent findings from the ocean basins, where geophysical results limit the potential size of crustal magma chambers to volumetrically small bodies. For example, seismic data indicate that the crustal magma chamber along the northern part of the East Pacific Rise is less than 1 to 2 km wide and no more than a few hundred metres thick (Sinton and Detrick, 1992). This melt zone is probably surrounded by a mush zone of significantly greater magnitude which is presumably bordered by some form of "cracking front," where cooler solid material is intruded by the melt. Similarly sized bodies likely exist along the southern East Pacific Rise, but at shallower depths below the sea floor. No comparable geophysical data have been collected for the Mid-Atlantic Ridge despite a large number of seismic experiments. Results from seismic reflection and refraction experiments and microearthquake and teleseismic studies appear to rule out the possibility of a large-scale magma chamber, although small, localized bodies could be present.

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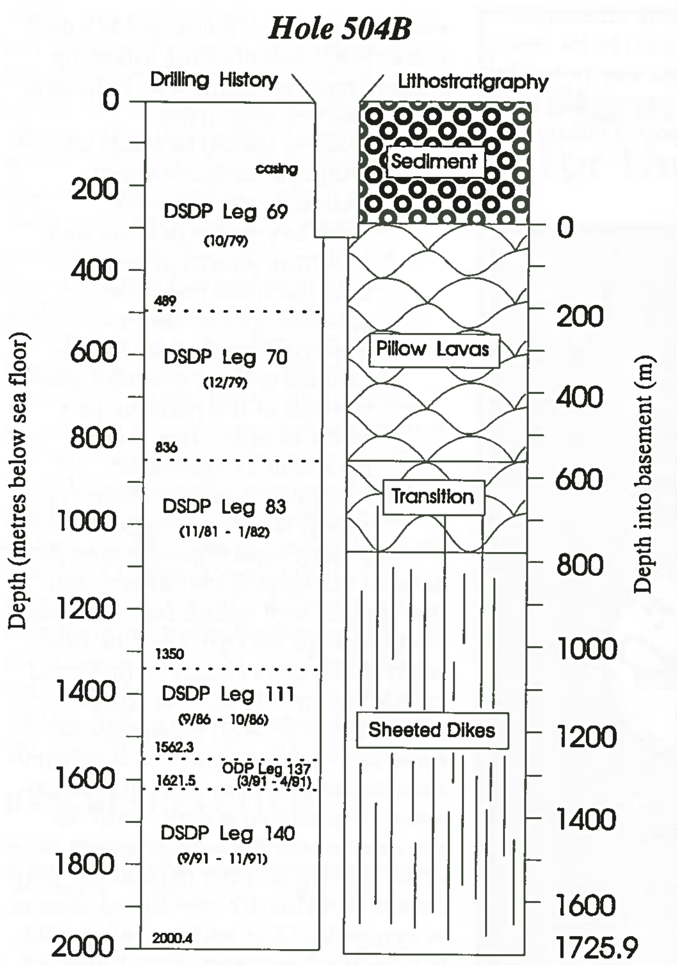

Figure 5. Drilling history and lithologic section of Hole 504B, eastern equatorial Pacific.

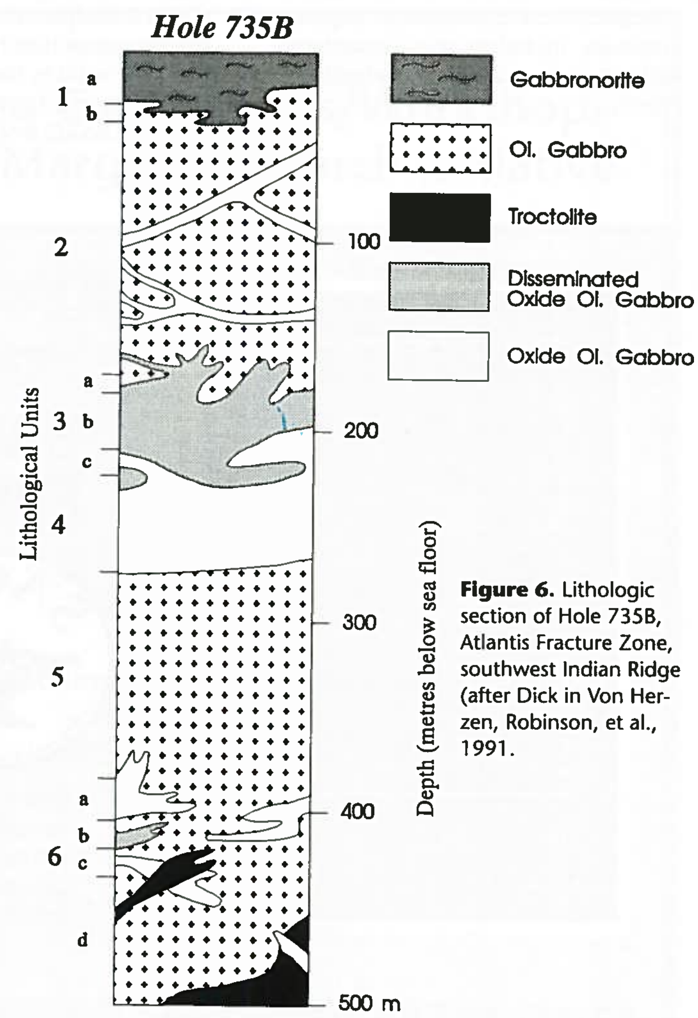

Figure 6. Lithologic section of Hole 735B, Atlantis Fracture Zone, southwest Indian Ridge (after Dick in Von Herzen, Robinson, et al., 1991).

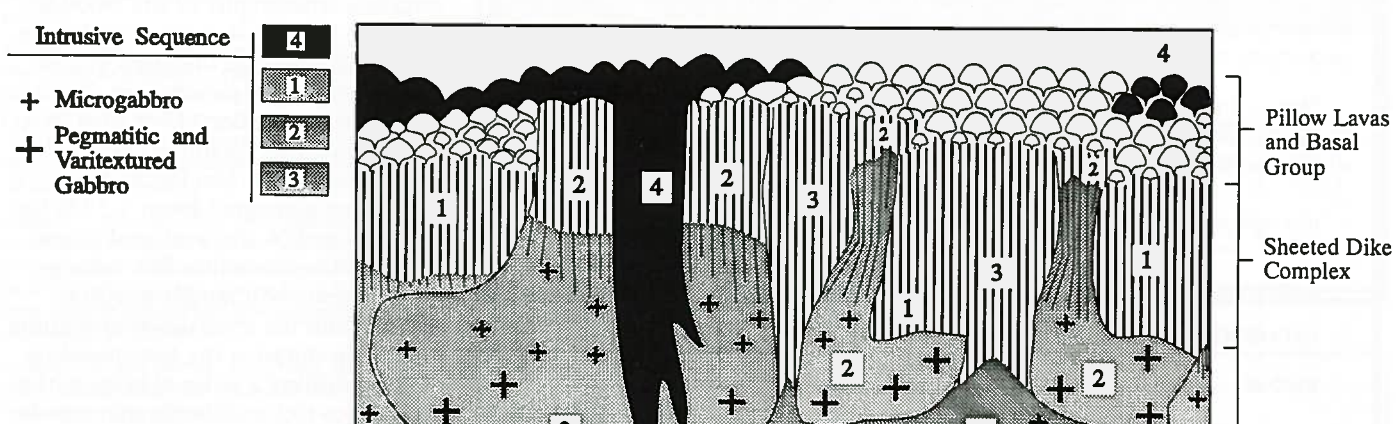

Figure 7. A model depicting evolution of the sheeted-dike complex of the Troodos ophiolite by complex multiple intrusion of high-level magma chambers.

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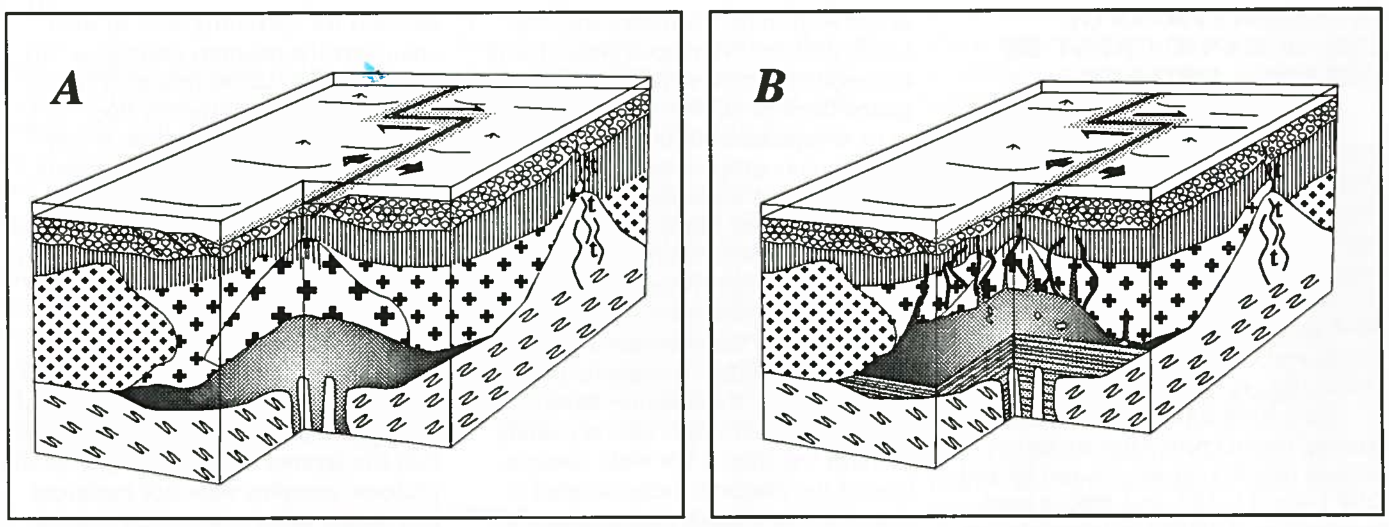
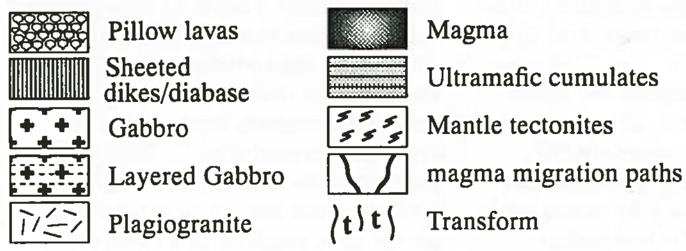
If models gleaned from ophiolite studies are compared, it seems as if those that propose small and ephemeral magma chambers are closest to present ocean-ridge reality (Fig. 8). One important feature of these models is that the older parts of the crust undergo high-temperature deformation during lateral translation. This explains the observations that many ophiolitic—and indeed oceanic—gabbros tend to be strongly deformed. Clearly, the deformation process initially takes effect while they are still crystal mushes but continues to well below the solidus and results in much of the observed "layering" in these rocks. The deformation process appears linked to the laminar-flow process that deforms the immediately underlying mantle lithologies observed in complete ophiolite sequences.





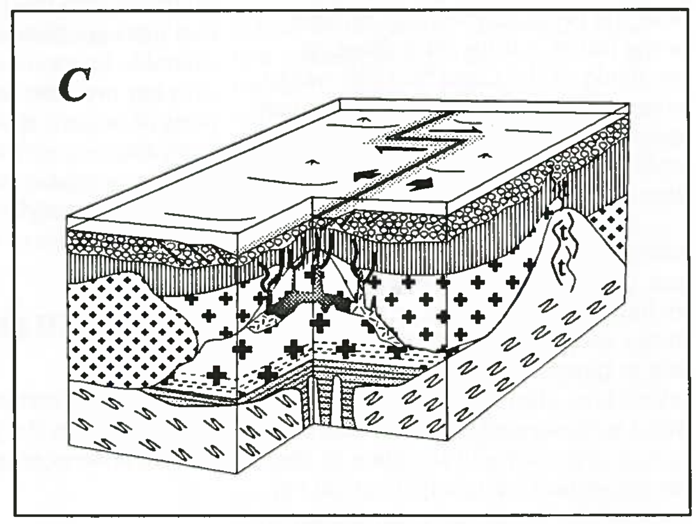


Figure 8. Crustal construction and magmatic spreading process derived from ophiolite studies.

WHY DRILL OCEANIC CRUST AND UPPER MANTLE?

Although the ophiolite analogy has been successful in giving us a first-order picture of the ocean crust and upper mantle, there are problems inherent in carrying it too far. The main one seems to be that, when detailed geology is investigated, there are clearly different models for different ophiolites. Ophiolites also only provide us with ancient sections of the lithosphere that preserve a confusion of sequential processes, including those associated with obduction and postobduction events.

Without ocean drilling, it is unlikely that we will ever be able to determine the average composition of the ocean lithosphere or fully understand the processes that have led to its evolution. So, what is the best way to tackle this problem? A consensus arising from the JOI/USSAC workshop "Drilling the Oceanic Lower Crust and Mantle" (Dick, 1989) describes the following strategy encompassing both long-term and short-term objectives:

1. Progress should be made on drilling a complete crustal section, to the layer 2-3 boundary in the first instance.
2. A drilling priority is to penetrate the transition zones between oceanic layers—most particularly, the Moho.
3. Obtaining long, uninterrupted sections of the plutonic part of the oceanic crust is of paramount importance.
4. Attempts should be made to core a deep hole in the upper mantle.

The short-term objective is to drill as far as the layer 2-3 boundary, and this appears well within reach in Hole 504B, perhaps on ODP Leg 148. Eventually it will be necessary to drill total crustal sections in order to compare crustal structure with seismically defined layers, to determine bulk (altered) compositions, and to infer the composition of unaltered oceanic crust. In the near future, through programs of offset drilling of those parts of the deep ocean crust that are now found close to or exposed on the sea floor, more data will be made available. This drilling of offset partial sections is a strategy similar to that employed by the Cyprus Crustal Study Project to sample the complete stratigraphy of the Troodos ophiolite through a series of drill holes rather than through one deep and extremely expensive penetration. It was employed during Leg 147 at Hess Deep in the equatorial Pacific in late 1992.

At Hess Deep, East Pacific Rise lower crust generated about 1.2 Ma has been exposed by the westward propagation of the Costa Rica Rift. A long-term program will sample a crustal section from the lavas down to shallow mantle produced at the fast-spreading ridge by drilling a series of holes within the various tectonic blocks that expose different crustal and mantle levels. A major target, drilling through the layer 3–mantle boundary, the petrologic Moho, has been one of the longest sought after objectives of deep-sea drilling, and a major aim of the lithosphere community, who wish to characterize the nature of the Mohorovičić discontinuity and to determine the petrologic and structural transition from crust to underlying residual mantle. During Leg 147, this horizon was penetrated in a series of holes that recovered dunites, harzburgites, troctolites, and gabbros. This drilling emphasized the fact that any successful attempt to address the dynamics of crust-mantle relations requires the recovery of oriented, continuous core to establish a kinematic framework. The definition of meso- and micro-scale structures in these rocks will aid in the understanding of the rheological laws that operate at these depths. Measurement of other physical properties, particularly those that produce acoustic impedance, is also a priority, but it will have to be made at pressures equivalent to their place of origin—i.e., in properly equipped high-pressure laboratories.

Another long-term objective is to understand the interaction of fluids and rock in, and close to, areas of crustal accretion. Seawater clearly circulates within the upper parts of the oceanic crust, and ophiolites indicate the likelihood of considerably deeper circulation. The emplacement of magma as a heat source beneath spreading centers provides potential for fluid flow and chemical reaction; indeed, the distribution and interaction of fluid and magma must control the cooling surface of magma chambers. In addition, the vigor of circulation affects the size and depth of magma emplacement. Seawater is modified by continuous reaction with ocean crust, circulates through regions of differing physical and chemical potential, and precipitates secondary minerals in cracks and fractures. This affects the circulation patterns as permeability is reduced and results in a complex interdependency of magmatic and fluid-flow processes.

Although the analysis of vent fluids at active hydrothermal sites has elucidated much of the impact of these processes on the character of the shallow ocean crust, much less is known about high-temperature fluid-rock interactions in the deeper crust and upper mantle, particularly at the actual

Penrose Conference Scheduled

From the Inside and the Outside: Interdisciplinary Perspectives on the History of the Earth Sciences

San Diego, March 19–21, 1994

The goal of this conference is to bring together people who write on the history of the earth sciences to discuss key methodological issues arising out of the different approaches taken in this field. A basic premise of the conference is that a deep schism separates the "insider" and "outsider" perspectives in the history of the earth sciences. The scientist-historian finds it difficult to comprehend how anyone who is not an earth scientist can grasp the history of the discipline without mastering it, or understand the internal dynamics of earth science research without taking part in it. Conversely, the historian of science is inclined to believe that scientists lack an appropriately historical outlook and have not learned how to frame historical questions or use historical materials. Scholars in other fields (e.g., sociology, philosophy) are also critical of earth scientists' efforts at humanistic interpretation of their own discipline.

Although there is some recognition of basic common interests between "insiders" and "outsiders," most observers realize that these constitute two rather distinct communities. The earth scientist–historians write primarily for other scientists, using the language and analytical forms of the sciences, and prefer to publish in scientific journals. Historians, sociologists, and philosophers use assumptions, methods, and terminology that may be unfamiliar to scientists, and they usually publish in their own professional journals. These divisions, while perhaps unavoidable, are currently deeper in the writing of history of the earth sciences than in the writing of the history of physics or of biology. This "insider-outsider" dichotomy is ironic, given that one of the primary purposes of historical investigation is precisely to bridge this gap. One authoritative restatement of this purpose, the National Academy of Sciences publication "On Being a Scientist" (1989), called for renewed attention to historical examination of science as a way of integrating social and personal values with the scientific process.

This Penrose Conference aims to assemble "insiders" and "outsiders" for robust and candid exchange on central issues in advancing historical understanding of the earth sciences. Emphasis will be on such issues as the purposes, methods, and analytical processes of research and presentation in the history of the earth sciences, with participants from both sides gaining insight and appreciation of viewpoints other than their own. Prospective participants should include, of course, not only persons already engaged in research in the history of the earth sciences, but also others thinking of entering the field. Thus, graduate students and persons with recent Ph.D.s in the earth sciences or in history, sociology, philosophy, or other disciplines are particularly invited to apply (some financial support may be available to defray expenses). If you would like to attend, please contact one of the co-conveners and indicate your particular area of interest in the history of the earth sciences. If you have relevant work in progress, please briefly state its nature and scope.

Application Deadline: November 1, 1993.

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margins of magma bodies. One highly visible and challenging target to drill is the "cracking front" that presumably immediately surrounds an advancing magma body. Only by drilling in-situ crust at the spreading axis can the important processes that occur at this interface be investigated while they operate. Ophiolites don't help us here!

THE TECHNOLOGICAL CHALLENGE

Deep-sea drilling has proven to date to be a most successful combination of science and engineering disciplines. In planning a science program involving deep-crustal drilling, it is clear that there must be concomitant engineering developments. If full crustal penetration to the mantle is to be achieved, a drill string at least 11.5 km long will be required. The drill string now on the *JOIDES Resolution* is only long enough to reach layer 3 in comparatively young crust, but not much more. In addition, although the diamond coring system under development is unlikely to aid significantly in the drilling of holes to the mantle, it will be of inestimable value in drilling young, fractured and unconsolidated volcanic rocks at ridge crests. We know from our experience at Hole 735B that conventional rotary coring is very effective in older layer 3 gabbros. Thus, it seems likely that a targeted hole to the mantle is best sited in older and altered crust where the available technology needs the least modification to be successful. Lengthening of the core-retrieval wire will be necessary, and reentry cones will have to be sturdy enough to withstand up to 100 reentries. However, these are not major modifications to equipment, in light of the scientific objectives and potential results. The major concern is the time that will be required to accomplish such drilling and how it might be accommodated into a global drilling program; for example, it is estimated that approximately seven 60-day legs would be required to successfully drill one rotary-cored hole to mantle—more than one year of drilling (Natland et al., 1989). It is in this light that the geological community must ask, Will such a hole be worth it scientifically? Of course, the answer must be weighed against other lithosphere targets for the future. Although the offset partial section strategy provides a means of directly coring plutonic and ultramafic rocks and providing information as to their drillability as well as immediate geologic and geophysical data, to date there has been a clear message from the lithosphere community that the concept of full crustal penetration should be pursued as a long-term objective and that engineering development to allow such drilling must be supported. The immensity of the task may necessitate drilling under different conditions than now exist in the Ocean Drilling Program—i.e., with a fully dedicated alternate platform to the *Resolution*, but there can be no doubt that the potential results from this deep lithosphere drilling will prove a major landmark in our understanding of Earth systems.

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Manuscript received September 7, 1992; revision received December 7, 1992; accepted December 12, 1992 ■

Correction:

The phone number for the Geological Society of London was incorrectly published on the ad appearing in the January issue of *GSA Today*. The correct number is (0225) 445046. We regret any inconvenience this error may have caused.

FORUM

Bruce F. Molnia

Forum is a regular feature of *GSA Today* in which many sides of an issue or question of interest to the geological community are explored. Each Forum presentation consists of an informative, neutral introduction to the month's topic followed by two or more opposing views concerning the Forum topic. Selection of future Forum topics and participants is the responsibility of the Forum Editor. Suggestions for future Forum topics are welcome and should be sent to: Bruce F. Molnia, Forum Editor, U.S. Geological Survey, 917 National Center, Reston, VA 22092, (703) 648-4120, fax 703-648-4227.

ISSUE: Geoscience Literature Pricing

The cost of acquiring geoscience literature in the United States has risen substantially during the past 20 years: journal prices for libraries have more than doubled since 1987, and book prices have increased more than 50%. The rate of increase exceeds both the basic inflation rate, as measured by the U.S. Consumer Price Index, and library funding during this period. Simply put, the price of geoscience books and journals has been rising much faster than the ability of libraries to purchase them. This situation is not unique to the geosciences, but reflects general trends in scientific publishing. Such substantial increases in publication costs are attributed to several factors: changes in dollar valuation against key European currencies; basic inflation; cost of technological change in the publishing industry; growth in publication size, especially of journals; consolidation of scientific publishing; demand for greater profit or operating margins; smaller journal circulation; smaller press runs for books; and geographical price differentiation by publishers.

This Forum provides data on geoscience literature prices and dollar value during the period 1987 through 1992 and presents perspectives from a commercial publisher, a society publisher, geoscientists, and libraries. The Forum was put together by members of the Collection Development Issues Committee of the Geoscience Information Society; the authors are responsible for all views expressed.

Forum Editor's Note: This, the first half of a two-part Forum, contains the academic library community's perspective on the geoscience literature pricing issue. The issue of April *GSA Today* completes this in-depth look at this controversial topic and presents five additionally stimulating perspectives.

PERSPECTIVE I: An Introduction to Geoscience Literature Pricing

Michael Noga, Geology/Geophysics Library, UCLA, Los Angeles and Steve Hiller, Science Libraries, University of Washington, Seattle (Chair, Collection Development Issues Committee, Geoscience Information Society)

Pricing data were analyzed for books for the period 1987 to 1992 and for journals for the period 1987 to 1993. The periods of time covered are not entirely comparable, because journal pricing data cover a calendar year whereas book pricing data are drawn from fiscal year information. Prices for 1993 journal subscriptions are included; this tends to overstate price increases for journals during this period as compared to books, which have been priced only through June 1992.

Geoscience Journal Prices. The pricing history for 178 titles representing a core collection of geoscience journals in the UCLA Geology/Geophysics Library was evaluated. These titles do not compose a comprehensive list, but one that is representative of a large geoscience library (Table 1). Prices increased an average of 117% between 1987 and 1993; the cost of foreign titles rose 50% more than U.S. titles. The average price of a foreign commercial title in 1993 is $864, an increase of more than 150% over the seven-year period (Fig. 1). The largest percentage increase for foreign publications occurred between 1992 and 1993 (Fig. 2).

No single factor accounts for these substantial price rises. Certainly many journals have increased in size and frequency to accommodate the growing onslaught of papers, yet there is no consistent connection with the magnitude of the price change. For example, the price of both *Computers and Geosciences* and *Physics of the Earth and Planetary Interiors* tripled between 1987 and 1991, but the number of pages in the former more than doubled, whereas the latter saw a slight decrease. Similarly, among scholarly societies, the price of the *Journal of Geophysical Research* (JGR) rose 81% with a 49% increase in the number of pages, while the price of the *GSA Bulletin* went up 67%, with only a 15% increase in content. There is, however, a clear difference in price between journals published by U.S. societies and those by foreign companies, even when journal size is considered. In 1991, annual subscriptions to JGR, the *GSA Bulletin*, and the *AAPG Bulletin* cost less than 10 cents per page, whereas foreign commercial publications generally cost 30 to 80 cents per page (Table 2).

TABLE 1. GEOSCIENCE JOURNAL PRICES 1987–1993

Type of publisher*	Average price (U.S. dollars) 1987	1988	1989	1990	1991	1992	1993	Change (%)
Commercial (52)	291	340	369	429	533	557	721	
U.S. (12)	122	127	144	150	200	208	246	103
Foreign (40)	341	404	436	513	633	662	864	153
Society (98)	139	156	166	182	215	241	265	
U.S. (46)	140	160	169	192	214	246	255	83
Foreign (52)	138	152	163	173	216	237	273	98
Other† (28)	106	123	129	141	151	169	188	
U.S. (11)	35	36	37	39	41	45	45	30
Foreign (17)	151	179	189	207	222	249	280	85
Total U.S. (69)	121	137	146	162	186	210	223	84
Total foreign (109)	215	249	267	303	370	395	491	129
Total (178)	178	204	219	248	298	322	386	117

* Number of journals in parentheses.
† Includes government, university, and institution publishers.

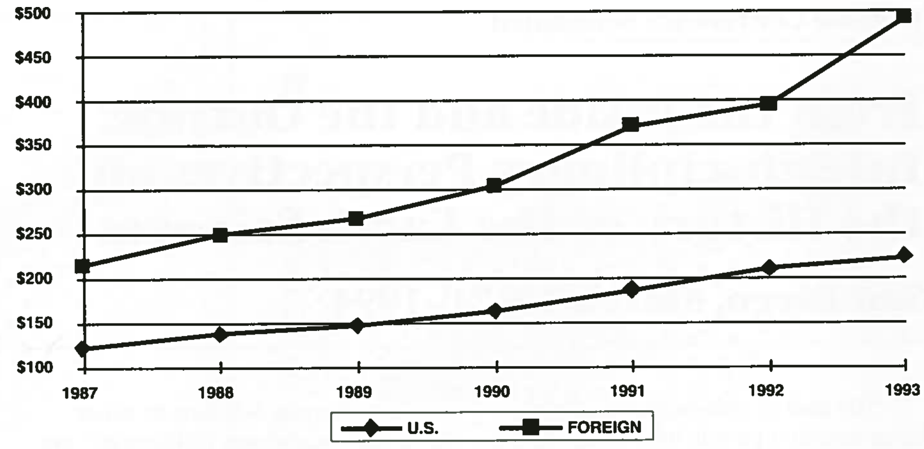

Figure 1. Average journal price, 1987–1993.

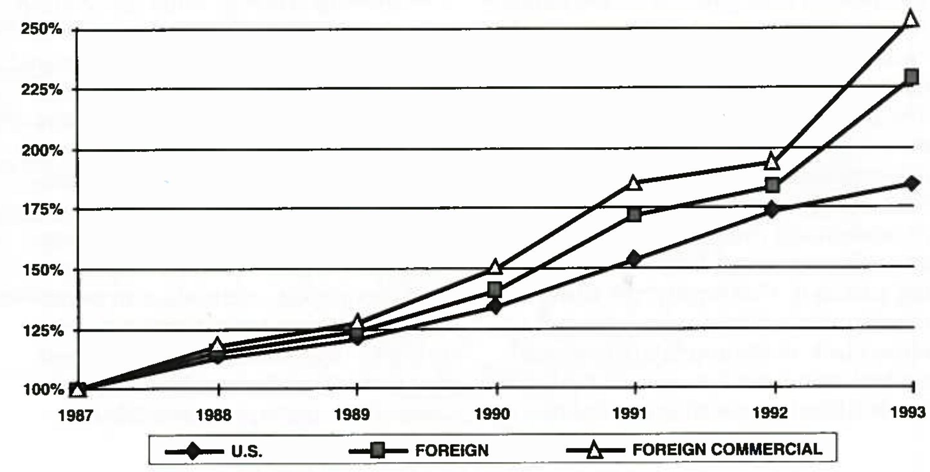

Figure 2. Journal price changes, 1987–1993; 1987 = 100.

TABLE 2. AVERAGE PRICES (U.S. DOLLARS) FOR GEOSCIENCE BOOKS 1986–1987 TO 1991–1992

	U.S.	U.K.	Europe	All	Published in U.S. (%)
1986–1987	40.49	58.38	75.14	54.54	44.5
1987–1988	48.13	73.49	83.24	65.69	41.2
1988–1989	43.03	82.71	93.72	68.61	42.8
1989–1990	58.04	87.64	98.70	78.97	42.0
1990–1991	57.65	87.05	117.46	83.11	41.1
1991–1992	53.12	98.11	122.99	83.63	45.7
Change (%)	31.2	68.1	63.7	53.3	43.5
U.S. CPI	27.8%				

Geoscience Book Prices. The average price of a geoscience book increased 53.3% during the period July 1986 through June 1992. However, the rate of increase is substantially different for items published in the United States and those published in Europe. The average price of a geoscience publication in the United States and Canada increased only 31.2% during this period (only slightly higher than the U.S. rate of inflation), whereas the increase in the average price of foreign books exceeded 65%. Pricing information was taken from 2013 trade books published by commercial, society, and institutional publishers, covering the earth and atmospheric sciences which were part of the Blackwell North America Approval Program from July 1986 through June 1992. Although the approval program covers a wide variety of publications, it does not include most government publications and small-press books. U.S.-published books accounted for 43.5% of the total, with the remainder evenly split between United Kingdom and European publishers. Nearly all European items were commercially published, many of them in series (Fig. 3).

The average price of a geoscience book is now more than $83, and in such fields as mineralogy, petrology, geophysics, geochemistry, structural geology, and stratigraphic geology, it exceeds $100, putting many books beyond the reach of both individuals and libraries. Nevertheless, the rate of increase for books is substantially less than for serials. One contributing factor to higher prices has been smaller press runs for scholarly monographs. University presses that once published 2000 copies now have press runs as low as 500.

Foreign Currency Value per U.S. Dollar 1987–1992. The declining value of the U.S. dollar is often cited as a major factor for price increases of European publications. Although the value of the dollar fell sharply against the currencies of those countries most involved in scientific publication (Germany, Netherlands, U.K.) between 1985 and 1987, it has been relatively stable since that time, with annual fluctuations of up to 10% per year. The average value of the Dutch guilder and German mark were about 10% higher in 1992 than in 1987; the 1992 value of the British pound changed little from the 1987 value. While some price increases can be attributed to changes in dollar value, especially for German and Dutch publications, it has not been the most significant factor, as Figure 4 shows.

PERSPECTIVE II: Pricing—the Libraries' Perspective

Steve Hiller, Science Libraries, University of Washington, Seattle

Libraries have been hit hard by a combination of rising prices and recessionary budgets during the past five years. While most libraries have shifted a larger proportion of their budget to fund literature acquisition, especially serials, it hasn't been enough. Nearly all libraries have canceled a significant number of serial titles, while book budgets remain generally flat.

Each summer during the past six or seven years, libraries have raised a collective shudder when publishers, especially foreign publishers, set journal prices for the coming year. The summer of 1992 was the worst in many years. The dollar had sunk to near-historic lows against some European currencies, and the average price of European commercial journals rose by 30%. The recession of the past few years has meant that annual budget increases have been small at best, and institutions in the most economically depressed regions have had actual declines. The largest North American academic research libraries, members of the Association of Research Libraries (ARL) raised expenditures for serials by 70% from 1985–1986 through 1990–1991, approximately 50% greater than the increase for other expenditures. As Figure 5 shows, even this increase wasn't sufficient, because the average cost of serial titles held by these libraries increased by 72%. The average cost of books during this period rose 47%, but library expenditures rose by only 25% (Fig. 5). The net result: the number of serial titles purchased declined by 2%, and 15% fewer books were bought. The proportion of money spent on serials compared to books increased from 55.3% in 1985–1986 to 63.3% in 1990–1991. Budgets at many institutions maintained pace with price increases for several years, but the cumulative impact of annual increases greater than 10% and recessionary budgets has meant a substantial loss of purchasing power. Many libraries find they are spending a larger proportion of their budgets on a relatively small number of European commercial publications.

The situation in geoscience libraries has followed a similar pattern. Journal subscriptions have been canceled at all major academic research libraries. During the past five years more than 30 libraries have reported canceling 1750 subscriptions to geoscience journals. This figure does not include cancellation of second or multiple copies of a title at a single institution. These are the very libraries that other libraries and geoscientists depend on for material that cannot be found in local collections. Thus, journal cancellations at one library may affect geoscientists everywhere. Initial cancellations tend to focus on expensive translation journals and long-running European geologic titles. These are high-cost and relatively low use items and tend to cover such subjects as petroleum, coal and energy geology, paleontology, and physical geography, although these varied according to programs at each institution.

However, recent cancellation projects have begun to cut into mainline English-language journals. At some institutions, the fat is long gone, and there's little left but bones. During the past two years, the University of Kansas has canceled earth science serial titles equivalent to ⅓ of the serials budget. Cuts of that magnitude concentrated on expensive European publications. The University of Illinois at Urbana-Champaign canceled more than 150 titles during the past year. At the University of Washington, the budget for science serials doubled between 1987 and 1992, but average annual increases of 12% were insufficient to cover price rises. During a 15-month period in 1991–1992, three cancellations took place—more than $10,000 in geoscience titles.

While the problems at academic libraries have been the most dramatic, because their budgets are much larger, almost all types of libraries have been affected. The Geological Survey of Canada canceled nearly 60 titles in 1992, and the U.S. Geological Survey has canceled several titles and in many instances holds just one copy to serve all of its branch libraries. Economic troubles in the oil industry have forced many petroleum libraries to reduce significantly on-site journal holdings.

At the same time, budgets for the purchase of books have remained flat or declined. It is much easier for libraries not to buy a book than to cancel a journal. At the California Institute of Technology Geology Library, a comparatively well funded collection, expenditures for geoscience books increased by 13.4% and serials by 88.9% between 1985–1986 and 1991–1992. A book budget that was "generous" seven years ago is barely adequate today. Even the significant increase in serials funding was not sufficient to prevent an 8% cut in the serials budget last year. The proportion of the geology materials budget spent on books at the University of Illinois now hovers at a little more than 10%, half the percentage of three years ago. Libraries increasingly report difficulties acquiring such items as guidebooks, proceedings, and other types of books due to insufficient funding. Some institutions report that the practice of feeding serial price increases and starving book purchases works against

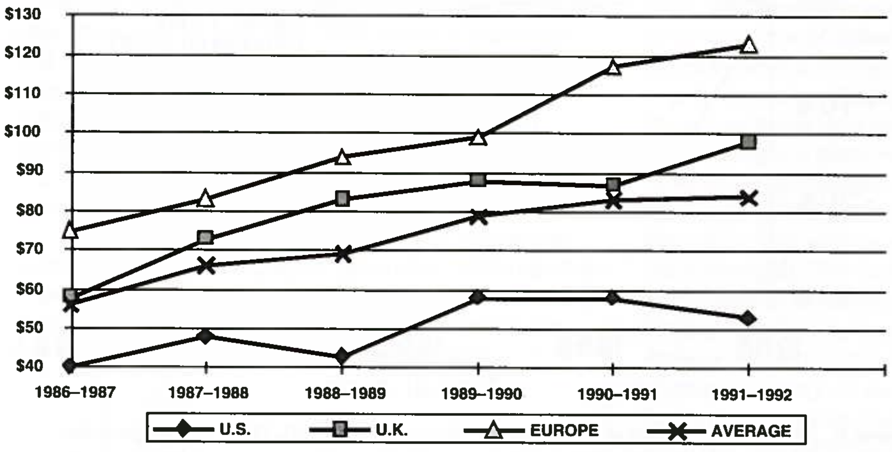

Figure 3. Book prices in dollars, 1986–1987 to 1991–1992.

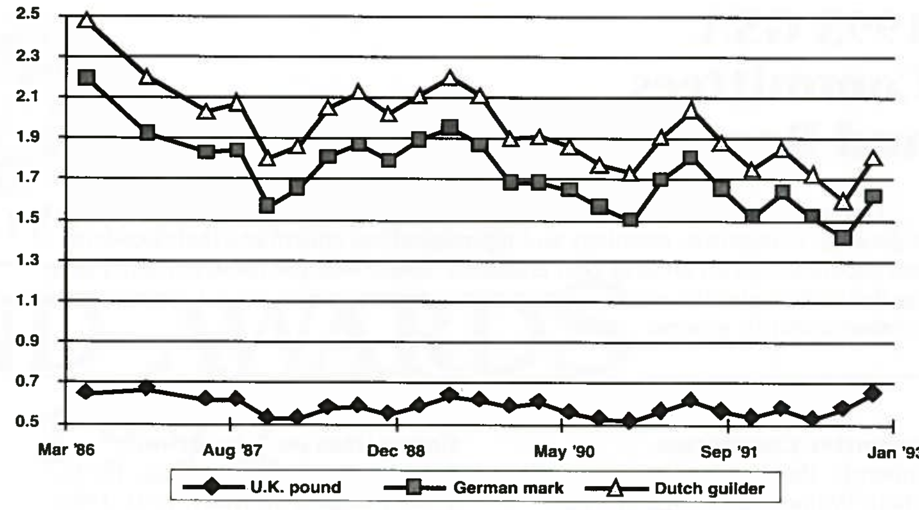

Figure 4. Foreign currency units per dollar, 1987–1992.

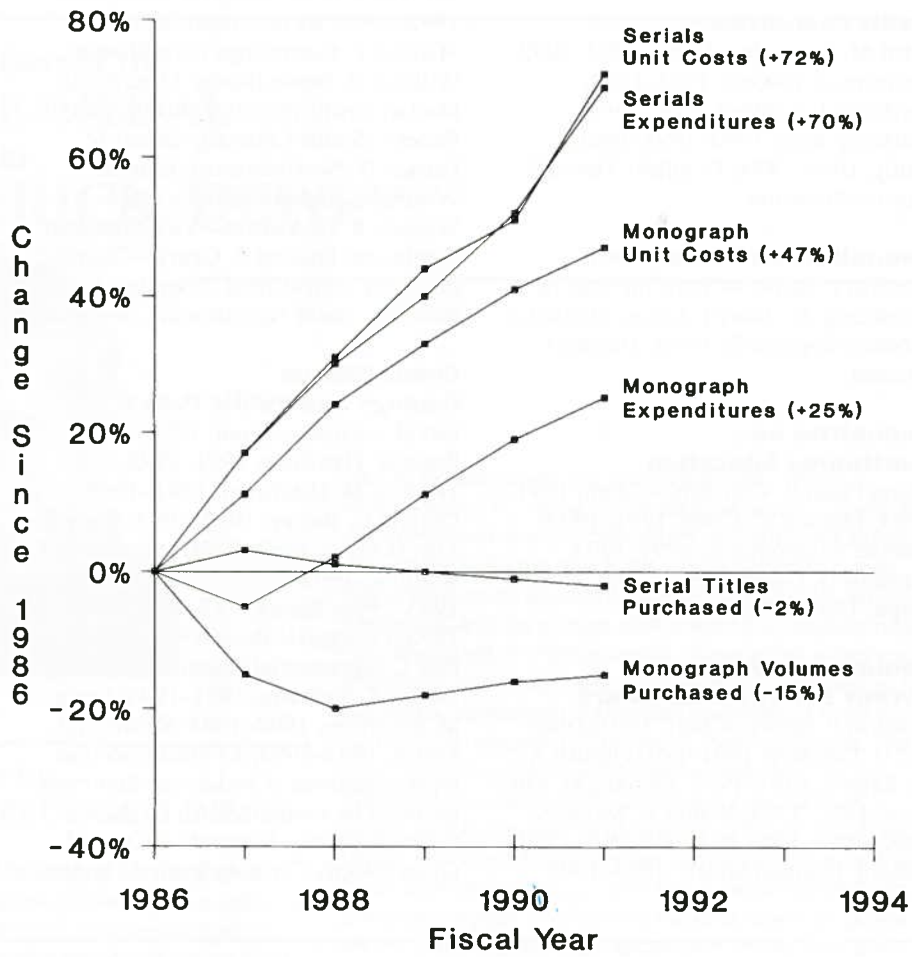

Figure 5. Monograph and serial costs for Association of Research Libraries members, 1985–1986 to 1990–1991. Copyright 1992, Association of Research Libraries.

undergraduate students, who can more readily understand and relate to review information contained in books. Another problem: many titles that libraries used to receive free on exchange or at low cost from government geological surveys are no longer free, or have become more expensive.

Are these canceled serial titles being missed, or are they marginal titles whose demise is unlamented, except by some publishers? Libraries have reinstated few of the titles canceled during the past five years, although many continue to purchase, albeit selectively, new serial titles. At the University of Iowa, a program was instituted in 1989 which provides the table of contents for nine canceled journals to geology library users, who can request articles delivered at no cost to them. Only 10 to 15 articles a year are requested under this program; the average annual cost is $300 (including the table of contents), a drop in the bucket compared to the $10,000 annual savings from cancellation. The University of Utah canceled 36 geoscience titles last year and will employ a table of contents–document delivery service for some of these titles, similar to the Iowa program. A growing number of institutions are providing electronic access to commercially produced table of contents services and facilitating fast delivery of articles to users, regardless of whether the journal is housed locally. Fortunately, most large libraries have not yet had to face the prospect of canceling core titles, but if the trends of double-digit price increases for European titles and insufficient budgets continue, this unwelcome prospect will soon be reached by many collections.

Librarians have also initiated more sophisticated ways of evaluating material in the collection. For example, journal use studies were conducted for the years 1988–1989 at Stanford and UCLA. These studies counted how often a journal was used, determined the journals in which faculty published, and noted which journals were cited in graduate student theses (Stanford only). The study revealed that English-language journals published by societies received the greatest use and were the most cited. Indeed, the ten most frequently cited journals were all published by scholarly societies. Other studies have combined use with cost to come up with a "value" index for titles. Some libraries are working together to ensure that at least one institution purchases a title that can be used by others. For example, University of

Forum *continued on p. 60*

1993 GSA Committees and Representatives

Committees are the key to GSA's accomplishments in promoting the science of geology. Committee members and representatives contribute their expertise and experience to all areas of GSA endeavor. Listed here are those currently serving the Society and the science as committee members and as GSA representatives to other scientific groups.

Executive Committee
Robert D. Hatcher, Jr.—President and Chair; William R. Dickinson—Vice-President; E-an Zen—Past President; David E. Dunn—Treasurer; John M. Sharp, Jr.—Council Member-at-Large

Audit Committee
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Committee on Continuing Education
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Committee on the Arthur L. Day Medal Award
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Committee on Education
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Committee on Geology and Public Policy
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Committee on Membership
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Committee on the Penrose Medal Award
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Program Committee
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Committee on Research Grants
Howard W. Day—Chair, 1991–1993; Molly Fritz Miller, 1991–1993; Peter C. Patton, 1993; Raymond V. Ingersoll,

GSA Committees *continued on p. 77*

Forum *continued from p. 59*

California libraries are coordinating purchases of conference proceedings and expensive translation journals in this manner.

Libraries face the unusual prospect of facilitating expanded access to the literature while at the same time reducing the number of titles acquired. Many academic and agency libraries now subscribe to *GeoRef on CD-ROM* (a comprehensive database of the geoscience literature produced by the American Geological Institute), and users are able to search directly. User frustration occurs when they take their comprehensive list of citations and try to find material in their library. The substantial rise in interlibrary loan statistics (47% in a six-year period, as shown in Fig. 6) is probably due as much to improvements in computerized access to literature as it is to the purchase of fewer books or journals.

No one denies that there is a problem and it is growing worse as the gap between prices and library funding widens. While it is difficult (and sometimes foolhardy) to predict the future, I believe that the following steps will be necessary to bring this situation under control.

1. Libraries will undertake more sophisticated analyses of collection use and tie these studies to cost. Costs will include not only the price of the item, but also processing costs (acquisition, cataloging, binding) and the space necessary to house material.

2. Libraries will continue to cancel journals and add books on the basis of institutional need and resources. Librarians will work closely with geoscientists to evaluate existing holdings and analyze how they fit into the research and instructional processes.

3. The traditional research library philosophy of building comprehensive on-site collections will change. Research libraries will continue to maintain collections of core material but will rely on other sources for just-in-time delivery of more specialized or peripheral material. Vendors and publishers will continue to develop delivery systems that address this need, and library consortiums will share specialized items between institutions.

4. Electronic access to and delivery of information will continue to expand as more material is available in digital format and networks provide the capability of delivering information, including graphics, quickly and inexpensively. More specialized information may be "published" only in electronic format. Electronic access to the literature will be fast and easy, and there will be several convenient options for information delivery.

5. The "publish or perish" syndrome must be changed or the system of scholarly communication as now constituted will collapse. There are too many manuscripts chasing too many publications at too high a cost. A Gresham's law of scientific publishing is at work which threatens the viability of scientific communication. Librarians and geoscientists must work together to ensure that information is provided to the widest possible audience at the lowest cost. This will undoubtedly mean the demise of some journals and the emergence of new forms, predominantly electronic, to disseminate knowledge in the field. ■

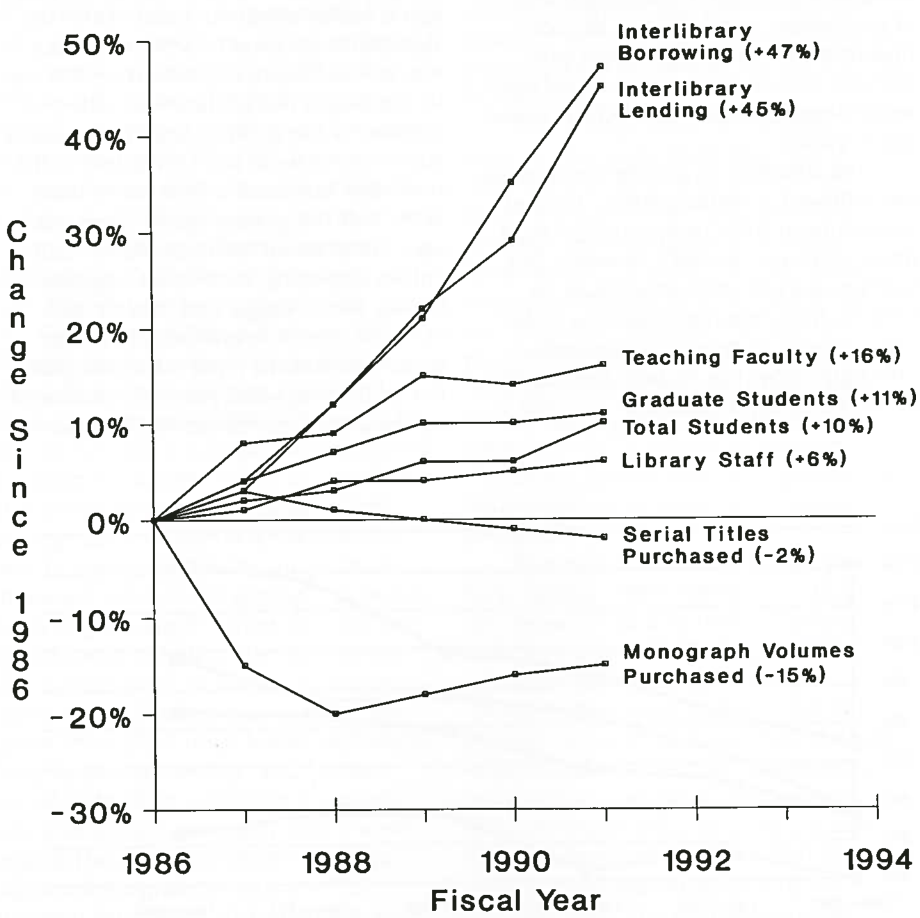
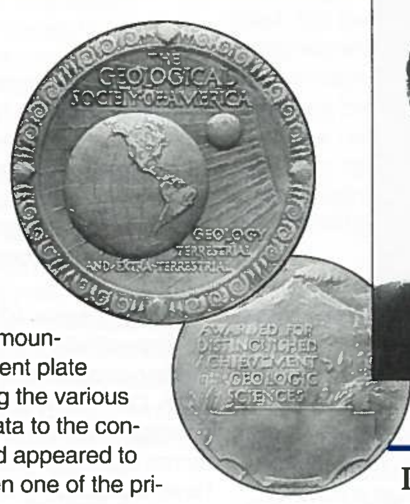


Figure 6. Supply and demand in ARL libraries, 1985–1986 to 1990–1991. Copyright 1992, Association of Research Libraries. This graph compares growth in numbers of users with changes in on-site resources and resource sharing.

GEOLOGICAL SOCIETY OF AMERICA

MEDALS AND AWARDS FOR 1992

Presentation of the PENROSE MEDAL to JOHN FREDERICK DEWEY



Citation by WALTER C. PITMAN III

When the hypothesis of sea-floor spreading was proposed, it was suggested that continental accretion and mountain building took place at the convergent plate boundaries. But the problem of relating the various complex geologic observations and data to the convergence process was unresolved and appeared to be monumental. John Dewey has been one of the primary movers in placing the problem of these geologic processes on a firm rational basis within the framework of plate tectonics. The variety, scope, and importance of his work in this regard are amply illustrated by the breadth of his publications.

His earliest work on the subject included papers dealing with the problem of converting an Atlantic-type margin to an Andean margin; the general problem of mountain building as related to the Wilson cycle; and relating the formation of the Appalachians and Caledonides to the convergence of the circum-Atlantic continents. The most important contribution of these papers was in developing a methodology that, in defining criteria or critical geologic indicators, would enable the unraveling of the history of a mountain belt within a plate-tectonic framework. This methodology was applied by Dewey and others, in a series of papers, to the deformation belt that lies between the African, Arabian, and Indian plates and Eurasia. In this work the importance of microplates at convergent boundaries and back-arc basins formed during the convergence process was realized. Dewey has recognized the importance of understanding plate phenomena that occur at nonconverging plate boundaries, such as rifting, the complexities of transform faulting, and hot-spot activity as manifestations that may eventually be swept up into mountain belts and are therefore key to understanding tectonic processes. He has made important contributions to our understanding of the processes of ophiolite obduction. He and his colleagues recognized three necessary conditions for ophiolite obduction: first, that the continental edge must have been thinned and, second, depressed, and third, that the ophiolite must be young. These criteria were recognized by Dewey and his colleagues in their study of the Bay of Islands complex and have been applied to mountain belts, both ancient and modern, first in recognizing that these are suture zones and then in unraveling the geologic history.

By any standard John Dewey must be recognized as one of the most imaginative, productive, and important geologists of our time. His ideas constitute some of the most significant contributions to the modern revolution that has taken place in the geological sciences.

Response by JOHN FREDERICK DEWEY

To be named Penrose Medalist by the Geological Society of America is the highest accolade that a field-based generalist can receive, and I am overwhelmed. I am particularly pleased to receive the Medal during E-an Zen's presidency, to have closely followed Bill Dickinson and Warren Hamilton in the award, and to have my close friend and deeply respected scientific colleague, Walter Pitman, as my citationist. The United States has had a major, perhaps critical, influence in my geological life. In 1964, Art Boucot invited me to join a small team remapping the Arisaig area of Nova Scotia, and that summer I also joined Marshall Kay in Newfoundland. To a young field structural geologist working in the Caledonides of western Ireland, this was a superb opportunity to broaden my interests to a regional tectonic scale. During the next few years, Bob Jastrow's Goddard Conference, Marshall Kay's Gander Conference, the Halifax Geological Association of Canada meeting and a developing field program in Newfoundland convinced me that continuity existed from the Appalachian to the Caledonian orogen and, moreover, that one cannot understand the particular (small-scale) without the analysis, synthesis, and compilation of the general (large-scale), and vice versa. Consequently, I jumped at Chuck Drake's offer of a sabbatical leave at Lamont in 1967, which I spent putting together a synthesis of the Appalachian-Caledonian orogen. This was the critical year in which the mobilist ideas of Hess, Dietz, and Tuzo Wilson were pulled together with the seismicity work of Sykes and others, and the magnetic anomaly work of Pitman and others into a coherent theory of plate tectonics; the plate-tectonic "converting" vision forced me to the view that tectonic geology can be explained only in a plate-tectonic framework. Bill Dickinson's 1969 Asilomar Penrose Conference brought together a number of geologists, including Warren Hamilton and Clark Burchfiel, who were developing similar ideas and methodologies. This was a truly wonderful, mind-blowing conference at which people stayed up all night arguing, forging many strong friendships, and at which Tanya Atwater showed us the rigorous quantitative kinematics of plate-boundary evolution as a basis for understanding the early geologic evolution of whole mountain belts. Now, the pull of the United States was becoming inexorably stronger, and I found myself unable to refuse Jack Bird's offer to join an expanding group at SUNY—Albany, where I spent 12 happy and productive years of geological excitement with superb colleagues and graduate students.

These were the heady days of almost exclusively responsive-mode academic science. Since then, an anti-intellectual and dangerous shift toward the dirigiste mode has developed in which scientific bureaucrats aided and abetted by some scientists are designing large research programs, abstracting substantial quantities of cash into, and inviting research proposals in, these programs. The notion that prognoses and sensible decisions can be made not only about which areas are important but, worse, which are likely to generate significant results, is banal. There is only one proper way to dispense cash to basic academic research, and that is for unsolicited proposals to be peer reviewed by the best scientists purely on the basis of intellectual excellence. In this way, the random walk of basic science is preserved by the best young researchers deciding what they wish to do, not what bureaucrats and older scientists tell them to do. Our whole system should be geared toward trusting and supporting clever young people with their own ideas rather than in designing projects that constrain and shackle them.

Closely allied with this problem is the corrosive "need" for funding agencies to constantly redefine goals and missions, inevitably distorting basic research to applied goals and the marketplace. When will bureaucrats recognize that basic research cannot be directed and that the small proportion of research that leads to breakthroughs, paradigm changes, and results of great societal and economic importance cannot be predicted.

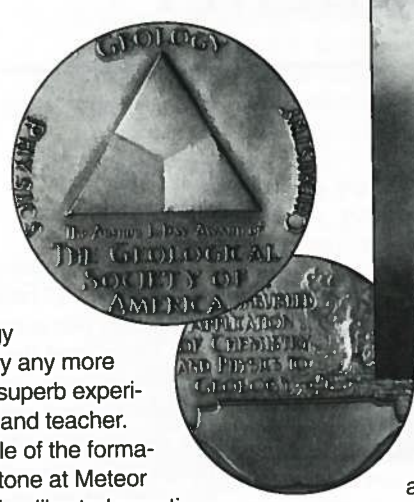
Another increasing tension in geology has been the progressive relegation of field geology to a diminishing role of the euphemism "data collection" and as "ground truth" for remote sensing and modeling. Numerical modeling and laboratory experiment are very important ingredients in geology, but the truth resides, ultimately, in the field; the problem is how to recognize and extract it. As Francis Pettijohn said, "nothing is as sobering as an outcrop." Geology is a massively complex eclectic and lateral-thinking science and needs everything including speculation, numerical modeling, experiment, meticulous field work and mapping and, above all, thinking at all scales from the crystal lattice to the whole Earth. We must let our minds wander away from conventional wisdom and constantly renew our thinking. Paradigmatic fashion in science is a useful framework upon which to hang our research, but beware closing our minds to unconventional notions, especially the catastrophic nature of the geologic record at all scales from the K-T boundary event to ice and rock avalanches and Andean erosion. Tragically, fashion and respectability are transient and fickle; the K-T boundary has polarized not only into true believers and opponents but into Chixcolub as a touchstone of respectability. By all means, let us expand our minds into the remotest concepts, but let us not believe in ultimate truths and proofs in science any more than in the arts; things either work or don't work, and are not right or wrong.

Another problem from which we suffer increasingly is the literature explosion, part of which is a genuine necessity of rapidly advancing data and ideas of science and the increasing number of people engaged in scientific research. Unfortunately, a very large part results from intense competition in which there is much careless rediscovery of the wheel (commonly in elliptical or rectangular form) because of declining standards of scholarship in knowing and citing the literature. The problem is compounded and encouraged by the absurd proliferation of scientific journals over the past 20 years. Increasing numbers in the science have led also to various forms of outcrop vandalism, partly by hordes of uncontrolled students and partly in the name of research where classic outcrops with their weathering patinas have been destroyed (for example, multiple drilling of the K-T boundary in Woodside Creek, New Zealand) or completely removed.

Lest all this sound too negative, we are at a wonderfully exciting period in geological research (as we always are) in which quantitative and logical rigor is developing with a new appreciation and methodology in process-oriented, question-asking field work. If we respect all the traditional and modern intellectual inputs into modern geology, treat the field environment with care, and engage in a rigorous scholarly and honest approach to the literature and communication with fellow workers, geology will continue to grow and flower.

My life as a tectonic geologist has allowed me to travel widely, to see a great deal of superb geology, to get to know a lot of fine geologists, and to make many lifetime friends, especially in the United States. To have been recognized for the rewarding life that I have lived is faintly embarrassing but I am profoundly honored and deeply touched to have been awarded the Penrose Medal of the Society for which and for whose members I have such affection and gratitude. I thank you all very much.

Presentation of the DAY MEDAL to SUSAN WERNER KIEFFER



Citation by EUGENE M. SHOEMAKER

Few scientists have as wide a range of interests, and none in geology have addressed with rigorous authority any more diverse set of topics than Sue Kieffer, superb experimentalist, field observer, theoretician, and teacher.

For openers, Sue solved the riddle of the formation of coesite in the Coconino Sandstone at Meteor Crater, Arizona. She developed a well-calibrated quantitative X-ray diffraction technique to determine the amount of coesite formed at different shock stages, demonstrated that the coesite occurs chiefly in collapsed pores, developed theoretical models that describe the shock propagation and the collapse of the pores, and concluded that coesite crystallizes from hot, sixfold coordinated silica produced by shock jetting from the pore walls. She then applied these results to a more general understanding of shock melting and shock lithification of porous regoliths.

As Sue worked on shock metamorphism, she had to worry about heat capacities.This led her off on the trail of lattice vibrations and Raman spectra, resulting in a series of five classic papers that, among other things, showed how to calculate isotopic fractionation factors from more basic physical observations. She extended the formulations of heat capacities to include both the acoustic and spectral data, and she derived values for complex silicates that closely match the best calorimetric measurements.

A unifying theme of her later work has combined thermodynamics with fluid dynamics. Basically, Sue wants to know why and how fluids erupt—here on Earth or on Io or Triton, 30 astronomical units away. There is a risk in pursuing this game. She helped Monitor Mount St. Helens from a site where, only days later, a USGS colleague was blown away in the blast. While carrying out an exquisite series of observations of Old Faithful Geyser, she slipped and broke her ankle, thus ending a career as an outstanding marathon runner. Out of all this has come a truly basic understanding of the theory of geysers and violent volcanic eruptions.

When active volcanic plumes were discovered on the Jovian satellite Io by Voyager 1 in 1979, and SO_2 gas was detected in one of the plumes, Kieffer promptly constructed the temperature-entropy phase diagram for SO_2. Such a diagram provides an elegant technique for tracing the thermodynamic path of a volatile fluid as it travels up a volcanic vent; she found that the observed heights of the plumes could readily be accounted for if they were driven by expansion of SO_2 gas from shallow reservoirs on Io. She then proceeded to a complete physical theory of violent gaseous volcanism, with application to plumes driven by both SO_2 and sulfur gas.

Now you wouldn't think that there is a connection between shock metamorphism and the flow of a river, but there is. Sue noticed that the equations stemming from conservation of mass, momentum, and energy for flow across a shock front are very similar to the equations for the flow of a river through a hydraulic jump.

In 1969, a catastrophic flood produced a debris fan at the mouth of Crystal Creek in the Grand Canyon, converting Crystal Rapid to the most difficult and dangerous rapid on the lower Colorado River. In the summer of 1983, discharge from Glen Canyon Dam was allowed to go as high as 96,000 cfs, and an enormous standing wave, up to 6 m high, formed at the foot of the steepest declivity at Crystal Rapid. Sue, an experienced "river rat," decided to investigate. She concluded that the giant wave had been formed by a hydraulic jump, and she carried out a thorough field and theoretical study, mapping the channel at Crystal Rapid at low flow and tracing the flow lines at various discharges. From this work, she established the equilibrium configuration of the channel for various discharges. By systematic study of many other rapids, she estimated a prehistoric peak flood discharge on the river of about 400,000 cfs. She found that the constrictions in the channels of each rapid were remarkably uniform and that this uniformity must be due to the capacity of the river to contour its own channel during episodes of supercritical flow.

Kieffer has an uncanny knack for solving the physics of interesting problems in geology and then developing applications of considerable generality. With deep insight in both geology and physics, she is the modern-day equivalent of G. K. Gilbert. Incidentally, while running rivers, she also plays the flute. Arthur L. Day would be proud of her.

Response by SUSAN WERNER KIEFFER

I would like to use this occasion of my acceptance of the Day Medal to offer a thought about interdisciplinary work—past, present, and future.

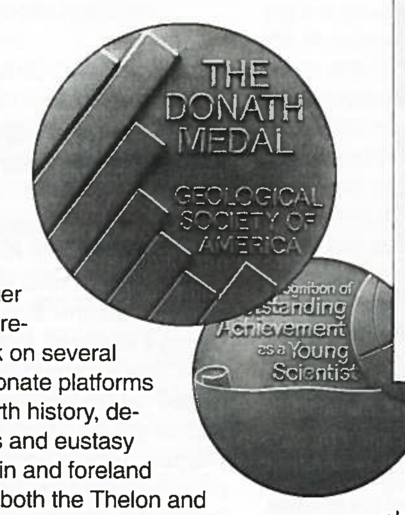
The medal was established by Arthur L. Day in 1948. He was 79 years old, and clearly remembered the difficulties that he had experienced decades earlier in bringing the subjects of physical chemistry and seismology into traditional geology; he had also struggled to establish his philosophy that the field could be used as a large-scale laboratory (see, for example, R. B. Sosman's memorial to Arthur Louis Day; GSA *Bulletin,* 1964, v. 75, p. 147–156). With the establishment of the medal, Day's intent was to foster and to recognize interdisciplinary efforts in the future.

Today, some 40 years later, I think that Day would be pleased with the progress we have made. Physics, mathematics, and chemistry are integral parts of our science. Our subdisciplines of seismology, geophysics, geochemistry, and planetary sciences clearly are established across older disciplinary boundaries. But, I believe that we have just started on the process of expanding the frontiers of geology, and that we have new interdisciplinary boundaries yet to explore.

We are going to tackle increasingly complex system problems on Planet Earth in the next decade. That science which we casually refer to simply as "geology" has the potential to be the most relevant, opportunistic, and intellectually aggressive of all of the sciences as our civilization moves from the 20th century exploitation of the Earth to a 21st century of conservation. I would like to see us welcome, embrace, and aggressively pursue fluid dynamics, biology, nonlinear dynamics, and computer systems sciences. I think that we have only begun to see the boundaries between our own relatively young subdisciplines of geochemistry and geophysics being explored. Societies like GSA can, and must, be a part of the ongoing metamorphosis of geology. We can maintain our strong traditional role by having meetings that are a forum for researchers, teachers, and students from many different fields and careers to meet and explore new ideas. We can—with other science and educational societies—have a new political voice. And we can greatly expand our involvement in education in many ways. In all of these activities, I hope we will work to push back the frontiers of interdisciplinary research and interdisciplinary teaching, and to pull down the barriers that separate seemingly disparate sciences. There really is a remarkable unity in studies of the Earth and its processes.

I deeply appreciate being a member of the scientific community, and in particular, of the geologic community. Sometimes the institutional, budgetary, or controversial events that get prominence obscure the fact that we are generally a supportive and generous community of scholars and friends. I thank all of you friends, teachers, and colleagues very much—not only for the honor of the Day Medal, but for the way we have shared our lives and our work.

Presentation of the
YOUNG SCIENTIST AWARD (DONATH MEDAL)
to
JOHN PETER GROTZINGER



Citation by
PAUL F. HOFFMAN

In the current renaissance in sedimentary geology, John Grotzinger has emerged as the master of the Precambrian. In 40 months of field work on several continents, he has shown how carbonate platforms evolved over two billion years of Earth history, demonstrated the interplay of tectonics and eustasy in the development of passive-margin and foreland basins, and decisively reinterpreted both the Thelon and Wopmay orogens of the Canadian shield.

This 35-year-old's career in earth science began in 1979 when, at the height of the oil boom, he chose to spend a summer working, without pay, in the mosquito-infested barren lands of the northwestern Canadian shield. A Philadelphia native, he had graduated from Hobart College in natural science and lacrosse. The late Preston Cloud saw his potential and encouraged him to try the Geological Survey of Canada as a sort of scientific "boot-camp." Hooked, he proceeded to the University of Montana at Missoula, where he did a Master's thesis under Don Winston on Mesoproterozoic carbonates in the Belt basin. His Doctoral thesis under Fred Read and Ken Eriksson at Virginia Tech was supported by the Canadian Survey, and it established the Rocknest platform of the Wopmay orogen as a paradigm for Paleoproterozoic carbonates. His intensive analysis of small-scale cycles and their lateral variation across the rimmed platform indicated a eustatic control suggestive of Milankovitch orbital forcing. Comparisons with Neoproterozoic, Mesoproterozoic, and Archean carbonate platforms led to his discovery of changes that he related to evolution in atmospheric and seawater chemistry.

As a postdoc at the Lamont-Doherty Geological Observatory working on contract to the Canadian Survey, he recognized and elucidated a foreland basin correlative with the Rocknest platform. The foreland basin interpretation contributed to the realization that the Thelon orogen represents a continent-continent collision, not merely an intraplate reactivation structure. Grotzinger then joined the faculty at the Massachusetts Institute of Technology and, following up an observation of the late Rein Tirrul, he and Sam Bowring dated zircons in far-traveled volcanic ash falls in the foreland basins of the Thelon and Wopmay orogens. They obtained accurate and precise ages for initial collision events in both orogens, and resolved a fundamental tectonic dilemma in the plate tectonic interpretation of Wopmay orogen. From stratigraphic data, Grotzinger and 1990 Donath Medalist Leigh Royden estimated a flexural rigidity for the Slave craton lithosphere two billion years ago. By this time, Grotzinger had demonstrated that the stratigraphic architecture of Precambrian carbonates and clastics was strongly influenced by relative sea level fluctuations operating on time scales ranging from 10^5 to 10^7 years and distances greatly exceeding the flexural wavelength of the lithosphere.

From the outset, Grotzinger exhibited extraordinary powers of observation and an intense natural curiosity. He personified the best in field geology, eagerly incorporating insights from geophysics, geochemistry, and geobiology, from theory and forward modeling, but believing (to paraphrase Ken Hsu) that geology is ultimately not about what should have happened but what actually did happen. He discovered early on to tackle general problems, doing so with poise and passion. Yet for all his fire and intensity, he is fondly admired for his sensitivity to the needs of others, born perhaps of personal knowledge that even the proverbial bed of roses has thorns.

On a personal note, I want to thank you, John, for rekindling my own interest in sedimentary geology and for suggesting the fantastic Kaoko belt of Namibia as my next field area.

Response by
JOHN PETER GROTZINGER

President Zen, Dr. and Mrs. Donath, ladies and gentlemen, it is an honor for me to be chosen as the recipient of this award. As established by previous medalists, this ceremony presents an opportunity to publicly thank those people who have contributed to my development as an earth scientist. I wish to thank several people in particular.

As an undergraduate at Hobart College I was first introduced to the excitement of research by William Ahrnsbrak and Donald Woodrow. The two enthusiastically guided my first research project involving a study of the diffusion of chloride through Quaternary sediments at the bottom of Lake Seneca in upstate New York.

From there I proceeded to the University of Montana for a master's degree under the direction of Don Winston. Don taught me how to study sedimentary rocks and developed my appreciation for the often spectacular preservation of Precambrian sediments.

My doctoral research was completed at Virginia Tech under the supervision of Ken Eriksson and Fred Read. I thank Ken for insights into the realm of clastic sedimentology and Precambrian sedimentation. Ken's broad-based research program demonstrated the need for a thorough knowledge of earth science outside of sedimentology and stratigraphy. I thank Fred for training me in the methods of carbonate sedimentology and for providing unlimited support and a fun and productive working environment. I also thank Fred for the opportunity to work with him in developing the first numerical model for carbonate cyclicity. This experience was responsible for my deciding to accept an institutional postdoctoral fellowship at Lamont-Doherty Geological Observatory. Virginia Tech also had many excellent graduate students, and I am indebted to Steve Dorobek, Charles Harris, Isabel Montanez, and Ed Simpson for many interesting discussions and debates.

At Lamont I had an extensive collaboration with Gerard Bond, Nick Christie-Blick, and Michelle Kominz, all of whom stimulated me to think about lithosphere-scale modeling. There I developed the idea to examine the geometry of Precambrian foreland basins as a way to determine the thermal structure of the continents at that time. Also, we worked together on assessing the potential for applying the concepts of sequence stratigraphy to nonfossiliferous Precambrian strata.

Since 1988 I have been a professor at MIT, and this has been a great experience because of interactions with many of the faculty there, including Clark Burchfiel, Sam Bowring, Kip Hodges, Wiki Royden, and John Southard. John and I have taught sedimentary geology together, and I have mapped with Kip and Clark. I have written papers with Sam on the tectonics of the Wopmay orogen and with Wiki on the elastic strength of Proterozoic continental lithosphere. I thank them all for their inspiration, enthusiasm, support, and criticism. I also thank our chairman, Tom Jordan, for strengthening our department in continental geology and continuing to promote harmonious collaborations among geology, geophysics, and geochemistry. Tom has been a very strong supporter of field geology at MIT, and at this time we now offer four separate field courses.

Since my graduation from Hobart College I have had a long and prosperous relationship with the Geological Survey of Canada supported primarily through the efforts of John McGlynn, the former Director of the Precambrian Division, and Paul Hoffman. Thirteen years ago Paul introduced me to regional mapping. His approach featured 1:50,000 observations collected at a 1:250,000 pace, in which we averaged about 15 miles per day, on foot, with the longest days marked by 25-mile traverses. Somehow I survived. Paul has had a great impact on my development as an earth scientist. He has acted as a mentor, enthusiast, devoted analyst of the Precambrian, and unflagging critic.

Moving along to familial relationships, I thank my parents, Paul and Mary Grotzinger, for showing me the value of intellectual flexibility and integrity, and the importance in considering all options. However, I am especially pleased to publicly thank my wife Donna, whose support and generosity are hard to fathom. She has tolerated field seasons spanning from 3 to 5 months per year for 8 years now, as well as late nights and lost weekends spent planning lectures and labs, grading exams, and writing research papers. In addition to her own career as an environmental engineer, she continues to support my endeavors, as well as those of my students, and recognizes the simple fact that aspiring field geologists can only be trained in the field. There are no shortcuts for this.

In closing, I thank you, President Zen, for this honor, which I humbly accept.

Presentation of the
GSA DISTINGUISHED SERVICE AWARD
to
ALLISON R. (PETE) PALMER

Citation by
E-AN ZEN

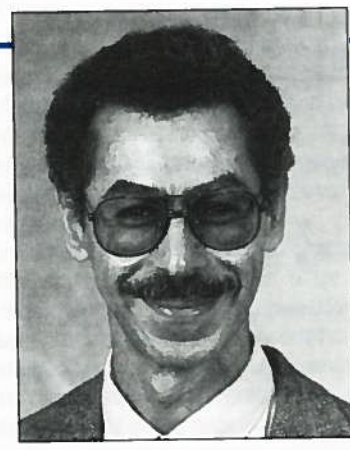
GSA's Distinguished Service Award is given to acknowledge especially significant contributions to the Society and to the profession. This year, the Council presents the award to Pete Palmer.

Pete was a distinguished paleontologist and stratigrapher with the U.S. Geological Survey, specializing in the Cambrian. He then joined the faculty at the State University of New York at Stony Brook before coming to GSA. The profession will remember his accomplishments as the coordinator for GSA's Decade of North American Geology project. Pete was immersed in all phases of fund raising and the planning, preparation, production, and publication of the DNAG volumes, charts, and maps, that summarize comprehensively our understanding of North American geology, and that stand as magnificent signposts to geological research. In the process, he performed the miracle of persuading nearly 2000 people to produce the manuscripts they had promised, and of meticulously editing their contributions. It's hard to imagine that any other person could combine the broad and keen scholarship, the editorial acumen, and the perseverance, persuasiveness, zest, and good humor Pete did to bring the DNAG dream to reality.

Pete was instrumental also in setting up the basis for GSA's entry into K–12 earth science education. From 1986 until his formal retirement from GSA a year ago, he was the part-time Education Coordinator for the Society. During this period, Pete established contacts with many other science organizations for future collaboration; he helped set the philosophical tone of what GSA should do, and he got our flagship effort, the Partners for Excellence program, off to a good start. The earth science education community, like the research community, benefited from Pete's skill and irrepressible enthusiasm.

Pete, I am privileged to represent the GSA in giving you this token of your colleagues' appreciation, respect, and affection, the GSA Distinguished Service Award for 1992.

Presentation of the
ARCHAEOLOGICAL GEOLOGY DIVISION AWARD
to
FEKRI A. HASSAN



Citation by
C. REID FERRING

On behalf of the Archaeological Geology Division, it is a pleasure and honor to present our Division Award to Fekri Hassan. The purpose of the Archaeological Geology Division Award is to formally recognize successful integration of geology and archaeology. Fekri's accomplishments in the arena of geoarchaeology are prodigious, and we are all proud of his success. Fekri's achievements have exemplified the aspirations of our Division. At the same time, his stature in the field of archaeology has enhanced the consideration and impact of geoarchaeology by many archaeologists, a goal that many of our members hold as one of our highest and most challenging.

I have enjoyed a friendship with Fekri for two decades, yet in this respect I am one of many. Fekri's wit and easy hospitality have always drawn friends and students to him. These friendships are inevitably intertwined with respect for Fekri's scientific philosophy and his progressively innovative ideas concerning the evolutionary dynamics of prehistoric cultures and their ecosystems.

The phenotype of his philosophy is a remarkable array of contributions to archaeology and geology. While awards such as this are typically made to register the printed evidence of productive contributions, I think that in Fekri's case, the award could easily have been made in recognition of the intellectual software behind the publications. For, over the past 20 years, Fekri has transcended geoarchaeology as interdisciplinary empiricism. Rather, he has incorporated geoarchaeological methods and concepts into his frequently bold and always challenging analyses of prehistoric cultural systems and the means by which we perceive them.

Fekri's education and training have clearly guided but not constrained his career. His bachelor's and master's degrees are in geology and chemistry from Ain Shams University in Cairo. After completing his Ph.D. in anthropology at Southern Methodist University, he began the geoarchaeological research in Egypt and adjacent regions that he vigorously continues today.

Fekri's research has remarkable topical and methodological breadth. The scale of his applications of geology to archaeology spans ceramic provenance to geoclimatic models of landscape change and human settlement patterns. His interest in paleoenvironmental reconstruction vis-á-vis culture change dynamics has been a mainstream focus for his research. These studies began with his work on the Late Paleolithic in the Nile Valley in the 1970s, continued with his exemplary studies of Capsian Escargotieres, and then mushroomed into a long-term focus on the emergence of agriculture and civilization in Egypt.

In these endeavors, there are few aspects of the archaeological record that have escaped his scrutiny. He has made significant contributions to archaeometry via analysis of radiocarbon ages and chronologies as these pertain to patterns of predynastic culture change. His geologic and paleoenvironmental investigations are meticulous; these are exemplified by his studies of playas in the Egyptian Sahara, where he has integrated data on climate, hydrography, and site formation into models of human adaptive responses. This work has been expanded more recently by analysis of global climatic changes and their biophysical impacts on landscapes and human populations.

Fekri's impact and stature as a spokesperson for interdisciplinary research is clearly evident in his activities in the profession. He is editor for two journals and writes numerous reviews for other journals. He has dedicated significant efforts on behalf of the Committee of the Egyptian Antiquities and the Ministry of Culture.

Fekri's contributions to our Division are greatly appreciated. He served on many committees prior to entering the "executive track" in 1987. I especially remember the symposium he and Bill Farrand organized in 1987, when they brought some of the leading paleoanthropologists and archaeometrists to the GSA forum to provide a stimulating discussion of early hominid biocultural evolution. This form of exchange between disciplines is really one of the signal aspects of Fekri's contributions to our Division's goals. He has written numerous synthetic review articles for journals and books, and he is a frequent speaker on behalf of geoarchaeology at symposia and as a guest lecturer.

His broad research interests, his fluency in archaeological concepts, and his innovative and thorough applications of geology to archaeology readily explain his achievements, on the one hand, and the attention he gains from archaeologists, on the other. Bertrand Russell said: "All the raw material of our knowledge consists of mental events in the lives of separate people." With our award, we recognize the unique energy that Fekri has brought to bear on archaeology, geology, and the methods of science. We are fortunate that his communicative skills have allowed and encouraged so many others to share in his experiences and gain insight into his perceptions.

Response by
FEKRI A. HASSAN

I am most honored by the Archaeological Geology Award inasmuch as it is an expression of my modest efforts in a field that I regard as one of the most exciting and fruitful domains of inquiry.

I find archaeological geology exciting because it opens up the door to the arena of human interaction with the earth. Although technically, we often define archaeological geology or geoarchaeology in terms of the applications of geological techniques and know-how to archaeological issues, the broader and deeper view of archaeological geology concerns the dynamic interactions between humankind and the earth from the moment human footprints were left in the volcanic ash of Africa to the recent archaeological past of nations and empires. From this perspective, I view archaeological geology not as a handmaid to archaeology, but as a field in its own right that promises to give us profound insights into our current ecological dilemma.

Gravitating toward archaeological geology was an outcome of my initial training in geology in Egypt. In the eternal shadow of the Pyramids of Giza, I worked on the stratigraphy and petrography of Eocene limestones. I was fortunate subsequently to work closely with Professor

Rushdi Said, who impressed me not just by his encyclopedic knowledge of geology, but also by the breadth of his intellectual pursuits, and his engaging personality. When I met Professor Said, he had been collaborating with Professor Fred Wendorf of SMU on the prehistory and Quaternary geology of Egypt. One morning, I found myself in a tent in Upper Egypt in the field camp of Professor Wendorf. It was an experience that changed my life. In camp were Bahay Issawi, Vance Haynes, Achilles Gautier, and Romauld Schild, a team of stars in geoarchaeology. I could not have had a better initiation.

A year later, I was in Dallas to study prehistoric archaeology. There, in the Institute for Earth and Man, I found myself in an environment where geology and prehistoric archaeology were the constituents of the air filling the corridors. It was a great learning experience to be in the company of Claude Albritton, Vance Haynes, and Jim Brooks. Fred Wendorf's work in Egypt exemplifying and nurturing the close collaboration between the earth sciences and archaeology allowed me to continue to dream of geology as I studied for my degree in archaeology. The dream was also sustained by the stimulating work of Karl W. Butzer on Egyptian geoarchaeology, and his general treatment of prehistoric geography and ecology which set the stage for the possibilities of archaeological geology.

When the opportunity arose of establishing a laboratory of geoarchaeology at Washington State University, following in the footsteps of the late R. Fryxell, I was at home in a department led by R. Daugherty, that recognized the key role of human interaction with earth in shaping the destiny of humankind. Working with students in my courses on Quaternary sediments and stratigraphy and geoarchaeology, I became keenly aware of many issues that face archaeologists in the field. Within the department of anthropology, my colleagues made me aware of the pulse of archaeology and the place of the human past in anthropology.

Equally important in shaping my geoarchaeological research is the collaborative research with Ralph Solecki, T. R. Hays, Don Henry, Robert Wenke, David Lubell, Michael Hoffman, and Barbara Barich. I was also sufficiently fortunate to lead my own expeditions to pursue my lifelong interest in the origins of agriculture and civilization.

I began working in the Western Desert of Egypt searching for evidence of the earliest food producers in the areas surrounding Siwa and Baharia oases. Geoarchaeological research involved reconstructing paleoclimate, developing a radiocarbon chronology, and examining the geodynamics of subsistence and settlement during the Holocene. Currently, work in Farafra Oasis, in collaboration with B. Barich and M. Mahmoud, extends geoarchaeological investigations to developing models of site formation in eroded playas, the hydrography of ephemeral lakes, and the implications of water resources for settlement and subsistence.

Along the Nile, my research in the Predynastic sites in the Nagada region provided opportunities to develop models of site formation using microarchaeological and microstratigraphic techniques, provenance studies, and geoekistics. The study of Holocene Nile floods proved to be useful in clarifying the role of Nile ecology in the rise of Egyptian civilization.

There are perhaps three issues that have emerged from geoarchaeological investigations that may be worth emphasizing. The first concerns the way we develop geoarchaeological models of people in the archaeological past. The second concerns the implications of geoarchaeological research for understanding the geologic past. The third concerns the implications of research for our current environmental crisis and the future of our relationship with the earth. It seems to me that it is essential to investigate the relationship between people and environment with a consideration of how people gain and retain knowledge of earth processes that impact their lives. It is perhaps the disparity between the human scale and the varied scales of geological processes that occasionally triggers crises and discontinuities. One cannot ignore the role of culture in shaping people's views of the earth.

Geoarchaeological information, by focusing on the dynamics of surficial earth processes within tight spatial and temporal controls, provides a wonderful opportunity to develop models for understanding the remote geological past. Data on desert erosion, the tempo of paleoclimatic fluctuations, and rates of riverine aggradations are a few examples of what we have gained from geoarchaeological research in Egypt.

The insights gained from geoarchaeological research on how our predecessors had dealt with the earth and on the outcome of their actions are undoubtedly crucial for understanding the history of the precarious links between environment and civilization. With the prospects of global warming, excessive soil erosion, droughts, catastrophic flooding, and the gravity of geological hazards in a crowded world of urban-dwellers, it behooves us to highlight our contributions to the understanding of the dynamics of human interactions with the earth. In this regard, I will also make a special appeal on the environmental hazards that now endanger the archaeological legacy of humankind. Humble scatter of lithic debris and spectacular temples and pyramids are threatened by pollutants and geological hazards resulting from economic and urban development projects. My recent work on the Great Sphinx of Giza and the Pyramids of Giza is a first step for me in addressing this critical situation.

I am heartened by the growth and development of archaeological geology as a viable and thriving interdisciplinary endeavor. Recalling the excitement of the meeting on geology in archaeology in Dallas in 1973, when the idea for this Division was born, I realize how far we have come as a society of minds dedicated to a better understanding of our place on this planet. I trust that a new generation of geoarchaeologists will carry the mission of this Division much further than we may have dreamed, and it is to those young scholars who now must lead us into the 21st century that my work, recognized by this award, is dedicated. I hope that their true reward, as mine was, will be the thrill of being at a new frontier.

Presentation of the GILBERT H. CADY AWARD to TOM L. PHILLIPS

Citation by WILLIAM A. DiMICHELE

The Gilbert H. Cady Award is presented to Tom L. Phillips for his pioneering studies of the plant-composition of Pennsylvanian-aged coals. Through the development of innovative techniques in sampling, analysis, and data retrieval, Dr. Phillips has quantitatively studied coal balls from over 50 coals in North America, Europe, North Africa, and China, a body of work that constitutes one of the most important paleobotanical contributions to science in the last quarter century. Using palynology and macrofossils, he and collaborators recognized and documented major changes in the botanical composition of coal, the most prominent resulting from a major extinction near the Middle-Late Pennsylvanian boundary. In a search for causal mechanisms he was instrumental in reawakening interest in climate as a major variable in the equation of Pennsylvanian coal abundance and quality. Through his collaborations, which have brought together paleobotanists, palynologists, coal petrographers, sedimentologists, and geochemists, Dr. Phillips raised the study of coal to an exceptionally high level of integration and resolution.

Tom Phillips has contributed to both botany and geology throughout his career. In addition to coal, he has studied the morphology and evolution of ferns, the paleoecology and life-history biology of arborescent lycopsids, and the stratigraphic distribution of coal-swamp plants. His definitive paleoecological work combines a distinctly quantitative approach to the study of peat-forming ecosystems with detailed study of individual species ecologies.

During his tenure at the University of Illinois, Tom Phillips has trained students in plant morphology, palynology, paleoecology, and coal petrography, and his enthusiasm as a teacher has inspired numerous undergraduates. Through his direction and vision the paleobotanical facilities at the University of Illinois have become among the best in the world.

Tom Phillips has been and continues to be a demanding task-master for himself, his colleagues, and his students. His achievements reflect a strong sense of responsibility and hard work, logic and yet the willingness to entertain the improbable if not seemingly impossible, and an irrepressibly positive, can-do attitude. He has served the coal geology community by relentlessly pursuing an understanding of the structure and function of Pennsylvanian peat-forming plant communities and the relationships between plants and qualities of the derived coal. Through the highest levels of scholarship, unabated enthusiasm for primary data collection and analysis, and broad vision, Tom Phillips has been central to the growth and health of coal geology.

Response by TOM L. PHILLIPS

Thank you, Tim Cross, and thank you, Bill DiMichele!

This is a very special occasion for me and I am delighted to be a recipient of the Gilbert H. Cady Award. Shocked, surprised, and reflective also describe my feelings about such recognition of team efforts.

In fact, I was so surprised that I checked the address on the envelope to make sure that it was correctly addressed. I noted that GSA headquarters had deftly altered the name of my home department with the insertion of an "e" to make it Planet Biology—a splendid name considering the increasing scope of studies in paleobiology and coal geology! This recognition of a team effort has caused me to reflect on all the help, encouragement and cooperation over the years and how very much I have enjoyed and continue to appreciate the joint efforts with colleagues in North America as well as in Europe. Most important, I am delighted that there was a Gilbert H. Cady, whose memory and influences are honored by this award. The tradition that Dr. Cady established and that has been passed along to all of us is extremely meaningful to me.

I want to especially acknowledge the extended collaboration with Henry N. Andrews, Russel A. Peppers, Aureal T. Cross, William A. DiMichele and all my former students.

What the Cady Award means to me is interwoven with joint efforts developed with colleagues of the Illinois Geological Survey: first with Matthew J. Avcin (later at the Iowa Survey) and, subsequently with Hermann W. Pfefferkorn, Russel A. Peppers, Philip J. DeMaris, W. John Nelson, Richard Harvey, and Debbie Gaines. Debbie Gaines managed the computer programs and data for years. Such collaborative research with the Illinois Survey was made possible by the sustained encouragement and support by an outstanding succession of Coal Section heads, M. E. Hopkins, Harold Gluskoter and Heinz Damberger, who have continued to help all these years in the best tradition of Dr. Cady.

Two earlier heads of the Coal Division, Gilbert Cady and Jack Simon, may well be considered "godfathers" of coal-ball–based research in the Illinois Basin. It was Dr. Cady who discovered the first coal balls in the Illinois Basin and who encouraged James M. Schopf in study of them as a basis for the botanical constitution of coal as well as the plant sources of spores. It was James M. Schopf who went on to become known as the "coal-ball man" by many, as much for his direct contributions as for his generous help and encouragement of colleagues. It was Jack Simon, as Chief of the Illinois Survey, who generously helped us get the coal-ball studies launched.

There was prolonged difficulty in obtaining support for what we were trying to do because it was so interdisciplinarian in nature—seeking the interrelationships of Pennsylvanian coal-swamp vegetation to coal origins with a reference base from coal-ball concretions. The most frequent, massive, and concentrated occurrences of anatomically preserved and identifiable vascular plant deposits in the entire geologic column occur in Pennsylvanian (Upper Carboniferous) coal deposits of North America and Europe, as coal balls in the Euramerican tropical belt. It was also a logistic challenge whereby tons of coal balls had to be located and collected and the botanical constituents identified and quantified. In retrospect, I can appreciate the skepticism and doubt, and I all the more appreciate the help of the Illinois Survey. I recall my initial hesitance to go and see Jack Simon, the Survey Chief. Robert M. Kosanke, a long-time friend of coal geologists and a cherished friend from his Urbana days, encouraged me to ask Jack Simon for help. Jack not only helped but had the kindest way of conveying it. He said something to the effect that what I was doing was needed and that the Survey ought to be willing to help. In turn, he asked me to see M. E. Hopkins about how many thousands of computer cards needed to be punched. "Hoppy," as he was known affectionately, was extremely kind and tolerant of my uncertainty about tens versus hundreds of thousands of computer cards—but I am getting way ahead of the story, as it involves the Illinois Geological Survey and a number of previous Cady Award recipients. What I want to convey at this point is that for years I have walked down the halls of the Natural Resources Building, which partially houses the Illinois Geological Survey, and felt more at home and in touch with valued colleagues than anywhere else. I have splendid colleagues in both the Plant Biology and Geology departments, and I think my feelings about the Illinois Survey reflect something special about the people and the Cady tradition they represent.

One of the key turning points in quantitative aspects of coal-ball studies developed from a three-way series of projects that led to several brief associations with Dr. Cady. I emphasize brief—one was memorable in my failure to answer Dr. Cady's questions about coal-ball origins; the other was memorable because of his help. The three projects underway at the time were the stratigraphic examination of coal-ball contents from all available sources with Matthew J. Avcin, assembling the history of paleobotany in the Illinois Basin with Hermann Pfefferkorn and Russel Peppers, and creation of computer programs with Barry Kunz and Daniel Mickish in the Physics Department. Barry and Dan developed the initial computer programs for the quantification of the botanical constituents of coal balls, and they were my first co-authors on the subject. Matt Avcin, a student of conodonts, came to my lab at the suggestion of Charles Collinson to complement his training in palynology. This developed into a life-long friendship that arose from sharing the stratigraphic search of coal-swamp vegetational patterns in coal-ball deposits. Matt and his wife, Penney, helped with every aspect of the research and made it a delightful team effort in the discovery of the extinction of lepidodendrids near the Middle-Upper Pennsylvanian boundary. The history project with Hermann and Russ was mostly the outgrowth of Hermann's wanting to know the history of paleobotanical studies in the Illinois Basin.

It was in connection with the history of coal-ball studies and perhaps missed opportunities of discovery that brought me to Bill Smith's office at the Illinois Survey. Bill had discovered several old reports that may have concerned massive occurrences of coal balls. Some were in the Colchester Coal, which has been one of the most elusive major coal deposits to yield such data. In briefly excusing himself to run down another inquiry, Bill introduced me to Dr. Cady with "Tommy is interested in coal-ball occurrences." Whereupon I was subjected to the most penetrating questions about coal-ball origins and all but withered at the clear lack of receptivity of what little I had to offer. Dr. Cady persisted, to the point, with his hearing aid turned toward me, that I clearly realized he was not buying any of what I was saying, nor was he letting me go. As best I can recall from that encounter, I finally tried to get off the hook by admitting we did not understand the origins of coal balls where they occurred so massively (Springfield and Herrin coals). If you have ever flunked an oral exam, you would empathize with my feelings. On the other hand, you would realize how such an encounter primes the pump to find out more about coal-ball origins. The studies of the Illinois Survey represented especially by DeMaris, Bauer, Cahill, and Damberger, much later, have elevated the level of inquiry to what might have answered some of Dr. Cady's questions.

As the stratigraphic patterns of Pennsylvanian coal-swamp vegetation reached a semiquantitative threshold, both Matt and Hermann urged me to give a seminar at the Survey and, in turn, request access to the Survey coal-ball collection. The Survey collection went back to A.C. Noé's time. Although the collections were not as massive as what Wilson N. Stewart and his students had collected at the University, they were stratigraphically extensive and diverse. The collections included the Nashville coal balls (Herrin Coal) that Jim Schopf had first quantified by different techniques from our own. We needed greater coverage as well as means to compare quantitative results by different methodologies.

Noontime seminars at the Illinois Survey are held on Fridays in 101 Natural Resources Building. I remember that just before the start of the seminar a secretary pointed out that Dr. Cady was in attendance and that was unusual in view of his health. Dr. Cady sat directly beneath the light switches for the room and much to my chagrin, he seemed to doze when the slides on the screen allowed one to see him in the course of the talk. I do not know how I felt about that—a mixture of disappointment and relief, considering our previous encounter. I did not address coal-ball origins.

When I finished and the lights came on, the first question was from Dr. Cady, as to why there were no plots for Nashville, Illinois (Herrin Coal). I admitted that we did not have access to the Survey coal-ball collections. After the seminar, Dr. Cady, in the presence of Jack Simon, M.E. Hopkins, and myself, said the magic words, "This young man needs to study the Survey collection." On Monday morning we loaded up the Survey collection of coal balls to expand our data base by 2000 specimens and numerous localities and stratigraphic occurrences. Out of this came a potential test of the absence of lepidodendrids in the Upper Pennsylvanian—at a hard-to-find hole in the ground. It was during flood stage of the Wabash, and we tramped across pigpens, with the owner's permission. In the intermittent rain, Russ Peppers, Kenneth Cope, and I spent considerable time messing around a hopeless situation of trying to find coal balls in a drowned pit, surrounded by trees more than 40 years old and a flush grass cover. The rumples in the grass cover around the pit attracted my attention as well as frustration, and I recall how futile the trip to Palestine, Illinois, seemed to me. All of us have been on field trips with such elements of discouragement! However, one kick at the rumples of grass revealed coal balls of the Bristol Hill Coal, generating a yell to my colleagues scattered elsewhere about the pit. Our first additional test of the "extinction hypothesis" held as well as the predicted dominance by *Psaronius*. Within weeks, Hermann Pfefferkorn, with Roger Nance's help, came in bearing coal balls from the Friendsville Coal; tree ferns were dominant, and there were no lepidodendrids in the Upper Pennsylvanian.

We were as sure as we could be at the time that a major extinction event had occurred somewhere near the Middle-Upper Pennsylvanian boundary, but it was really Russel Pepper's work that clearly delineated where and confirmed our interpretations based on coal-ball data. Russ had missed my Survey seminar but had called the next week and asked if I would like to see what the stratigraphic pattern of dispersed coal spores looked like. Several months later he had plotted such a chart. That stratigraphic chart was the centerpiece of our paper in *Science*. This was an exciting time for us, and while we felt that we had "discovered" the extinction of lepidodendrids in North America, previous contributions clearly pointed to the same patterns—especially David White's observations, the studies by Marcia Winslow, and indeed, the work of Robert M. Kosanke, Russ's graduate advisor at the University of Illinois. Bob Kosanke had pioneered the basin-wide biostratigraphic correlations of coal deposits and had, in effect, in earlier papers established the diminution patterns of the key *Lycospora* microspores.

In 1972, when Hermann, Russ, and I were assembling the *Development of Paleobotany in the Illinois Basin,* Jim Schopf provided the most detailed set of recollections about the history, as well as some philosophical gems that I have never forgotten. He wrote, "It seemed to me that coal geology, in general, was the reasonable field of economic interest for someone who was in paleobotany and it has always seemed strange to me that so few paleobotanists have had more than a very generalized acquaintance with coal." There have been numerous exceptions recognized by the Cady Award—especially James M. Schopf, William Spackman, Aureal T. Cross, and Robert M. Kosanke. All of these colleagues have been extremely influential in what our team has tried to do. John C. Ferm, the 1991 recipient, also has an interest in paleobotany, and his kind help has been deeply appreciated in our field work in Ohio.

In the 1972 letter from Jim Schopf, he recalled from his observations of the Herrin coal-ball constituents at Nashville, Illinois, "I was greatly interested in the paleoecology of the deposit, but it was a problem that I never was able to work through to a general conclusion. It seems to me that this is still a very important objective because coal-ball assemblages stand a better chance of characterizing the coal measures peat swamp environment than almost any other source of information." I considered that view an enormous source of encouragement, and as many of you know, Jim Schopf was our strongest supporter in the development of such studies.

I need to back up to earlier times to acknowledge the helping start from teachers. As a senior at the University of Tennessee I was fortunate to have had Robert E. McLaughlin as a teacher in many courses, including my first paleobotany course. "Mac," as he was affectionately known to us, introduced me to coal balls and, along with Harry Klepser and Aaron J. Sharp, directed me to graduate studies with Henry N. Andrews at Washington University in St. Louis. One of the most important kinds of advice a senior can receive from teachers is where to go for graduate studies—more especially, the best mentor. I was fortunate to receive such advice. Henry Andrews was one of the American pioneers in coal-ball studies, along with James M. Schopf and Aureal T. Cross, among others. Henry is a quiet-spoken scientist with enormous energy and enthusiasm for explorations of many kinds. As my advisor he kindly gave me much latitude in developing my own interests. It was Henry Andrews who acquainted me with large-scale coal-ball collecting, starting with a bulldozer, and encouraged my Euramerican-wide evolutionary studies of *Botryopteris* ferns. Henry shared with me his research interests and field explorations, especially in the Devonian of West Virginia and the high Arctic from the 1960s to near the time of his retirement in 1975.

It was through Henry that I first met James M. Schopf at the USGS Coal Geology Laboratory at Columbus, Ohio, where I worked in the summer of 1959. Jim was a rigorous supervisor, and it was only in retrospect that I came to recognize the Cady influence in this outstanding researcher. On the other hand, one anecdote told by Robert M. Kosanke about Jim being confronted by Dr. Cady upon arrival at work at the Illinois Survey at about 10 a.m., conveyed an important understanding on Jim's part. When Jim first called Washington University to offer me the summer employment, the well-informed Botany Department secretary said something to the effect, "Oh, he never shows up until after 10 a.m.!" Nevertheless, Jim called back. Jim worked very long hours, as did I, but I was never late to work in his lab! That brief experience with Jim Schopf led to a sustained association in later years, marked by much correspondence and invaluable advice and encouragement.

When I joined the Botany Department at the University of Illinois in 1961, there was a vigorous paleobotanical program led by Wilson N. Stewart and two of his former doctoral students, Theodore Delevoryas and Robert M. Kosanke. Bob taught palynology and directed graduate theses in addition to his full-time Illinois Survey work. I quickly learned about the Schopf influence on Stewart's development of coal-ball studies, and eventually of the long friendships among Schopf, Kosanke, Simon, and Cross.

My first direct connection with the Geology Department was with John M. Dennison (a former student of Aureal Cross), with whom I shared teaching interests and field work in West Virginia. John and I decided to sit in on Harold R. Wanless's Pennsylvanian stratigraphy course—the last time it was offered. That experience had a lasting impression on me, and indeed, I felt relieved not to have to take the final exam! However, it was a key introduction that proved vital as we tried to track the approximate correlations of Pennsylvanian coals later. Here I should mention the importance of able graduate students. We would never have gotten into the correlations and compilations of bituminous coal resources had it not been for Philip J. DeMaris and John Shepard. It was all motivated by their interests to test geologic indicators of relative wetness and other factors permitting such coal deposits. We had an enormous amount of help from Robert M. Kosanke, Russel A. Peppers, Hermann W. Pfefferkorn, William H. Gillespie, and others. The starting baseline was Wanless's stratigraphic chart. Teachers make an enormous difference in what each of us ultimately undertakes!

Because of early interests in plant evolution and my paleobotanical pursuit of *Botryopteris* ferns in the Pennsylvanian Period, I came to realize that the paleoecology of the coal swamps had to be tackled. Stratigraphic patterns of changing *Botryopteris* species were associated with certain events or environmental perturbations affecting the coal-swamp vegetation as a whole. This led to our team efforts to delineate the approximate times and kinds of changes as well as to look for causal mechanisms. It was during this time that I came to be associated with the Coal Division of the Geological Society of America. From the outset I was impressed by the openness and welcoming encouragement of coal geologists to our efforts. This was accentuated when Peter H. Given and Arthur D. Cohen invited several of us from Urbana to participate in the 1974 symposium "Interdisciplinary Studies of Peat and Coal" at Miami (jointly sponsored by the Organic Geochemistry Division of the Geochemical Society). This symposium was the first opportunity for us to share our beginning studies, and I really appreciated the encouragement from so many people. However, the highlight of those meetings was clearly the field trip to the Okefenokee and Everglades led by Bill Spackman, Russ Dutcher, and the Penn State navy. This was truly an inspirational experience and an opportunity to meet many people for the first time.

Eleven years ago here in Cincinnati, Norman C. Hester and James C. Cobb organized a division symposium, "Origin of Coal," and invited Russ Peppers and me to participate. That invitation was an impetus to getting our thoughts before you on climatic control and, in turn, led C. Blaine Cecil and me to organize another symposium, "Paleoclimatic Controls on Coal Resources of the Pennsylvanian System of North America" at the 1983 meetings in Indianapolis. It was dedicated to James M. Schopf. Since then the role of paleoclimate in our thinking about Pennsylvanian coal swamps has become increasingly important, in large part thanks to the work of Blaine Cecil and his colleagues and to Fred Ziegler and his colleagues. The role of plant paleoecology in the late Carboniferous tropics has expanded, especially through the contributions of Robert Gastaldo, Hermann Pfefferkorn, Andrew Scott, and their respective students. These growing contributions, including many more than I have mentioned, were nowhere more evident than at the recent Wolfville, Nova Scotia, meetings organized by John Calder and Martin Gibling. The symposium was entitled "The Euramerican Coal Province: Controls on Tropical Peat Accumulation in the Late Paleozoic." It was a worthy successor to the Crystal Cliffs conferences. I was delighted to meet Peter A. Hacquebard there for the first time.

It is also important to refer to the Coal Division's 1992 program here in Cincinnati, "Physical and Chemical Responses to Allocyclic Processes in Carboniferous Coal-bearing Strata," organized by Blaine Cecil and Cortland Eble, as well as their field trip, and the session on "Biotic Responses to Allocyclic Processes in Coal-bearing Strata." It is evident in the scope of upper Carboniferous reconstructional studies of earth history that the activities of the Coal Division, as exemplified by these symposia and field trips, have helped coal geology become a focusing discipline for many related studies. We are after the "big picture" as well as its many small "windows"! As a consequence, there has never been a better time to be all-inclusive in encouraging young people from allied fields to join us.

In closing, I wish to return to an appreciative note on the roles of geological surveys, particularly at a time when budget cuts have been extremely severe in my home state of Illinois. Our research has been dependent on the cooperation of the geological surveys over the years, as I have emphasized with the Illinois Survey. It has extended to many others, and I want to acknowledge especially the help and collaboration of Matthew J. Avcin and Robert Ravn during their times at the Iowa Survey, Samuel A. Friedman at the Oklahoma Survey, Donald L. Eggert and Harold C. Hutchison at the Indiana Survey, and many at the Kentucky Survey, especially Allen Williamson, David Williams, Donald R. Chesnut, and Cortland F. Eble. There are many research programs at universities that simply would never get off the ground were it not for the important role of our state surveys and, of course, the USGS. Ours is a case in point.

Much of the heritage and tradition of Dr. Cady has been passed along through the geological surveys. I feel that I am doubly fortunate to have received such help and encouragement as well as the recognition conveyed by this award. Thank you!

Presentation of the E. B. BURWELL, JR., AWARD to GEORGE ALFRED KIERSCH

Citation by ELLIS L. KRINITZSKY

The E. B. Burwell, Jr., award serves two purposes. It is a memorial to E. B. Burwell, a leader in the practice of modern engineering geology and first chief geologist for the U.S. Army Corps of Engineers, the largest engineering organization in the world; and it recognizes a published work that has enhanced our knowledge and advanced our profession. To maintain great distinction in these categories, an award recipient is eligible from anywhere in the world and does not need to be a member of this Society.

For 1992, the recipient of this honor is Professor George A. Kiersch. He has distinguished himself as an educator, a researcher, and a practitioner. In these few moments that are allotted to me, I cannot do justice to the breadth of his contributions, but let me state some of the most salient.

In academia, George Kiersch led the way in expanding geology beyond its traditional paths into those that have given geology its importance in engineering, exploration, and control of the environment. As a consultant, he was an advisor for nearly 200 projects in the United States and around the world, and he was involved in nearly 100 legal cases, including issues before the World Court. Also, he has written 350 papers and reports and five books, was editor for eight other books, and has served in many useful capacities in eight professional societies. This wealth of involvement came together in the work for which we are honoring him: *The Heritage of Engineering Geology, The First Hundred Years.* This is a symposium volume that scans the state-of-the-art and which Professor Kiersch conceived, directed, and edited. The work contains 25 important reviews. Five bear Professor Kiersch's name.

This book is much more than its title tells us. it is a history of engineering geology and of its leaders, and it is a detailed summary of the state-of-the-art in its many aspects.

Engineering Geology interacts with many other fields. As a consequence, it is subject to encroachments by non-geologists. Civil engineers, soil mechanics specialists, hydraulic engineers, even seismologists, are apt to claim expertise at the expense of engineering geologists. They do so only with disadvantage to their projects, because the engineering geologist is better prepared for those tasks. A book such as this amply shows what constitutes the engineering geologist's domain.

Professor Kiersch's book surveys the relevance to engineering of geological processes, gives a comprehensive survey of the methods used to accomplish geological investigations, and discusses failures, litigation, and the geologist's responsibilities. There are instructive discussions of surface and ground water, sea coasts, slopes, subsidence, faults, earthquakes, rebound from unloading, permafrost, construction materials, and siting. The state of the art is here, as well as extensive collections of useful experience.

This book is a credit to the many experienced professionals who contributed their knowledge, but the greatest credit rests with George Kiersch, who not only added much to the content of the book but also performed a valuable service to the profession by bringing together this superb group of reviews. This is a book that every engineering geologist can read with pride in his profession, and that users of engineering geology can read with an appreciation of its worth.

On behalf of the Geological Society of America and the Engineering Geology Division, I take pleasure in presenting the Burwell Award to George A. Kiersch. He has amply and deservedly earned this honor.

Response by GEORGE ALFRED KIERSCH

Thank you Ellis, for the very generous and kind citation and career comments; it has been a half-century of stimulating and rewarding activities.

Colleagues, friends, and guests, I am honored and accept with humility the Edward B. Burwell, Jr., Memorial Award of 1992. To all those responsible—my gratitude and appreciative thanks for this recognition, which has been made possible by the strong support of and past guidance from many professionals and by the positive assistance of my family, especially my wife, Jane.

Thirty-four experienced geoscience professionals participated in the preparation of the final 25 chapters in the *Heritage* volume. Additional chapters were planned on topics relevant to Environmental/Engineering Geology theory and practice, such as geologists and public policy, urban geology, military geology, radioactive waste disposal, computer technology and exploration techniques, weathering phenomena—weak rock and soil, and karst terrains. Regrettably, the contributors were unable to complete GSA-quality manuscripts and graphics. Anyone who has edited a broad-based volume is aware of the challenges.

Heritage is not a textbook per se with an intended smooth flow between chapters. Rather, the volume is a historical review of the changes in "engineering geology practice" through time, especially the past 100 years. Furthermore, where it is relevant, the authors note how the efforts of geologists for engineering works have contributed to our advances in knowledge and techniques of the geosciences.

On this occasion, I believe a short critique is appropriate on our chosen field of Environmental/Engineering Geology as reviewed in the *Heritage* volume. Many critical issues and concerns have emerged since the 1970s that affect practitioners and academics alike, and more are ahead. These issues will invariably impact on the Division's future and on geoscientists in general, so this seems an ideal place and time to mention several of them.

1. Foremost among concerns is the strongly organized thrust by elements of the engineering profession to reshape and redefine the broad discipline of engineering geology. The dominantly geoscience-oriented practice (60% to 80% geology related) has long been considered a branch of technical geology along with mining, petroleum, groundwater and hydrology, and the younger environmental-related practices. Today, the geoscience-oriented practice of engineering geology is being slowly and systematically propelled into an engineering-oriented discipline and practice, a trend that should be derailed and reversed. The engineer's plan-of-action is supported by both geoengineering programs, (one or more curriculum options in geological engineering) and geotechnical engineering training for civil and environmental engineers at many institutions.

The geological specialist on the team of technical experts provides an inherently different philosophical approach to the geology-related needs and reactions set in motion by the construction and operation of engineered works from that of an engineering-oriented specialist. Most importantly, the geoscience expert furnishes a multiple-hypothesis approach to each anticipated reaction or problem, supported by field observations and substantive data on the effects of processes or events. The underlying cause or origin of the difficulties consequently becomes the basis for selecting and designing the most suitable solution. The geoscience approach thus moves from simple and logical concepts to complex ones, with insight and guidance from the recent past—a key to the future.

The engineering-oriented approach to such problems usually strives for the safest and most economical solution and, unlike the engineering geologist, may not consider and/or emphasize the underlying geology-related cause(s). Instead, a typical engineering approach will assume a specific likely cause or causes based on similar case histories and from this database analyze the likely effect on a design or the constructed works. Many failures and difficulties associated with operating engineered works have occurred because of the engineer's inadequate database for mitigating the geology-related problems. A few such cases are noted in the *Heritage* volume.

2. A second concern relates to the abundance—sometimes the preponderance—of nongeological practitioners within the broad field of Environmental/Engineering Geology—specifically the large number of engineers with the limited academic training of "geology for engineers only." This introduction to the principles of geological science is customarily offered as a single course at the stronger civil and environmental engineering institutions around the world. While some geotechnical engineers in the United States may take an additional course or courses in geoscience, this is not common in most foreign institutions, as in Central and South America, Pacific-Asia, Africa, and some European countries. Many geotechnical engineers in North America and foreign countries with this limited geological training, termed "Band-aid knowledge" by W. J. Mead, a professor at MIT in the 1930s–1950s, become involved with important geological decisions affecting engineered works. Evidence of their inadequate scientific background for evaluating risks are represented by many failures and/or controversial designs of past works worldwide. "Geology for engineers" training, initially called "engineering geology" at many institutions in North America beginning in the early 1900s, has been renamed and correctly focused at most institutions in the United States since the 1950s. Regrettably, however, this same correction and redefinition of engineering geology practice has not been accepted by many foreign practitioners, and to them "geology for engineers" implies professional training. This misnomer has become a basis for serious misunderstandings within the international community. Particularly relevant is the belief of Dr. Karl Terzaghi (1963), called the father of soil mechanics techniques, that every soils specialist should be half geologist—a combination, he acknowledged with disappointment, that had not been followed by his successors. Soil specialists accepted the European terminology during the 1960s and renamed themselves geotechnical engineers; usually, they have a very minimal geological background, except in Canada and several schools in the United States where the training is broader (*The Heritage of Engineering Geology,* p. 79).

3. A third concern relates to the organized thrust by the International Association of Engineering Geologists (IAEG) to redefine the discipline of engineering geology. They have endorsed a change from the established geoscience-oriented and related practice, and they define engineering geology as "*a science* devoted to the study and solution of engineering and environmental problems ... that are related to geology ..." (IAEG Council meeting, 1992). How can a specialized scientific discipline founded on the principles, techniques, and know-how of the geological sciences suddenly become a new science closely oriented to engineering? IAEG originally proposed a redefinition as an "engineering science." (Many other areas besides civil engineering are served by engineering geology practitioners, among them applied sciences, contaminant hydrogeology, counseling, land-used planning, litigation, hazards mitigation, geological risks, and resource development.)

Consistent with IAEG's thrust for redefining our field of practice are actions of the International Union of Geological Sciences (IUGS) at its council meetings in 1992. IUGS accepted the application of the International Society of Soil Mechanics and Foundation Engineers (ISSMFE) and appointed that organization a member of its council. The ISSMFE's affiliate in the United States is the Association of Engineering Firms Practicing in the Geosciences (ASFE).

A collateral concern relates to the U.S. National Committee on Rock Mechanics and the ISRM worldwide; both have drastically reduced the input of geosciences to the testing and interpretation of rock stress data. What many of us called "structural geology with numbers" back in the 1940s–1950s has evolved primarily into the mechanical testing and statistical analysis of data. While I was serving the Division as its representative to USNC—Rock Mechanics, 1980–1986, a replacement for the retiring executive secretary of USNC—RM was appointed. The geotechnical engineer selected had no training in the elements of geology, yet the committee's functions were related to geology. Today, technical sessions commonly offer papers on "geology for rock mechanics"—a long, long way from where we started back in the mid-1940s, with Underground Explosion Test Program (UETP) research, and later William Judd's 1963 conference, "State of Stress in Earth's Crust." Will future design engineers be limited to a classification of the rock type, along with a compilation of related statistical test data from which to select the rock parameters—in lieu of a field investigation by an experienced geologist to ascertain the physical characteristics, geological history, and deformation events that affected the rock mass and are very likely crucial to the assessment of geological risk?

4. The engineer must assume risk from natural- and human-induced hazards or a project-wide calculated risk when designing engineered works in complex geological environs. These common uncertainties can be substantially reduced, with the insight and guidance from mature, field-experienced geoscience practitioners.

Unfortunately for all concerned, many engineers support the *myth* that only a calculated mathematical analysis including finite element and computer language constitutes an engineering or applied-science solution for a problem. When confronted by this attitude, a geological practitioner lacks the opportunity or incentives to apply the best rationale to problems that cannot be solved by standard calculations. There is overwhelming evidence that geological decisions based on experience and mature judgment are the logical and best approach to the uncertainty associated with a site and its environs, and not an engineering calculation per se.

5. An adjunct to the engineer's calculation *myth* is a growing acceptance of "hired-gun" and "oriented-expert" witnesses to introduce "junk science" in geologically related litigation. Unconventional and untested "theories" are advocated to explain a geological process, event, or feature; invariably, a "twist" or biased testimony favorable to the client's legal position is included. Facts, science, and judgment are not of equal importance to the "junk science" advocate, who is usually a person with minimal geological experience. They can be destroyed by a tough, in-depth cross examination by an opposing attorney with the assistance of a knowledgeable geological expert.

Because every earth structure is constructed in or of a geologic medium, the overwhelming influence of the geosciences on engineered works and human activities cannot be dismissed from our thinking. Practice never becomes routine and dull; case histories and formulas *alone* cannot provide the quality of insights needed. The best source is the field-experienced geoscientist.

6. Far from being least important, my last group of concerns relates to education, academic staffs, and training for Environmental/Engineering Geology professionals. None of us can expect to be a specialist in all the sub-branches or disciplines of geosciences and technology relevant to engineering geology practice. Yet it is imperative that we become a specialist in some area(s) and deal with these categories ourselves, but know of more specialized colleagues for the other areas. As the technical assessor of the earth's near-surface environs and dwelling place for human activities and engineered works, we have a singular responsibility to investigate all pertinent aspects and to communicate the database in an understandable manner for all humankind. To accomplish this, more attention and focused effort must be given to a number of interrelated issues.

Educate students for careers, not just for the first job or the initial few years of practice. There are major differences in the long-range potential and capabilities of geological training as offered today. The typical geological engineering major with a generalized geological background (some 20% of coursework), is exceptionally well versed in exploration techniques, standards of practice, report writing, and the systematic actions and regulatory compliances surrounding modern practice. Too often, however, lack of an in-depth understanding of specialized geoscience principles and techniques becomes a handicap with time and increased responsibility; the typical geological engineer becomes unable to provide the needed insight and meaningful evaluation of processes and geologic events so critical to the interpretation of an increasingly sophisticated database. Lacking these abilities, the geological engineer logically shifts to administration, quality-control, and a less scientifically oriented practice. Many examples demonstrate this "ceiling" on the thought process and interpretation of observed data, a ceiling that has affected project actions and has even led to failures. However, geological engineering can be an excellent background for advanced-level training in the geosciences and Environmental/Engineering Geology practice, as many prominent practitioners can attest.

Obviously, all geoscience-oriented practitioners are not wholly qualified for any critical task in the broad field of practice. However, experience has clearly shown that the mature geoscientist with natural abilities is more likely to be capable of both locating and interpreting the specific geologic data required for project purposes. The advanced level of geological training invariably strengthens a geoscience practitioner's capability for unraveling the complex geological issues, and for becoming a field leader. A strong orientation toward field observations, combined with mapping skills, documentation, and ability to interpret the data are stand-out attributes.

Although practice requires a broad knowledge of other disciplines it is a team player effort with other specialists. Engineering geology is not a hybrid profession (both engineering and geology), as some suggest, any more than a physician using sophisticated engineering and geology-like techniques to examine your heart action or body is an engineering medico. All specialists today must utilize a similar broad-based approach.

Faculties should include major professors offering departmental Environmental/Engineering Geology options. One—and preferably two—of the courses required in curricula for an undergraduate geoscience degree should be offered. In the past, the major professor(s) invariably taught one or more courses such as physical geology, structural geology, sedimentary geology, petrology, geomorphology, field geology, and more recently seismology, physical properties of Earth, and other geophysical and hydrological subjects critical to engineering geology practice.

If a "critical mass" of three or four specialists is available to serve the broad-based engineering geology option, each participant should be anchored to at least one core-curriculum course. This interdepartmental dependence invariably contributes to a positive attitude toward the Environmental/Engineering Geology courses and options, at both undergraduate and advanced levels.

Some environmental/engineering geologists feel that they are too technically sophisticated to teach a building-block course in the undergraduate program. Balderdash—the neophyte geologist's attitude is largely molded in the early years of training, and major faculty should participate in that training. Departments with engineering geology options should reexamine teaching assignments and, if needed, insist that the engineering geology specialists teach one or more fundamental core courses. Too often, such specialists teach a series of horizontal courses, Engineering Geology 1, 2, 3, Advanced Engineering Geology, etc.

A myth supported by some academics claims that the student chooses a school or program because the faculty specialists in Environmental/Engineering Geology are registered and licensed or certified. This is a self-serving attitude at some schools and not a valid factor. Regardless of a license or registration, caliber of the geoscience coursework and training is demonstrably more important as a background for the critical decisions in practice.

The discipline's designation as *engineering geology* has persisted since 1874, when Professor F. V. Hochstetter of Technische Hochschule, Vienna, first used the term in a lecture. The misidentification of our practice as "engineering" over the intervening years has been both unfortunate and, often, misleading. As has been accomplished by other main branches of applied geosciences, engineering geology should improve on the discipline's name and thereby gain an improved acceptance throughout the geoscience community. For example, ground-water geologists accomplished this by renaming the discipline "hydrogeology," and mining geology became "mineral geochemistry" and "exploration." The term "environmental" implying the natural hazards of physical geology processes basic to engineering geology practice is yet another illustration of terminology that sparkles.

In the 1920s, Professor Stini in Europe designated the engineering geology discipline as "construction geology," but few in the United States had accepted this terminology by the 1960s. In 1962, in an attempt to refocus the meaning of engineering geology practice and return it to its scientific basis, I suggested the name "physico-geology," which eliminates certain misconceptions surrounding the adjective "engineering." Although our colleagues understand the misconceptions, the public and many geoscientists do not—a major part of our dilemma. In the 30 years since, few have expressed a liking for "physico-geology" or the more recent suggestion of "geotechnical geology."

Engineering Geology Division activities should redirect and increase the awareness of the geoscience community to the full meaning and scope of engineering geology practice—a dominantly geoscience-oriented discipline. Related actions should include the participation of the emerging practitioners who teach the engineering geology courses at more than 60 schools in the United States and four in Canada, and the large group of experienced practitioners in government, state, and local agencies, private firms, and industry. Division activities must serve the emerging new subdisciplines and elements of *modern* engineering geology practice.

Everyone recognizes that engineering geology practice of the future will be notably different and more complex than that of past practitioners and specialists. This Division of GSA represents the dominant *geologically oriented* group of engineering geologists in the world and is ideally positioned to be the leader in the transitions ahead.

Again, my thanks for the Burwell Award and for the privilege of sharing my views.

Presentation of the GEORGE P. WOOLLARD AWARD to ROB VAN DER VOO

Citation by JOHN W. GEISSMAN

It is with sincere pleasure that I read the citation, on behalf of the Geophysics Division, to our 1992 George Woollard awardee, Rob Van der Voo, professor of Geological Sciences at the University of Michigan. The award is presented in recognition of outstanding contributions to the geological sciences through applications of the principles and techniques of geophysics. The Division chose an exceptionally qualified and most appropriate recipient! Rob Van der Voo's numerous accomplishments, in part working with a broad spectrum of colleagues and students, have led to important advances in the field of paleomagnetism and these in turn have substantially influenced many aspects of recent to current research in the geological sciences.

Rob received his Ph.D. from the University of Utrecht in 1969. A summer position with Shell Oil first brought him to the United States in 1968, and he returned when Michigan was most fortunate to lure him to the department in 1970, where he has been ever since. A Fellow of the Geological Society of America and the American Geophysical Union, and, until July 1992, president of the Geomagnetism and Paleomagnetism Section of AGU, he has authored or co-authored over 150 scientific contributions.

Considering the army of students and colleagues who have studied under and worked with Rob, I am indeed fortunate to be presenting this citation. Please permit me to briefly expound on my history with our 1992 Woollard awardee. He may not remember this, but I first met Rob over half my life ago. In the spring of 1971, while a visiting professor at Michigan, he was a "guest" lecturer for Professor Frank Rhodes's Historical Geology course. Filling in for Frank Rhodes (now president of Cornell University) is "a tough act to follow," so to speak, yet his 50-minute discussion of the paleomagnetic evidence for the rotation of the Iberian Peninsula, the subject of his dissertation work, captivated me and my many colleagues and generated a lively discussion at the end of the period. Years passed before I became more closely acquainted with Rob. He was engrossed in assembling a quality laboratory in paleomagnetism at Michigan, one of the first analytical laboratories in the department at that time, working with some most capable graduate students, notably Rowland French, Donna Jurdy, and Tom Crough. In the summer of 1973, Rob taught part of Michigan's Field Geology course, at Camp Davis, in northwest Wyoming. Here he, Jack Dorr, Ken Grubbs, and Tom Crough planned the essence of the famous paleomagnetic study of impingement-related vertical axis rotations in the Wyoming overthrust belt. Realizing that some of the sampling needed to be done in rather remote areas, Rob and Jack sent me and Jack Kostowny, another field-course student, on a so-called "advanced project," about which we were very happy, until we realized that we were headed for a most rugged part of the Hoback Range, with no trails to guide our way and simply a spot on the map where good outcrops of the appropriate age Triassic strata were located, in the middle of a cliff of several hundred meters of relief!

The next year I was the TA for the field course. Toward the end of the summer, an unusual set of circumstances, not the least of which involved scheming by Rob and Michigan's economic geologist Bill Kelly with numerous Anaconda geologists in Butte, Montana, launched my graduate work in paleomagnetism, first at Butte, then Yerington, Nevada. Over this time period, I appreciated the tremendous level of freedom Rob allowed in numerous research pursuits, from studying the magnetism of single, oriented feldspar grains, to, with Doyle Watts, a Ph.D. student studying the paleomagnetism of lower Paleozoic sedimentary rocks in North America, designing and launching numerous intricate helium balloons out the window of the paleomagnetism laboratory on the fourth floor of the C. C. Little Building.

In the summer of 1980, I was fortunate to be able to accept a teaching position in structural geology and tectonics at the Colorado School of Mines. At that time, I was Rob's second student in paleomagnetism, Donna Jurdy being the first, to accept an academic position. I sensed that he was very happy about it. My experiences over the next year prompted me to recall, on numerous occasions, the comment Mark Twain made concerning his father—how amazed he was that the "old man" would have learned so very much over the short period of time that Twain was gone on each of his many "adventures"!

Early in his tenure at Michigan, Rob began to tackle what has proved to be a most complicated subject—the Paleozoic paleomagnetism of North America. What implications this work would have! Two decades later, major portions of the apparent polar wander path of North America are now far better, albeit still not completely, understood, thanks largely to Rob and many associated students working in the Michigan laboratory, including Rowland French, Doyle Watts, Chad McCabe, Doug Elmore, Rex Johnson, Mike Jackson, Roberto Molina, and Joe Meert. From these endeavors came further confirmation that the late Paleozoic was a time of widespread remagnetization. Rather than gloom and attending disappointment, new research pursuits have blossomed! The association between remagnetization, diagenetic changes in sedimentary rocks, and relations to fluid-migration and tectonic activity continues to be a major topic of study, in the Michigan laboratory and elsewhere. Fortunately, not all Paleozoic rocks have been remagnetized, as several carefully documented studies have shown. With an increasingly robust paleomagnetic data base of primary magnetizations of Paleozoic age, Rob and colleagues have turned their attention, more and more, to reconstructing Iapetus in the Paleozoic, thus placing major constraints on the history of the Appalachian orogenic belt.

His most recent endeavors have focused on compilations and critical assessments of the global paleomagnetic data base for the Phanerozoic. Efforts with Mike McElhinny and Jo Lock came to fruition last year with the appearance of the first edition of the computer-based Global Paleomagnetic Database. Several major summary articles, dealing in particular with Phanerozoic reconstructions of the Atlantic-bordering continents, have appeared over the past few years. Finally, his book *The Paleomagnetism of the Atlantis, Tethys, and Iapetus Oceans* will soon be released by Cambridge University Press.

We should not overlook that many of the endeavors described above were taking place while another side of Rob—his deep devotion to the workings of the Department of Geological Sciences at Michigan—was shining through. He was chair of the department from 1981 to 1988. As if that was not enough, after returning from a one-year sabbatical in Spain, where he wrote most of his new book, he accepted the chair for another term (or terms???).

Perhaps the mark of an outstanding advisor, as well as an exceptional individual, is that you continue to remain friends as well as professional colleagues and that he or she remains someone you can learn from as well as share your discoveries and/or concerns with, long after a thesis or dissertation is finished. Rob Van der Voo, I am proud and elated, on behalf of the Geological Society of America, to present you with the 1992 George P. Woollard award.

Response by ROB VAN DER VOO

Thank you, John, for your flattering citation. It is a great honor to be the recipient of the George P. Woollard Award and I thank the Geological Society of America and its Geophysics Division for this recognition.

I do remember the 1971 lecture that John referred to, but not that he was there, although soon afterward he made his undergraduate presence felt. What I found most remarkable about student reaction to my discourse on the rotation of the Iberian Peninsula was the enthusiasm and interest shown by the Michigan students. Having arrived fresh from Europe, I was not used to this. I also remember one student coming up afterward and telling me that "the lecture was very clear and interesting, but what was magnetism anyway?"

She had not heard of it before!

The two decades since that time have been characterized for me by many delightful interactions with students, graduate and undergraduate alike, and even some high-school students, and of course also by interactions with postdoctoral fellows and faculty colleagues. These interactions and subsequent co-authorships have been enormously gratifying. It is safe to say that I would not be here without them; the energy, vitality, ability, skills, responsiveness, curiosity, drive, ambitions, and integrity of my many student and postdoctoral co-workers have made my career what it is. Invisibly but indispensably and vicariously, they share this honor today with me. And, of course, John is one of the premier representatives of this illustrious group.

I had the good fortune of being a geology student from 1958 through 1969, which was a most exciting time. My principal professors in geology, Martin Rutten and Rein van Bemmelen, were open-minded about continental drift in the 1950s. They had invited Warren Carey to present a lecture at Utrecht, and I was simply fascinated. The opening of the Bay of Biscay story, to this day, gets my heart racing faster than even a good Verdi opera. While working on Spanish rocks, I had a chance to collect in, or work on rocks from, a variety of other circum-Mediterranean locales which led to a coherent paleomagnetic picture of the western apex of the Tethys Ocean by the time my thesis was done. After going to Michigan and a brief escapade in seismology, I decided that pre-Mesozoic tectonics was the upcoming thing, and in retrospect I think this was correct. Theoretical, mathematical, and geometrical aspects of geomagnetism or paleomagnetism have kept me engaged as well, and true polar wander is still a subject that fascinates me.

The nice thing about the Woollard award is that it exists! By that I mean that there is annual recognition of the fact that geophysics and geology are intimately connected. I have always been grateful for this interdisciplinary tradition that is so strong in the United States, and I have benefited tremendously from it. Some European countries and Canada have traditions by which geophysics is more closely allied with physics; this liaison has its own reward, but I would not for one moment have been happy in such a tradition. The contented marriage of geology and geophysics is most evident in the field of tectonics, which has been and still is one of the driving forces for much paleomagnetic research.

I would also like to comment on the remarkable opportunities that exist in this continent for someone who desires success in teaching and research. The structure of our universities, the traditions of scholarship, the funding opportunities from federal and industry sources, the strength of the scientific societies, and the opportunities for fertile collaboration with colleagues from the same or different institutions all conspire to provide an environment that is unsurpassed in the world. Competitive yet collaborative, complicated but fair, democratic yet relatively unbureaucratic, the system works well for those who are willing to invest much time and energy to solve the problems of education and to further the quest for knowledge. The most recent announcements of a shift in direction of the National Science Foundation toward more applied research and technology transfer could pose a serious threat to this system, but this is not the occasion to start crying wolf.

Let me end by thanking those anonymous colleagues who nominated me, my colleagues at Michigan and elsewhere who have encouraged me, and my former students who have energized me, even though they are too numerous to be mentioned individually by name. My role models deserve special mention—as unlikely as these senior scientists may think of themselves as such. Allan Cox, Neil Opdyke, Martin Rutten, Ted Irving, Ike Smith, Bill Kelly, and Henry Pollack: you made a difference at one point in time or another. I thank all of you.

Presentation of the HISTORY OF GEOLOGY DIVISION AWARD to MICHELE L. ALDRICH

Citation by ELLIS L. YOCHELSON

Citationist and awardee have in common the same middle initial. This is a relatively slight base on which to build a professional relationship, let alone a friendship. Accordingly, it came as a bit of a shock to me to be asked to bask in Michele's limelight. One surprising aspect of her choice is that I am a man. If there is one person in the field of history of geology who is outspoken about women's rights, it is Michele Aldrich. Her outspokenness, however, is a shout for proper recognition of the accomplishments of female geologists (and females in other fields of science). It is emphatically not grounded in the proposition that a woman scientist is automatically important simply because she is a woman. Michele does not suffer fools gladly—to drag out a real chestnut—regardless of their sex. Notwithstanding that, again regardless of the gender involved, if one is a beginner in the field of history of science and has a sincere interest in the subject, Michele will spend time and an effort to steer a tyro—to drag out a good Scrabble word and attach it to a chestnut—in the proper direction.

Michele Aldrich's roots go back to Seattle, Washington—she claims that her name is spelled with one "l" because her family had planned on a boy—but she spent her teen-age years in the San Francisco Bay area. Thus, it is understandable that her university of choice was at Berkeley, from which institution she received a B.A. in Geology 28 years ago; at the time, her emphasis was on geochemistry. Like so many Californians, she headed east, but her goal was not yet Washington, D.C. Rather, it was the university of Texas at Austin. Mark Aldrich, who had met her at Berkeley, and has since made a name in economics and economic history, also moved to Texas. They have celebrated 27 years of matrimony and have logged thousands of miles in their classic commuting marriage.

Geology in the strict sense was not for the new graduate, but history of science, with emphasis on the Earth, was. A decade after Berkeley, Dr. Aldrich came newly minted—t.d.o.a.r.c.—out of the academic mill. William H. Goetzmann, of western exploration fame, was her principal advisor, but her thesis subject, the early years of the New York State Geological Survey, was one for which she had to develop her own ideas and insights with minimum guidance. In later years, Professor Goetzmann, who ought to be a good judge of how his own graduate student met his expectation, wrote about Michele: "She ranks as one of this or any other generation's outstanding researchers, and over the years she has set a new standard for scholarly friendship" (*Looking Far North: The Harriman Expedition to Alaska*, by William H. Goetzmann and Kay Sloan, Viking Press, New York, 1982).

Like so many Texans, Dr. Aldrich now headed for Washington, D.C., and spent a year as an assistant editor with the Joseph Henry Papers, located in the Arts & Industries Building of the Smithsonian Institution. Washington was not a totally new area, for among her many positions while in graduate school, she had been a research assistant with the Geological Survey on the national atlas project and, later, with the Smithsonian Institution Archives. Like so many graduates, regardless of their excellent qualifications, Michele faced a difficult employment market and spent the next two years in temporary positions at a variety of libraries, searching for letters of Aaron Burr and unpublished material on women in America, a wide spectrum, but Michele covers well all parts of a wide spectrum.

Three years after graduate school, Dr. Aldrich connected permanently with the American Association for the Advancement of Science. For six years, she was project director of Women in Science. She then moved on to being AAAS archivist and had two years exclusively for doing what she does exceptionally well. The organization recognized talent and loaded on the additional position of manager of computer services. For the past two years, Michele Aldrich has been Director of Information Services for AAAS.

By now, one may properly ask what this catalogue has to do with a citation. One fundamental reason for an award is a recognition of hard work. For five years, Michele Aldrich was in charge of publicity for the History of Science Society, which was as long a term as she has been secretary-treasurer of the Forum for History of Science in America. From 1982 onward, the editor of the GSA History of Geology Division newsletter has been the same M. L. Aldrich. For even longer, she has been the AAAS staff liaison with Section L, History of Science, with the Pacific Division of AAAS. Along the way, Dr. Aldrich has also been president of the History of Earth Sciences Society. None of the manifold offices she has held was honorific.

That is nice, but lots of people work hard in this world, and they are not rewarded or given awards. The reason for her selection by this Division is that Michele Aldrich is an outstanding historian of geology. She has investigated people, from William Barton Rogers on the East Coast to John B. Trask on the West Coast. She has investigated ideas, from the relationship of railroads to geology to the transfer of the concept of Gondwanaland from the zoological literature to the geological. She has considered science policy from the time of James Hall to that of Harry Truman. As noted, this is a wide spectrum.

Michele has not published quite so much as some of her academic colleagues. She does have three volumes of reports for AAAS and a volume of *The Joseph Henry Papers* in her portfolio. Those who read *Earth Sciences History* will note frequent articles by her, as well as an issue of volume 4 on "Plate Tectonics and Biogeography," coedited with her colleague Alan Leviton. Her other papers have appeared in a variety of journals.

There is another aspect to Michele Aldrich's career, and it is that of critic. If one is presenting a paper, and she is in the audience, the facts presented had better be correct. One can argue interpretations and conclusions, but facts are facts. In his comparison of science to building a wall, Vannevar Bush placed considerable emphasis on the importance of those who make certain that each stone is properly emplaced and that the wall is straight and true. We are all better off for her concern about proper historiography in the subject we all enjoy. If my facts are correct, it is thus appropriate that I present the awardee for 1992, Dr. Michele L. Aldrich.

Response of MICHELE L. ALDRICH

Thank you, Ellis. I appreciate your citation and the presentation of this award from the Geological Society of America. Getting to this honor was not a straight road. Like many other high school students of the 1950s, I was encouraged by teachers to go into science as a patriotic response to the Sputnik crisis, and at first was unaware of the humanities as an option. During the early 1960s at Berkeley, geology seemed like a perfect major for a student generally interested in science because it required taking all the other sciences. The University of California at that time prevented its students from early specialization—four courses were mandated in the social sciences and four courses in the humanities, and history courses could be used for both categories. A blessing for me, because I hated geology.

During the four years I majored in geology, I got the impression that this was a highly competitive discipline where one worked in isolation. History at Berkeley in those years was the opposite, and when I enrolled in graduate school at the University of Texas at Austin, I found the geology department there was more like history than Berkeley geology. Those graduate years, and historical study of geology since then, have persuaded me that the Berkeley image of the science was anomalous. After the student rebellions of the 60s and 70s, Berkeley too changed, and the geology department of today seems more collegial and more welcoming of undergraduates. But my skepticism about the practice of geology in certain times, places, and circumstances remains, and in investigating such topics as the history of women in geology, it has served me well.

In the late 1960s, the history of American science had its problems. Historians of science regarded early American work as colonial; whatever American scientists did of merit was derivative from European science, and whatever American scientists did that was novel or unique was of less value than European innovations. Some American historians without scientific training blundered in their judgment of American science, such as George Daniels's contemptuous putdown of "Baconianism." William Goetzmann at the University of Texas taught his graduate students to put American science in its national context, as had already been done by scholars of American literature, and material culture. He also sent us off to introduce ourselves to the practitioners in our field, and they in turn sent us on to others. Before I left graduate school I had met George

White and Ellis Yochelson, and they made sure I visited Cecil Schneer and Claude Albritton.

I took six years to write my dissertation (I was known as "chapter-a-year Aldrich" back in Texas). I had moved to New England because Mark had a teaching job at Smith, and I became involved in the women's movement there. This activism led me to an abiding interest in the history of women in geology. Several other scholars are now also working on this topic, in which lots of important questions remain. Women appear in early American geology as scientific illustrators: did this indirect entry through socially acceptable "women's work" speed or retard the eventual integration of women into professional, research-oriented geological science? Was the American experience unique—were women as geological collectors more important in England than in the United States? How does the experience of women in American geology compare to that of minority ethnic groups seeking entry to the field?

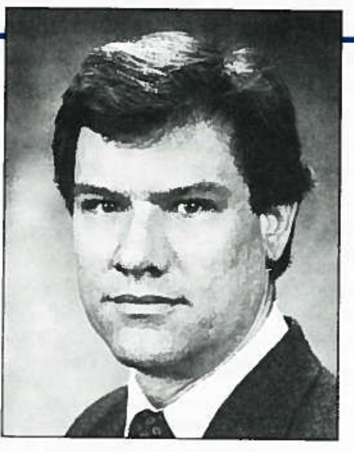
To geologists, I seem to be a historian, and to historians, a geologist. I once unfolded an AAPG highway map of the South at a conference on antebellum science as part of my commentary on a paper, and the other participants treated this as deviant documentation ... not quite fair to the author whose paper I critiqued. Historians of science look to other historians, not to scientists, for validation of their work. As Mott Greene remarked, they generalize from the history of chemistry, physics, or biology to all of science; if geology doesn't fit the model, it is set aside as an anomaly.

For eight years I have tried to bring historical methods of research and analysis to geologists through the History of Geology Division. The people most receptive to my message had already heard it elsewhere, from historical colleagues at their university or through reading. I despair of the others, and ask that if they will not learn history, they at least apply the same canons of evidence and argument that they use in geology. It is asking little to avoid generalizing from a sample of one, which some geologists have done in extrapolating from a biographical study of one scientist to his or her entire era. It is asking little to check the secondary literature before proclaiming that one has discovered that Darwin was a geologist. These are mistakes the authors would not make as scientists ... or do I presume too much?

While there is much to grumble about in the history of science as practiced today by historians or scientists, some of the old questions have been set aside. Scholars are no longer preoccupied with whether American science was better or worse than, ahead of or behind, science elsewhere at any given time. The question itself became unimportant, regardless of the answer. This is not to say geology's internationalism puts its practice above nationalistic concerns; that would be naive and counter to many historical examples to the contrary, as any recent writer on Murchison can attest.

Despite my complaints, the study of the history of geology has been personally very rewarding. The vision I had of geology from my undergraduate training has changed not only from watching modern geologists in other settings like Texas, but also from historical examples such as the New York Survey, where highly fractious and strong-willed individuals studied and fought to achieve a synthesis and understanding of their natural world. In this regard, the history of geology mirrors the practice of science and of history in their best senses. It is an endeavor of great instruction and delight.

Presentation of the O. E. MEINZER AWARD to CRAIG M. BETHKE



Citation by CHRISTOPHER E. NEUZIL

Ladies and gentlemen, members of the Division, and guests, I am very pleased to have the opportunity to acquaint you with the winner of the 1992 Meinzer award, Craig Bethke. No doubt many of you are already familiar with his work. What I will attempt to do here is to put Craig's work in perspective and explain why it is significant. Before doing that, though, it is appropriate to say a few words about Craig himself.

In the jargon of psychologists, Craig was "imprinted" with geological thinking at an early age. His dad, Phil Bethke, is a well known economic geologist at the USGS. Indeed, Phil is not present because he is presiding over the Society of Economic Geologists luncheon in a nearby room.

Craig apparently acquired Phil's enthusiasm for geology, because he majored in earth sciences at Dartmouth. While an undergraduate, he got his hands dirty doing grunt work in a mine in Silverton, Colorado. We know, therefore, that Craig has come in contact with real rocks at least once in his career. Although he enjoyed working in the mine very much, he has, as far as I know, stayed away from similar situations since then. During and after his undergraduate studies, he worked with Hu Barnes at Penn State and also had positions at Exxon and Arco. The latter assignments gave him insight into the perspective of petroleum geologists, an insight that later allowed him to exert an important influence in that sphere.

Craig next headed for the University of Illinois. Pat Domenico, an earlier winner of the Meinzer Award, was then at Illinois, but left about the time Craig arrived. Craig stayed, however, and in a largely self-guided fashion completed his Ph.D. in 1985. His thesis concerned the role of fluid transport in the creation of Mississippi Valley–type ores. Before Craig completed his degree, Illinois hired him as a lecturer, and on completing his Ph.D. he became an assistant professor. This rapid and unconventional progress set the stage for succeeding achievements. In short order he was named Presidential Young Investigator (1986) and received the Lindgren Award from the Society of Economic Geologists (1987). More recently, he was a visiting professor at the Ecole des Mines in France (1991), and last August was made a full professor at Illinois.

As I stated earlier, I want to briefly describe Craig's work and explain why he is an excellent choice for the Meinzer Award. Perhaps the thing that impresses me most about Craig's work is that while making important contributions to the science of hydrogeology, he also demonstrated its broad implications for understanding geologic processes, and did this for a wide cross section of scientists. His work has gained the attention of researchers in disciplines that before had not paid all that much attention to hydrogeology.

In a 1985 paper on the Illinois Basin, Craig analyzed flow in compacting sediments. As most of you know, hydrogeologists have tended to approach this type of problem with tools derived from soil and rock mechanics, describing compaction in classical terms, using quantities like compressibility. Craig chose instead to cast the problem in a different way, approaching it in terms of porosity loss with depth. These approaches are essentially equivalent, but Craig's conceptual framework greatly enhanced the acceptance of his ideas by the petroleum community. That's because petroleum geologists are much more comfortable with the concept of porosity loss, which is what one can detect by logging deep boreholes, than with the more hydrogeological concepts of effective stress and compressibility. I might add that the numerical simulator Craig wrote for this work, known as BASIN2, continues to be very widely used in petroleum exploration.

In 1986, Craig published a significant paper derived from his Ph.D. research. This work also explored compaction of the Illinois Basin, but in this case examined it as a mechanism for transporting the heat and chemical mass that created MVT ores. Craig concluded that compaction was insufficient but that topographically driven flow was a viable mechanism for the necessary rates of transport. This paper attracted a great deal of attention among economic geologists and, I would argue, significantly influenced their thinking.

We come now to two more recent papers, which are the basis of the award we are recognizing today. In the first, with co-authors Harrison, Upson, and Altaner, Craig described the simulation of fluid flow in evolving basins using high-speed parallel-vector computing. One of the applications it described was a new analysis of the development of geopressure in the Gulf Coast, which is a problem of long-standing interest to hydrogeologists. The results showcased the superb computing capabilities Craig has developed for his research and presented an excellent overview of the importance of basin hydrologic processes. Significantly, it was able to reach an exceptionally broad scientific audience because it appeared as a full-length article in the journal *Science*. In the second paper, published in *Geologische Rundschau,* Craig provides a very nicely organized synthesis of flow in basins, summarizes the current state of knowledge, and enumerates many of the most significant problems remaining in this area. This paper presents a very insightful synthesis of many aspects of the problem, especially petroleum migration, regional properties, and diagenesis.

This brief outline only touches on Craig's work, which also encompasses areas such as geochemistry and clay mineralogy. However, I think you will agree with me that today's recognition is well deserved. Ladies and gentlemen, I present the winner of the 1992 O. E. Meinzer Award, Craig Bethke.

Response by CRAIG M. BETHKE

Thank you, Chris, for your gracious words. I accept with great honor the Meinzer Award for 1992. At Hydrogeology Division luncheons over the past decade, I have watched the authors of papers that as a graduate student I read again and again stand in my position today. I must admit to being in awe. Fortunately, I have my family to put things in perspective.

When my wife Abby told her mother that I had received an award, my mother-in-law asked what it was and how I won it. Abby explained that it was a silver bowl and that I won it by working hard. My mother-in-law asked whether, if I worked really hard, I would get punch glasses to go with it. My father-in-law was disturbed to find out that I would pass on the bowl to a new awardee next year. He thought I should ask if they might not retire it if I won it three years in a row.

The difficulty in accepting an award such as this is that the credit is really due to a long list of students, employees, colleagues, and collaborators who have gone out of their way to offer assistance and, against all odds, keep me on the right track. I have not always made it easy for these people. Asked what is it like to be my student, Tom Corbet once said, "Well, he's not easy to understand, but he's hard to work with."

I cannot possibly thank all of these people today, but there are several that I would like to mention. Two professors taught classes that changed my life. When I was an undergraduate at Dartmouth, Bob Reynolds introduced me to theoretical geochemistry and mineralogy. Working with him my senior year to calculate X-ray diffraction patterns of disordered crystallites, I learned of the powers and limitations of numerical modeling. Turgay Ertekin, a professor of mineral engineering at Penn State, taught a course sequence on fluid flow and numerical modeling so thorough and exacting that I still refer to my course notes in preference to any published text.

My interest in numerical modeling had started earlier. When as an undergraduate I had run out of tuition money, Hu Barnes hired me as a laboratory assistant at Penn State. I was supposed to run the mass spectrometer, but thanks to a failure of the faculty to supervise me carefully enough, I became involved with computing. Working with punch cards, and with much help, I wrote my first computer program, SOLUPLOT, which calculates redox-pH diagrams. I still remember carrying my program in a long cardboard box to the mainframe in the computer center each time I wanted to run it. If I had dropped the box and scattered the cards, all would have been lost. Today SOLUPLOT runs easily on computers found in stores that sell refrigerators and ranges.

After I graduated, Bob Pottorf hired me at Exxon Production Research as a summer employee. The "summer" ended up lasting eight months. At EPR I learned to appreciate the complexity of fluid flow in sedimentary basins, and the need in basin hydrology to develop more quantitative tools. I was hooked on hydrogeology before I started graduate school.

At Illinois, Jim Kirkpatrick, an igneous petrologist and mineral physicist, had the courage to take on a student who said he wanted to study, of all things, the groundwater hydrology of sedimentary basins. Even before I had written my thesis, he encouraged me to apply for a faculty position there. I am not sure how he decided that an awkward graduate student, one whose brother once described him as a "slide-rule–carrying turbo-geek," would make an adequate professor.

Most of all, I have been fortunate to work in a field of study whose scholars are quick to share ideas and disseminate their data without hesitation. An award from peers such as these is the highest possible form of recognition. Thank you very much.

Publication Citations—O. E. Meinzer Award
by Alan Dutton

The Hydrogeology Division was organized in 1960 and established by the GSA in 1964 to bring together scientists interested in hydrogeology, to facilitate the presentation and discussion of their problems and ideas, to promote research and the publication of results on hydrogeologic studies, and to advise and assist the officers and committees of the Society in matters pertaining to hydrogeology.

As part of its promotion of the science of hydrogeology, the Division established the O. E. Meinzer Award, to be made to the author or authors of a published paper or body of papers of distinction that advances the science of hydrogeology or a related field such as water hydraulics or geochemistry. The award honors O. E. Meinzer, architect of modern hydrogeology and third Chief of the U.S. Geological Survey Ground-Water Division. The O. E. Meinzer Award generally is made annually, but it may be withheld if no suitable paper is selected, as in 1982. To be considered for the award, a paper or body of papers must (1) be in hydrogeology or a closely related field and (2) have been published not more than five years prior to its selection. The award includes a certificate and custody of the silver Revere-style Birdsall Bowl, a gift of John Birdsall. Names of the awardees are engraved on the bowl, and each awardee receives a miniature silver replica engraved with his or her name.

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Presentation of the
G. K. GILBERT AWARD
to
JOHN ARMSTEAD WOOD

Citation by
HARRY Y. McSWEEN, JR.

Distilled insights can often be found in the unlikeliest of places, such as on garish bumper stickers and worn out T-shirts. One of my favorite examples is a cartoon-like logo drawn by John Wood, whom we honor as this year's recipient of the G. K. Gilbert Award for planetary geology. A few years ago, John presented T-shirts embossed with this logo to all those students and postdocs who had worked in his laboratory. It shows a geologist, recognizable by his scuffed boots, peering intently through a hand lens at a chunk of the starry firmament that he has just dislodged with his rock hammer. I suppose that the geologist is intended to represent anyone who dares to treat extraterrestrial materials as rocks, but in fact he bears more than passing resemblance to a younger John Wood. This logo not only defines John's scientific focus, but also provides insight into why he has been singled out as the first petrologist to receive the Gilbert Award. In an interdisciplinary field where extraterrestrial petrology intertwines with astrogeology, astrophysics, and cosmochemistry, John has never lost sight of the goal of transforming astronomical objects into geological worlds.

John Wood grew up in Virginia, Florida, and Georgia and completed his undergraduate geology studies at Virginia Tech. As a graduate student at MIT, he first became interested in the petrologic properties of meteorites, about which rather little was known at that time. After receiving his Ph.D. in 1958, John spent a postdoctoral year at Cambridge University, where he continued his study of chondrites. This work culminated in his recognition of metamorphism as an important process in chondrites and its later incorporation into the now universally used classification scheme for chondritic meteorites. Subsequently John spent several years as a research associate at the University of Chicago, where he formulated a computational model for nickel diffusion in meteoritic iron. He was able to use nickel diffusion gradients to assess the cooling rates of the parent bodies for iron meteorites and ordinary chondrites. This work not only demonstrated that meteorites were derived from asteroidal-sized parent bodies, but also was a crucial first step in unraveling the thermal histories of these objects.

In 1965, John accepted a position as geologist at the Smithsonian Astrophysical Observatory in Cambridge, Massachusetts, where he has remained to this day. During the next few years he applied the electron microprobe to various meteorite problems, including the properties and possible origin of chondrules, a vexing challenge that remains one of his current interests. During this period John also coauthored a descriptive paper on refractory condensates in the recently fallen Allende meteorite, in which the proposal was first made that such inclusions were early condensates from the nebula. These studies also served to prepare his laboratory for the arrival of the Apollo lunar samples. John had the perspicacity to request a sample of Apollo 11 regolith rather than rock, reasoning that soil would contain diverse materials and possibly even impact ejecta from the lunar highlands. In his tiny vial of lunar soil, John and his colleagues found fragments of feldspar-rich rocks, which they inferred were derived from a highlands composed of anorthosite. From this insight, they conjured up a startling model for the formation of the ancient lunar crust—fractional crystallization of a global magma ocean. This idea, first published in 1970, was readily embraced by most of the scientific community, and it remains a cornerstone of lunar geologic history today. Since then, John and his colleagues have published numerous other important petrologic papers that have profoundly influenced our understanding of the geology of the Moon.

Throughout his career, John has sought to use information gained from extraterrestrial materials to constrain the origin of the planets, the Moon, and the Solar System in general. From the properties of chondrites, he has argued that the solar nebula experienced transient events that allowed melting and distillation of solid matter, that it had regions of nonsolar (dust-rich) composition, and that accretion was prompt. In lunar samples he has found support for the idea that the Moon formed by collisional ejection of a significant part of the Moon's mass from the Earth. As a current member of the Magellan science team, he has used his petrological experience to speculate about metamorphism on the broiling surface of Venus and its global implications.

John's lunar and meteorite research has been previously recognized by NASA's Medal for Exceptional Achievement in 1973, the J. Lawrence Smith Award of the National Academy of Sciences in 1976, and the Leonard Medal of the Meteoritical Society in 1978. In 1991 he was elected to the National Academy of Sciences. He has also taught Solar System courses in the Department of Geological Sciences at Harvard University for many years, and in 1976 he was appointed Professor in the Practice of Geology. Any of his students will tell you that his teaching is as outstanding as his research. John has served as mentor for many graduate students and postdoctoral associates, and I consider myself singularly honored to have been his first doctoral student.

Of course, we don't give medals for the way scientists conduct their professional lives, but the awarding of medals is a suitable time for celebrating the person as well as the accomplishments. John has many qualities I greatly admire, and those of us who have been associated with him over the years have learned a great deal about how science should properly be done. Perhaps his most enduring legacy, though, not only to his students but to all of us, is his gift in communicating the excitement of the human enterprise we know as science to a world desensitized by science fiction. Let me use John's own words to illustrate. This is an excerpt from a recent letter to me describing the arrival of the Apollo 11 lunar samples: "It is impossible to convey to you how wonderful and priceless we, and others, considered a piece of the Moon to be in those days. You grew up in a world that had lunar samples in it; hell, we get them from Antarctica now—no big deal. There is no way you can understand how we were affected emotionally by having a piece of a whole different planet in our lab; it is even hard for me to remember the feeling, but I do remember that it was there and it affected us all." John has consistently been one of the most effective spokesmen for planetary science to the geological and lay communities. His many popular articles and review papers serve as a standard for understandable and enticing scientific writing that is rarely equaled. His two books, *Meteorites and the Origin of Planets* (McGraw-Hill, 1968) and *The Solar System* (Prentice-Hall, 1979), which were translated into Russian, Japanese, and German, have influenced a generation of geologists and educated thousands of nonscientists.

The GSA Planetary Geology Division is pleased to honor John Wood's many important petrological contributions to our understanding of the geology of the Solar System, and his effective communication of the discoveries of planetary science to a wide audience, through the 1992 G. K. Gilbert Award.

Response by
JOHN ARMSTEAD WOOD

Hap, Chairman Lucchitta, my other friends in the Planetary Geology Division, I am more grateful to you than I can say for this honor. There is no community whose approval means more to me. It seems traditional to provide a few words of personal history on an occasion like this.

I always had a romance about things long ago and far away, and this led me to major in geology in college. I found myself feeling cheated by my course in historical geology, though, because our text devoted 96% of its pages to the most recent 11% of geologic time. Especially insulting was the first chapter, with its mealymouthed review of shopworn theories of how the Earth was formed. As a student I had a naive dream that I would remedy this situation some day by writing a book titled *The Earth,* which would deal forthrightly with questions of the origin and early evolution of our planet. Of course, my own research would figure importantly in the book. I would even illustrate it myself, with Bill-Hartmann–style paintings of critical stages in the Earth's history. I was oblivious to the fact that even the title of my hypothetical book had been preempted by Harold Jeffreys.

My plan was to train up in hard-rock petrology, but I was diverted from this in my third year of graduate school when I discovered, in the Mineralogical Museum at Harvard University, a cubical rosewood chest, about seven inches on a side, containing an old set of thin sections of meteorites. The sections had cover glasses, glued on with dark yellow Canada balsam, and were on long, narrow glass slides. They came from a sizable, balanced collection of meteorites that had belonged to J. Lawrence Smith in the 19th century. Upon Smith's death in 1883, Harvard had purchased the collection, and a section had been made from every specimen. I borrowed the chest from Cliff Frondel, took it back to MIT, and stepped into the wonderful world of meteorite petrography. Things were never the same after that; I was hooked. Here, surely, was the key to understanding the origin of the planets. And in 1957, almost nobody was studying meteorites! This whole marvelous, rich field of research was mine for the taking!

The honor you do me with the G. K. Gilbert Award suggests I had some success in exploiting the opportunity. I feel undeserving, though, because I know in my heart how much my accomplishments resulted from luck and the help of other people, and how little from my own merit. I will develop this point by shifting into the format of Academy Award recipients, and thanking some of the people who gave me a lift.

I will start with Gordon J. F. MacDonald, my thesis advisor. Gordon and I were both more or less loners, so there was not a lot of communication between us; but Gordon thought the field of meteoritics and the early Solar System was important, and he endorsed an eleventh-hour shift of thesis topic, from a rather prosaic terrestrial problem that I had not been making much progress on, to meteorites. This was considered an unorthodox if not controversial thing for a geology student to do in those days, but Gordon supported it. Like my fellow graduate students, I observed Gordon's style closely and filed things away, in case I should be called upon some day to give the appearance of an eccentric genius.

Harry Hess—there's an OK geology name! Harry said my earliest work on chondrites was "breaking through to new ground," and he urged me to speak on it at the upcoming AGU meeting. "But the abstract deadline has passed," I said. That's all right, Harry said, he would get me on the program. And he did, in, of all things, a session on tectonophysics. The morning of May 4, 1959, found me in a small room in the General Services Administration building in Washington, D.C., telling a dozen or two bewildered geophysicists about the wonders of chondrites. At that time I was looking for support to spend a postdoctoral year at Cambridge University; Harry found it for me in, of all things, the AAPG Petroleum Research Fund.

Ed Anders. When I met him, Ed's brilliant early work on meteorites had catapulted him to the top of this unfolding research area, while my own first meteorite paper had been rejected by the *Journal of Petrology*. But Ed had read it, and he said enough kind and flattering things to scrape me off the floor. "*Journal of Petrology* is an absurd place to submit a meteorite paper," he said; "try *Geochimica et Cosmochimica Acta*!" Ed could have seen me as a potential competitor, or a nonentity, but instead he extended the hand of friendship.

Fred Whipple. Fred saw merit in the idea of studying the material properties of meteoritic objects, as well as their trajectories and ablational behavior in the atmosphere, and he hired me at the Smithsonian Astrophysical Observatory, for the same position I occupy today. I disappointed Fred by opting to study meteorites rather than cosmic dust, which is what interested him; but let it be said in my defense that this was 1960, and genuine cosmic dust did not become available for study until a decade later, when Don Brownlee discovered how to collect it.

I think I am the only meteoriticist who works surrounded by astrophysicists. I've learned a good deal from them. The most important thing is that they put their pants on one leg at a time, like everyone else. Many of my meteoritical colleagues seem not to understand this. My special insight gives me a competitive advantage in dealing with the important astrophysicial component of cosmochemistry.

I especially want to thank, but will not name, the young people who have worked with me. They have been an unfailing source of inspiration, and of course they do most of the hard work around the lab. I hate to reward Hap McSween for his affectionate citation by contradicting him, but I don't think I've given these youths nearly the amount of support they deserved. My policy has always been one of benign neglect, resting on a solid foundation of laziness. When these young colleagues have gone out in the world and impressed people, as they often have, it's because they're good, not because of any underpinnings I provided.

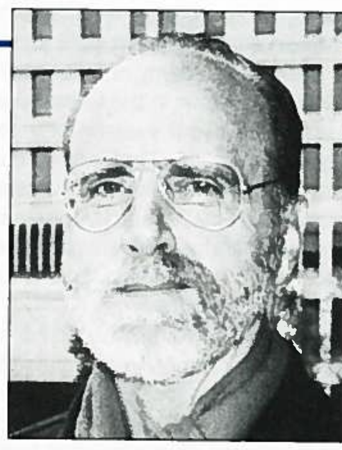
I am very grateful to Bill Quaide, whose invisible hand at NASA headquarters guided me into the Magellan program. Bill has been anonymously kind to many of us in this room, many times.

Most of all, I want to thank my parents: not only for the love and care parents are famous for, but for their exquisite sense of timing in conceiving me in 1931. This depression year must not have seemed an auspicious time to add another dependent to the family roster, but my parents shrewdly foresaw that starting my lifeline in 1932 would see me out of graduate school between wars (1958), at a time of unprecedented national prosperity and opportunity. In the 1950s the United States was the only major power not prostrate after a devastating world war. The U.S. had never been in such an advantaged position before, and it probably never will be again. Rightly or wrongly, social problems did not absorb as much of the nation's resources and energy as they do now. Education and research were expanding, and jobs (plural) were available. I was privileged to ride the crest of the Kennedy administration's emphasis on academic excellence in general and space research in particular. I benefited from the artificial competition in space research that developed between the U.S. and the USSR. And 1966 saw me perfectly positioned, at the Smithsonian Astrophysical Observatory, to propose to NASA to participate in the study of samples which that agency thought it was going to bring back from the Moon. (Perhaps not perfectly; it was probably detrimental to my chances that I had never given a minute's thought to the science of the Moon. However, this eventually turned out to be an asset, because it meant I had no preconceived notions about the Moon. Most of those who did were proven wrong by the Apollo data.)

All this hinged upon timing! Everyone should have such prescient parents. In addition to the professional benefits of timing just listed, I am also grateful for having been able to live most of my life in simpler times. Times when you could whang on outcrops with your geology pick; you could rinse a thin section with organic solvent and not have to give the used solvent a burial with full military honors in a toxic waste dump; you could shoot off firecrackers; you could burn the pile of autumn leaves you raked up; you could find a parking place for your car on the street, in downtown Boston; you could drive drunk, without a seat belt. And there were a number of other things you could do that you can't any more.

My thanks again to the award panel and all the rest of you for this honor. I'm having a very nice evening.

Presentation of the
KIRK BRYAN AWARD
to
R. DALE GUTHRIE



Citation by
TROY L. PÉWÉ

In 1979 a frozen carcass of an extinct giant bison was uncovered by mining operations in retransported loess of late Wisconsin age near Fairbanks, Alaska. The skin was covered with crystals of vivianite—a light-blue iron phosphate mineral. Here was a giant "Blue Babe" of antiquity—a mummified carcass 36,000 years old. The story of the life and times of this ancient animal is the basis of a thrilling and ingenious detective story of how it met its death and how it came to be preserved. The animal provides a unique window into ice-age life and the environment.

The outstanding book entitled *Frozen Fauna of the Mammoth Steppe, The Story of Blue Babe* by Professor R. Dale Guthrie of the University of Alaska at Fairbanks was published in 1990 by the University of Chicago Press and is the Kirk Bryan Award Winner for 1992.

But this book extends much further than the investigation of the bison.

The story of Blue Babe is the launch pad, the centerpiece to set the stage, for a detailed description and proof of the existence of a vast, cold, grassland environment that supported a diverse population of ice-age mammals and extended as an immense collar around the northern part of the world from Europe, across Asia, and the site of the present Bering Straits to Alaska and the Yukon. Guthrie carefully demonstrated that much of this unglaciated vast region was not covered with tundra or taiga forest, as it is today, but with an extinct grassland environment, unknown at the present time. Guthrie terms this the *Mammoth Steppe.*

It was in 1963 that a fresh Ph.D. from the University of Chicago, R. Dale Guthrie, was hired as a joint appointment by the Department of Geology and the Department of Zoology at the University of Alaska at Fairbanks. The geology department needed an expert to study the enormous and growing collection of Pleistocene mammal bones and especially to aid in the unraveling of the Quaternary geological and environmental history of mainly central Alaska. The Zoology Department needed a competent vertebrate paleontologist and zoologist in their rapidly expanding group.

Dale was a declared art major when he enrolled at the university, but after taking a biology course from a stimulating instructor, his lifework has concentrated on zoology, past and present. Art is still an active part, however, as evidenced by the abundant and informative illustrations in his award-winning book. His artwork admirably complements his paleontological enterprises. His sculptures of Pleistocene animals have been cast in bronze, and they grace some of the leading museums. It is not surprising to learn that one of his major art interests is the study and use of the European Paleolithic cave art of mammals of the extinct Mammoth Steppe. These cave drawings have supplied many details of Pleistocene life and the environment.

Over most of the past three decades, Dr. Guthrie has brilliantly described and interpreted the animals, and especially the environment of the last part of Pleistocene time in polar and subpolar areas around the world, notably Alaska. He is still at the University of Alaska, pursuing investigations of new mammal finds of early Pleistocene age.

Perhaps it would be well to briefly outline the answers to the questions that Guthrie raises. How could all these animals thrive in a habitat so dry that trees could not grow? How did they endure long winters in the severe winds and air temperature of 60 degrees below zero?

Guthrie proposed, and more than that, he demonstrated for the first time, that this land behind the north wind did exist and was, as he states, a giant of a land and a land of giants. It was a cold, arid, high productive grassland with a high carrying capacity—the Mammoth Steppe.

Palynologists have long known that there was a virtual absence of trees in the unglaciated north country during glacial episodes of the Pleistocene. Many of them believed that the present northern boggy, unproductive tundra was more expansive, and downplayed the existence of grasses and even the presence of a continuous fauna. They thought that the megafauna was practically limited to interglacials and interstadials.

Guthrie had to show that not only did an extensive fauna exist, but that only an arid, windy, grassy land was favorable to the fauna.

First, the easiest way to prove an extensive fauna is to look at the enormous number of bones preserved in all horizons of Wisconsin loess and alluvial deposits of the unglaciated north country of Siberia, Alaska, and the Yukon. For many years, the fossil mammal bones were collected by the tons at the height of the surface goldmining operations. In the summer of 1938, for example, 8008 catalogued specimens, weighing 8 tons, of mostly mammoth, bison, and horse were collected by Otto Geist in the Fairbanks area and shipped to the American Museum of Natural History in New York City. Second, most of the megafauna were grazers and lived on grass. Third, the vegetation preserved in the teeth and stomachs of the frozen fauna is mostly grasses. The mammoths did die with "buttercups in their mouths," because buttercups did grow on the Mammoth Steppe. Fourth, Guthrie shows that the vegetation of the present tundra and taiga is toxic and low in nutrients; much of it is poisonous. He shows that while less vegetation existed on the Mammoth Steppe, virtually all of it was edible. Fifth, this was a grassland, not the boggy tundra of today. For example, the feet of antelope, bison, and horses, unlike those of moose or caribou, are not suitable for boggy country, but for firm, dry terrain. Sixth, most of the large grazers of the Pleistocene in the Mammoth Steppe could not tolerate the snow depths of today. But low snowfall and wind-blown terrain were ideal. Although modern bison have been introduced into Alaska, they exist only where the winds keep the snow swept away and where dry, partially grassy areas are present, such as along the lower glacial Delta River in central Alaska. To summarize, Guthrie says that frozen fossils yield a detailed picture of a vast northern grassland and a diverse animal community that flourished during the Pleistocene. He brings to this book decades of experience, thought, and commitment about the climate, environment, and fauna, especially during the late Pleistocene.

Frozen Fauna of the Mammoth Steppe is the climax of his detailed investigations, starting in Alaska and spreading worldwide to include the whole northern area that existed mostly north of the ice sheets, from France to Asia to Alaska and the Yukon, a cold and grassy plain that supported the megafauna. Not only is the area immense, it is also truly circum-polar in scale. It definitely was not, as earlier suggested by others, a polar desert incapable of supporting a vast fauna.

The book provides a detailed description and determines the origin of a unique and *extinct* ecological system, the Mammoth Steppe. There is no modern counterpart of this environment today. Such a revelation can only lead to the next idea—certain geomorphological processes and landforms present then may not be forming today! This may have been the only time when cryoplanation terraces were actively forming. Finally, this book transcends the boundaries of many scientific disciplines and successfully integrates geology, paleontology, ecology, palynology, and archeology. It is indeed a tour de force.

Response by R. DALE GUTHRIE

Thank you Troy, members of the committee, ladies and gentlemen. In searching for ways to express some of my pleasure and surprise, I read what Kevin Scott, Arthur Dyke, and other people had to say when they stood here accepting this award, and I found in reading their speeches that we had much in common. First of all, there is a need to say something about this embarrassment of being selected out of a group of scientists who have certainly contributed as much as I, and, further, I must acknowledge how much my work depends on that of my colleagues. I don't have to expand on the nature of science as a cooperative and cumulative enterprise; it is something you all know well.

I decided to talk here about something else, something that is a daily part of our work. Many recipients of this award have commented on how much fun their work is. Science seems to have certainly been fun for Kirk Bryan, and I can say, from personal knowledge about Troy Péwé, that his devoted enthusiasm to things Quaternary far exceeds his job description.

So tonight I want to say something around this theme of fun, and tell you a brief story about the Quaternary legacy of play. E. O. Wilson, who some would call the father of sociobiology, once said that "good science consists largely of play disguised as work." Surely, few scientists would disagree. What I propose to you is that it is not only legitimate that good science be fun, but that fun may be the critical essence of its definition.

As historical scientists, we find our fun by examining the rubble of the past. The trophies of our games are insights. But these insights are not often obvious. My own experience taught me that. I grew up in Huck Finn–Tom Sawyer country along the breaks of the Mississippi River, not far from the Illinois table lands, near Hannibal, Missouri. And, like Tom and Huck, I was ignorant of the Ice Age dimension of the landscape in which I lived. I didn't know it was a Pleistocene sculpture: the rivers were deeply downcut from torrents of Pleistocene meltwater, and those flatlands under all that corn had been planed smooth by ice from the north. I didn't know any of that, nor did my teachers. We called the oxidized loess "ground-hog dirt," because red mounds of dirt dotted the hay pastures from fresh ground-hog diggings. There were rumors of giant bones having been found in road cuts, but the rumors never panned out, never opening up a bigger story. Most farmers had a box of "arrowheads" which they had found walking fresh-plowed fields after spring rains, and we kids often did the same. We had pieces, but we did not know that the Quaternary was all around us.

I had a lot of fun growing up slowly, in good Tom Sawyer tradition, but I had to leave Pike County to learn about this rich Ice Age legacy of midwestern landscapes. The fun of discovering the past began to open up to me in college. It had such an appeal that I entered the academic conduit which ultimately landed me in Alaska.

Alaska was a long way away from Pike County, so I was puzzled to see Alaskan gold miners finding Pleistocene bison, horse, and mammoth fossils just like those uncovered in the grasslands of the Midwest. It became apparent that the grassy landscapes which dominated the American West were similar to those across Eurasia, and on into Alaska. At times these connected, Alaska becoming the belt buckle of the faunal interchange—the turnstile of this connection. The book *Frozen Fauna of the Mammoth Steppe,* for which this award is given, is an installment from a continuing saga of trying to resolve those puzzles.

I guess one experience we all share as scientists is that the insights which emerge from the things we climb over or look at under microscopes are not always apparent from the thing itself. You have to catch the light just right to see the dusty Alaskan Pleistocene plains under the present sphagnum moss and boreal forests. The strange course of the Sangamon River, or the morainal swell of the Des Moines Lobe out in the flatlands do not take on their full meaning until you can envision the breathtaking immensity of continental glaciation. It took a leap of creativity to see that for the first time, and geology, of course, is full of this sort of creativity.

As a paleontologist I keep a sometimes awkward professional stance—one leg in geology and the other in biology. And at this point I am going to shift weight to the biology leg and talk about the natural history of fun—to introduce you to a new idea. I want you to consider the idea that the fun of geology is itself an Ice Age legacy. Could our appetite for the play of science have an evolutionary history? I think it does, and, further, I suspect it is a particularly Pleistocene history.

Let me begin with a piece of apparent trivia. Do you know that invertebrates don't have fun? That they don't play? Not a single one. Coral polyps, earthworms, bees, and termites are all very purposeful. There is no play in an ant hill—perhaps satisfaction of work well done, but no play. Furthermore, there are no fish that play. What about amphibians or reptiles? Play has never been recorded for them, not in toads, or turtles, or snakes. In fact, only a few birds are known to play. Ravens and crows, the corvines, are an outstanding exception.

We can define play as the self-satisfying pursuit of a behavior without obvious external rewards or apparent benefit—it just feels fun to do it. In play, for example, the chaser will often switch willy-nilly and be the chased. Most mammals play, but the majority do so only when they are young. It is interesting that mammalian species who play the most are those that are opportunistic—for example, carnivores and primates. In fact, we can observe that the amount of play is directly correlated with more opportunistic ecological niches. The more facultative and flexible the organism is, the more it must play—animals who don't play have a hard-wired behavioral program.

Biologists examining the details of play have illustrated that play isn't really play at all, but serious business. Yet it strains our objective abilities to think of play as serious, because it is too close to us. But play is the self-teaching mechanism of the animal world for encouraging facultative abilities. Play is calisthenics for an open-ended program, an open mind. One does not learn to be raven smart or to be creative de novo; the cleverness of a raven, the insights of a good scientist, come from exercising those talents in play. Of course, humans are the big players in this evolutionary game. We have no real niche, and so we need to play a lot, not only when young but also as adults.

Now let us shift back to geology. What is the Pleistocene record of play? Has it left us any moraines or paleosols? The answer is, most certainly, yes. Play has left us trace fossils in cranial endocasts and on deep cave walls. The record of the swelling neopallium and the blossoming elaboration of tools, exploration, and art are the most salient features of hominid Pleistocene prehistory. Moreover, the powerful forces of the Pleistocene itself seem to have been responsible for all of this. Its turmoil and unsettling changes throughout the globe introduced new habitats, new combinations of old habitats, and unpredictable vicissitudes in new environments. These uncertainties favored animals who were capable of evolving their creative streak, facultative flexibility, abandoning the old hard-wired proven behavior. Pleistocene hominids made a career of skipping through the cracks of opportunity. But creative behavior itself was unprogrammable. There seems to have been no available evolutionary hardware for creativity. One has to learn it deviously, indirectly. Natural selection took an indirect tack—as it often does. As strange as it may seem, play is the evolutionary vehicle for all creative opportunists, bird and mammal alike. As a young mammal, one develops insight and imagination through experimentation in a protected atmosphere, like children in a family setting, students in school, faculty under tenure, or researchers in USGS. It is significant that the Latin word for playground is *campus.* The driving emotion of this play we call *fun.* Good science fits exactly any definition of play, something done without obvious immediate benefits, for its own internal satisfactions, because it is exciting fun. The point is that this kind of play is not an evolutionary epiphenomenon, it was selected for as surely as pituitaries or incisors.

The tail of this story is that geology itself is an Ice Age legacy. our ability and delight in reconstructing the past are facets of the playful empirical approach that evolved in the late Pleistocene as part of being human. It was this play of mind which created our large brain, which produced the human animal. And it was this play of mind which stumbled over the past as an exciting perspective. As scientists, we polish that special nature into some shared aspect of formal collegial focus which makes it even more fun. This science stuff is only a new name for an old Pleistocene addiction—a fun-driven appetite to understand and create. So it is indeed a remarkable and strange loop to the story that the essence of geologizing itself is an Ice Age legacy—we are, in more ways than one, children of the Pleistocene.

Certainly, for me, working with frozen bits and pieces of the Alaskan Quaternary was fun: stumbling across things like tooth punctures in the skin of a frozen 36,000-year-old bison carcass, marking where it had been strangled by a Pleistocene lion. And, yes, we did cook up a chunk of neck meat into a stew with new spring potatoes, rather like a metaphor for Quaternary research, combining the past with the present.

Kirk Bryan, whose creativity you recommemorate every year by this ceremony, would have, I think, been pleased to know that the excitement and pleasure he derived from Quaternary insights were *themselves* part of an Ice Age legacy—part of this same poetic loop. So my last line must be to remind you to honor this play in your own work, to remind you that good science must ultimately be driven by the enjoyment of it.

Presentation of the

STRUCTURAL GEOLOGY AND TECTONICS DIVISION CAREER CONTRIBUTION AWARD

to

JOHN C. CROWELL

Citation by ARTHUR G. SYLVESTER

John Chambers Crowell might humbly describe himself as simply a general geologist with a primary interest in structural geology and tectonics. His many students have known him, however, as the single professor who made them think the most and most deeply about significant geological problems, and as a teacher who is the most effective and happiest in the field where the rocks are. We know him as one of the foremost geologists of our time, because of the understanding he has given us about the nature and tectonic role of strike-slip faults, especially the San Andreas fault; on the sedimentation in basins related to strike-slip faults; on paleoclimatology, and on the causes of glaciation and continental drift, especially in the Southern Hemisphere.

Some of you may think that John has always been in California, but he was born in Pennsylvania and made his way to California by way of Texas, where he took his B.S. in geology.

After an interlude as a U.S. Army oceanographic meteorologist forecasting sea, swell, and surf for the Normandy invasion in World War II, he took his M.A. in 1946 in oceanographic meteorology from the University of California, Los Angeles, and Scripps Institution of Oceanography. One year later he took his Ph.D. in geology from UCLA.

Then he commenced a career as a university professor for 20 years at UCLA and for 21 years at the University of California, Santa Barbara, a distinguished career that culminated recently in his being named professor emeritus. Usually this title means entitlement to retirement and to the pasture, but not so for John. He complains to me about being busier than ever, but happily and deeply involved in geotectonic and paleoclimatic problems that are as complex and intriguing as any he has ever studied. We can look forward to continued revelations from his ramblings in the field and from the keyboard of his word processor over the next many years.

John is a member of several societies, including AAPG, AGU, and SEPM, and he is a Fellow and has been a Councilor of the Geological Society of America. He has a list of distinguished honors and awards, including fellowship in the American Academy of Arts and Sciences and membership in the U.S. National Academy of Sciences.

We honor him today with another award—the Career Contribution Award for the Structural Geology and Tectonics Division of the Geological Society of America—for his contributions to our Division and Society as a structural geologist and tectonicist whose writing and teaching have influenced almost each one of us—some of us even profoundly.

Mr. Chairman, ladies and gentlemen, it is my pleasure to present for your recognition the fourth John in five years to whom this award has been presented: My colleague, friend, and indeed, my geological godfather—John Crowell.

Response by JOHN C. CROWELL

Thanks Art—sincerely! Your words are deeply appreciated.

I stand before you and glow
Very humble indeed.
It's very nice to know
That my peers agreed
My work to heed,
And to be honored so!

On a recent teaching field trip to the California desert, a new graduate student asked: Why go to the field? I was with a dozen grad students newly arrived from many different undergrad institutions, gathered around a campfire in a desert wash. We had just spent the day clambering over barren and rugged mountains, peering at complicated rocks. We had crawled across migmatites, peered at mylonites and fault gouge, struggled to understand joint patterns, gazed at a steep canyon wall of folded and faulted strata that included a folded unconformity, walked across a contact where granite had intruded older gneisses, and examined an old volcanic flow interbedded with sandstone layers, and a thin layer containing fossils. These rocks made up the local continental crust. The student, however, was "turned on" by his just-selected dissertation project involving a piece of the Pacific Ocean floor. I sensed a reluctance in his mind to going back to the ancient "boots and hammer" stage in the evolution of our science. More exciting vistas than field mapping lay ahead. Some students somewhat resented spending a long weekend away from their cherished labs where there was so much to learn and so little time to learn it.

Moreover, in getting to our desert mountain range and our pleasant campsite, we had crossed vast spreads of desert flats. What lay beneath the alluvium? A perceptive student pointed out that only remote methods would reveal the structure of the terrane at depth. Geophysical approaches, such as seismic profiling, revealed much. She noted that studying the ground beneath the desert flats was not too much different from the oceanographic problem: the sea floor is covered by kilometers of water, and so remote methods are needed there also. In continental regions, the geophysical methods give useful information on limited characteristics of rocks at depth, such as the ability of discontinuities to reflect or refract sound waves, or on density distributions, or on their magnetic properties. Drilling is necessary to get our hands on rock, and so relate its geophysical properties to its other properties.

Outcrops within mountain ranges bordering expanses of alluviated desert provide clues because we can crawl across them on our hands and knees, peering at their details. But such peering is not enough. Many analytical methods are now available—to date rocks isotopically, to learn of their chemical composition and of diagenetic changes, to fingerprint volcanic ash, to recognize tectonic rotations and tiltings from paleomagnetic measurements, to interpret ecology from fossils and sedimentary facies. Several students in the campfire group were keen to apply such specialized methods to add their bit to understanding our Earth.

We named a few new methods that had arisen during the past few years such as tomography and the use of the global positioning system. Remote sensing data from satellite images use wave bands that our eyes cannot discriminate. Who would have thought a few years ago that studies of pressure-temperature-time paths could tell us so much about the ancient history of metamorphic rocks? Can you guess now what new geochemical or geophysical technique is about to be invented which will tell us even more about the history of the crust?

What new instrument may come along that will add significant information? One student even pointed out that our science is often led by technology—the invention or improvement of a new instrument. Science does not necessarily lead technology! Often it is the reverse.

I reminisced that new concepts were important as well. When I made my first geologic map over 54 years ago, many concepts had not yet arisen. In Ridge Basin I had to go back after mapping the region and look for turbidity-current structures, such as sole markings, after the role of turbidity-current processes became recognized. I had not noticed such structures when I focused on contacts and mappable units and faults. Magnetostratigraphy came along, so I returned to the same area to learn how reversals fitted into the stratal sequence. We now have some reflection profiling in the region, and crustal extension is the vogue, so recently I returned to the same region to look for flat faults and kinematic indicators—now that we know how useful the indicators can be. Our group recognized that field work never ends—new concepts and new methods keep coming along. One student muttered under his breath, "I see why a guy like you keeps going back to the same area again and again."

All day we had been using geologic maps. They not only guided us to significant outcrops, but in themselves revealed much about the history of the region. Samples collected without careful relation to their field setting are worthless. We need more good maps, and I am pleased that there is a new national effort, just budding, to recognize their usefulness. In the late 1940s, my dissertation mapping along the San Andreas fault near its intersection with the Garlock fault impressed upon me that the histories recorded within rocks bordering these great faults were very different. How could I account for these differences? The mismatches seemed unsolvable by dip slip alone. Ever since then I have been trying to resolve mismatches into matches. This approach has been a key to documenting great strike slip on some of California's major faults, and in other tectonic belts. I embraced plate tectonics with enthusiasm because Tuzo Wilson's concept of transform faults explained how strike-slip displacements of several hundred kilometers fitted into a world tectonic scheme. But geologic mapping alone is not enough. Explanations of differences or contradictions in histories depend on careful analytical work. Isotopic dating and other geochemical data are essential. Many questions could not be answered by only going to the field. Lab work is vital to our progress in understanding the Earth.

Our field trip was giving us a feeling of *scale and complexity and history and time.* The main reason for going to the field is to gain first-hand appreciation of the complexity of Earth history and of the scale and type of the record. As tiny air-breathing animals, we can best learn this by crawling across field outcrops and fitting our wee observations into the hugeness of our planet and its long, long history.

We were also having great fun on our trip. We were enjoying the camaraderie around the campfire and as we hiked up washes to outcrops. Geology is indeed a social science. We work together, although at times we prefer to wander and ponder by ourselves. It has been great fun to go to the field with students, professors, colleagues, and novices over the past half-century. I am deeply indebted to my many former students and my teachers and my colleagues. They have made my happy way of life. There is a long list of people to whom I owe very, very much. Here I can only mention my wife Betty, who for 47 years has put up with my geological eccentricities and helped me at every turn. I thank her from the whole of my heart.

My message is that our world is complex and at many scales and that geologic time is immensely long. New methods and concepts keep coming along. Data from space are now available. So are data from the depths. The data come in at all scales—from tiny bits from the microprobe to satellite images. Jet airplanes allow us to get quickly to remote areas, where we can apply new concepts and techniques. No one of us can expect to be expert in all methods available to approach questions of Earth history. But each of us can have sincere appreciation for the work of other scientists and enjoy and relish their contributions. Each of us must have a generalist attitude and also be steeped in the skills necessary to work in a specialty. With delight I have lived through exciting times, times that have witnessed the convergence of many subdisciplines—field mapping, geochemistry, geophysics, paleontology, and others—into one: *the science of understanding the Earth and its long and complex history.*

Thank you for this honor, which I shall deeply cherish!

Conference Addresses Interface of Professional Societies and Two-Year Colleges

Why would the National Science Foundation fund the conference "The Role of Professional Societies in Two-Year College Science, Engineering, and Mathematics Education"? The Congressional Committee on Science, Space, and Technology last spring held hearings on reward systems in higher education. The need for better teaching has never been more urgent, given the poor performance in science and mathematics by the large numbers of students in our schools and colleges. This conference, held in October 1992, provided an opportunity for professional societies to consider how they can contribute support in strengthening science and mathematics education by validating their members' efforts in improving teaching and curriculum development at all levels.

An NSF-based study, *Matching Actions and Challenges,* recommended that "professional associations need to recognize the role of the two-year faculty in the area of science, engineering, and mathematics, and seek to enhance their participation as active and valued members. National-level grants and scholarships should be made available to a large number of interested faculty. The collective talent of the nation's two-year faculty, administration, and staff has been under-recognized for far too long." NSF has also recommended that discipline-based professional societies assume a leadership role in the lower division curriculum. A need exists to strengthen the interactions among the organizations, the faculty, and funding agencies.

At the October conference, the 77 participants representing academia, industry, government, and professional societies met in interdisciplinary groups to examine the role of two-year institutions, the interaction between the various organizations, and what can be done to enhance two-year colleges. The latter part of the meeting was focused on specific disciplines. Ed Geary from GSA, Frank Ireton from NESTA and AGU, Marcus Milling and Marilyn Suiter from AGI, Keith Sverdrup from AGU, Brian Tormey from NAGT, and I constituted the representatives from the geosciences.

Numerous misconceptions exist about the roles and missions of two-year institutions. They are often regarded by four-year colleges and universities solely as remediation centers and vocational training institutions. The increasing numbers of students attending these institutions demand that professional societies play an active role in correcting this impression. In 1991, 43% of all postsecondary matriculating students in the United States were taking courses for college credit from two-year institutions. There are more than 1400 public and private two-year institutions serving 5 million students in the United States, and California alone has 109 two-year institutions serving approximately 25% of those students. In fact, the California State University statistics for the 1990–1991 year show that 26,174 (52%) of the 50,352 degrees they awarded were granted to students who had transferred from a California community college. In the near future, two-year institutions in the United States will become an increasingly attractive option for more high school graduates.

The necessity of understanding geoscience has expanded rapidly in the past decade. Societal, environmental and economic pressures, geohazards and georesources problems, new careers, and retraining have had an impact on the geosciences. The need for greater comprehension of geoscience-related problems, better-quality geoscience teaching, and more practitioners is urgent. The multifaceted role of the two-year institution accommodates many needs in today's educational climate, which includes an increasingly diverse student population, and severe budgetary cutbacks, yet a projected need for more science majors. The two-year institutions' geoscience departments fulfill a number of roles, including (1) *preparation* of students for a variety of educational options, (2) *improvement* of the quality of geoscience education, and (3) *production* of more geoscience majors (California two-year institutions produce more than 50% of the geoscience majors who attend state universities in California).

The large number of introductory geoscience course sections, low student/teacher ratios, commitment to lower division courses, and enhanced faculty-student interaction possible at two-year institutions are all advantages in creating an initial interest in the geosciences. Updating and enhancing of two-year faculty, and increasing the lower division load of transferable courses would increase the time students have to develop an interest in the geosciences. The transfer of sufficiently rigorous lower division two-year-institution geoscience courses needs to be encouraged. The probability of this occurring increases with a better qualified faculty.

What can GSA do?

1. Encourage and support more sessions at meetings which focus on teaching and learning at the two-year college level.
2. Recognize how two-year colleges serve as pivot points in the successful transition of students from high school to four-year institutions.
3. Place greater emphasis on the value of good teaching and on curriculum development.
4. Use its resources to enhance communications between two-year schools, between all levels of education, and with GSA through publications and networking that bring attention to issues relevant to this unique level of teaching.
5. Strive toward publication of a readily available directory that includes all two-year institutions.
6. Focus greater attention on issues related to two-year faculty concerns at meetings, within committee structuring, and through committee organizations.
7. Be instructive in providing stimulus and opportunities for two-year faculty to become successful at grant writing by informing two-year faculty about funding opportunities, fostering improved writing skills, and forming consortia between two-year and four-year schools.
8. Publicize job opportunities at the two-year college level, thereby providing a service to job-hunting graduates and benefiting students with an increasingly better trained, better qualified, and more involved faculty.

With the help of GSA, two-year institutions need to

1. Become more involved by taking the initiative in forming strong disciplinary and interdisciplinary networks that strengthen undergraduate education.
2. Join and play a more important role in professional societies such as GSA, especially in areas that focus on issues related to teaching and curriculum.
3. Produce articles, organize and lead workshops, and learn more about funding sources.
4. Work with business and industry in community and economic development to determine curricular needs.

GSA stands to benefit by

1. Increasing membership from better prepared students entering the geoscience profession.
2. An enhanced communications network that links modern knowledge and technology of students and faculty into the scientific world.

Dorothy L. Stout
Chair, GSA Geoscience Education Division ■

GSA Committees *continued from p. 60*

1992–1994; Darryll T. Pederson, 1992–1994; Ben A. van der Pluijm, 1992–1994; *NSF Conferee:* Thomas O. Wright

***Treatise on Invertebrate Paleontology* Advisory Committee**
Richard Arnold Davis—Chair, 1993–1996; Ronald R. West, 1991–1994; F. Michael Wahl, Executive Director

Committee on the Young Scientist Award (Donath Medal)
Sharon Mosher—Chair, 1992–1994; Robert H. Dott, Jr., 1991–1993; Robert N. Ginsburg, 1991–1993; Frank S. Spear, 1992–1994; John C. Behrendt, 1993–1995; Leonard F. Konikow, 1993–1995

Ad Hoc Geosphere Alliance Committee
William S. Fyfe—Chair; Fred A. Donath; William L. Fisher; Robert D. Hatcher, Jr.; Susan W. Kieffer; Raymond A. Price

Ad Hoc Committee on Membership Services
Arden L. Albee—Chair; Genevieve Atwood; Kenneth E. Kolm; Marie E. Morisawa; Karen L. Prestegaard; John M. Sharp, Jr.

GSA Member of the American Geological Institute (AGI) Member Society Council
William R. Dickinson—Vice-President

GSA Member of the AGI Education Advisory Committee
Edward E. Geary—Coordinator for Educational Programs

GSA Member of the AGI Government Affairs Program Advisory Committee
M. Gordon Wolman, 1991–1994

GSA Representatives to the American Association for the Advancement of Science (AAAS)
Section E—Geology and Geography: J. Thomas Dutro, Jr., February 16, 1991–February 15, 1994; *Section W—Atmospheric and Hydrospheric Sciences:* John G. Weihaupt, February 16, 1991–February 15, 1994

GSA Representatives to the AAAS Consortium of Affiliates for International Programs
Kevin Burke, President, GSA International Division; F. Michael Wahl, GSA Staff Liaison

GSA Representatives to the North American Commission on Stratigraphic Nomenclature (NACSN)
Peter R. Vail, 1990–1993; G. B. Morey, 1991–1994; Donald L. Baars, 1992–1995; Representative-elect: Lee C. Gerhard, 1993–1996 (term begins during the NACSN 1993 fall meeting in Boston)

GSA Representative to the *Treatise* Editorial Advisory and Technical Advisory Boards of the Paleontological Institute
Richard Arnold Davis

GSA Delegate to the Circum-Pacific Council
Robert L. Fuchs; May 2, 1984–

GSA Representatives to the Joint ASCE-GSA-AEG Committee on Engineering Geology (American Society of Civil Engineers, Association of Engineering Geologists)
John D. Rockaway; July 1, 1990–June 30, 1993; Jeffrey R. Keaton; July 1, 1991–June 30, 1994

GSA Representative to the U.S. National Committee on Tunneling Technology
Charles A. Baskerville, July 1, 1992–June 3, 1995

GSA Representative to the U.S. National Committee on Scientific Hydrology
David A. Stephenson, 1990–; Bruce B. Hanshaw (alternate)

GSA and AASG Selection Committee for the John C. Frye Memorial Award in Environmental Geology (Association of American State Geologists)
Frank E. Kottlowski—Chair; AASG representative, John P. Kempton; GSA representative, 1990–1993; Diane L. Conrad, AASG representative ■

GSA Annual Meeting Technical Program: Structure and Planning

Richard W. Berry, San Diego State University, Technical Program Chair—1979 and 1991 Annual Meetings, Program Committee Chair—1992

INTRODUCTION

Misconceptions concerning how the GSA Annual Meeting technical program is put together are making a complicated process more unwieldy than necessary. As immediate past chair of the Program Committee, charter member and acting chair of the Program Review Committee (predecessor of the Program Committee) and twice chair of the Joint Technical Program Committee (JTPC), I believe it might be helpful to present my sense of how the meeting's technical program is created. Many of the problems that I have had to deal with as chair of the Program Committee and JTPC stemmed from ignorance of the structure and process that resulted in the technical program. This should not be confused with official GSA policy, nor is it meant to represent the thoughts and viewpoints of GSA headquarters staff. The viewpoint and words are mine. The commentary is meant to be descriptive and neither critical nor supportive of current practice.

CONSTRUCTION OF THE TECHNICAL PROGRAM

The process of structuring the technical program is *senatorial, representative, autonomous, and egalitarian,* all at the same time. It is *senatorial* in the sense that each of the cooperating entities (Divisions of GSA and Associated Societies) regardless of size are allotted one half-day symposium. (There may be some exceptions regarding entities that joined the meeting program recently.) This assures that the smaller groups will not be entirely dominated by the larger groups.

The structure is *representative* because any group with a large membership and a high abstract submission rate controls a larger part of the meeting, in proportion to the group's size. The number of disciplinary sessions is adjusted to be in proportion to the categories and numbers of abstracts submitted. The fairness of distribution of volunteered (disciplinary) sessions is protected by applying all necessary rejection rates "across the board" on a percentage basis.

It is *autonomous* because each entity has control over its own symposium. Each entity also has primary control over the quality of abstracts accepted to disciplinary (volunteered) sessions that share subject expertise with the entity. For example, the Mineralogical Society of America (an Associated Society) has control over which abstracts will be accepted for the various mineralogy and hard-rock petrology sessions.

It is *egalitarian* because of the opportunity for any one person or group of persons associated with any Annual Meeting entity to advocate a theme (volunteered) session.

The rules and regulations that have evolved over the years were set up with the intent to protect the system as it now operates. They are not inflexible, however. Special symposia may be offered on Sunday. Groups may cooperate to blend their half-day symposia into larger blocks of time. Special programs may be fit into the program at the last minute if circumstances warrant.

WHAT DRIVES THE MEETING?

The meeting is driven from the bottom up. Individuals have the opportunity to exert some control by being able to submit abstracts. Those Associated Societies and Divisions that are organized most effectively and efficiently exert the most control. JTPC acts in many ways as a clearinghouse, allowing or making it possible for the various entities to minimize conflicts and optimizing each entity's chances to have as many as possible of its wishes and needs met.

GSA headquarters staff has all it can do to stay on top of the activities that are necessary to facilitate the coming together of the meeting. These activities include reserving appropriate rooms in which to meet and keeping track of abstracts and the action on each abstract. The headquarters staff exerts its primary control on aspects outside the technical program, such as exhibits, short courses, and social events. The Program Committee serves as an oversight body that makes recommendations to the GSA Council.

Some folks criticize, as an "adhocracy," the present structure (which is driven from the bottom up). Those who are most critical of the current structure of the meeting are people who prefer a meeting that is controlled from the top down. Although benefits accrue from a meeting that is run from the top down, major sacrifices of power and control would have to be made by Associated Societies and Divisions. Advantages of a top-down–driven meeting would be in coordination and focus. Care would need to be taken to include all the disparate groups when planning the focus in order not to risk alienating organizations.

ROLE OF DIVISIONS AND ASSOCIATED SOCIETIES

Under the current system, the stronger the structure and leadership of a Division or Associated Society, the more effective will be its influence on the meeting and the better will be the entire meeting because of it. All Divisions and Associated Societies would benefit from overlapping terms of officers and long-range planning committees. The better their internal communication and coordination, the better they are able to interact with other organizations as the meeting program is being put together. Communication between organizations and GSA headquarters depends upon headquarters staff being informed about changes in officers and addresses. GSA would benefit greatly if well-organized groups helped other entities improve their organizational effectiveness and impact on the Annual Meeting. The JTPC meeting, chaired by Bill Dickinson at the 1992 Annual Meeting (Cincinnati), was an important first step toward facilitating a useful exchange of information about programs and procedures.

Until now and until directed by the Council to change, the JTPC and Program Committee impose no control over Divisions or Associated Societies except as required to maintain quality and to assure equality of opportunity for access to the technical program.

CLOSING THOUGHTS

The system is not perfect. Mistakes are made by the humans who have been given responsibility for coordinating the efforts to bring a meeting's technical program to fruition. GSA headquarters is improving steadily in its use of computers to keep track of the myriad of variables that now come together in an Annual Meeting. Computerization of the support system needs to be accelerated. The underlying methodology for putting the meeting together has not changed much during the approximately 17 years that I have had something to do with the GSA Annual Meeting. However, the number of variables (abstract categories, total cooperating organizations, volume of abstracts, meeting registration) have increased greatly during this period. Without increased computer assistance, the meeting will need to stabilize in size or perhaps shrink a bit.

My advice to those who advocate major changes in the ways by which the Annual Meeting is put together is the same as my advice to those who listen to the advocates of radical change. If you really think a change is warranted, do it, but first make sure the advantages and disadvantages are weighed carefully. The character (flavor) of the meeting will change if we move more toward an American Association for the Advancement of Science or American Geophysical Union structure. Some would welcome such a change. There is a big difference between a meeting driven from the bottom up and one controlled from the top down. We must be sure which style best serves the greatest percentage of the GSA membership before either setting the current method in concrete or advocating major revisions.

Responses to this article are welcomed. Please send to Dick Berry, c/o Meetings Department, GSA, P.O. Box 9140, Boulder, CO 80301. ■

GSA ANNUAL MEETINGS

■ 1993

GSA Annual Meeting
Boston, Massachusetts
Hynes Convention Center
October 25–28

Chairman: James W. Skehan, S. J., Boston College

Abstract Deadline: July 7
Preregistration Deadline: September 24

For information call the GSA Meetings Department, (303) 447-2020.

■ 1994

GSA Annual Meeting
Seattle, Washington
Washington State Convention and Trade Center
October 24–27

Chairman: Darrel S. Cowan, University of Washington

Call for Field Trip Proposals: Please contact the field trip chairman—Donald A. Swanson, Department of Geosciences, University of Washington, Seattle, WA 98195, (206) 543-1190. *Deadline: May 15, 1993.*

For information call the Meetings Department, (303) 447-2020.

New for '93

Registration Materials will be in JUNE *GSA Today*

REGISTER EARLY

■ FUTURE

Boston	October 25–28	1993
Seattle	October 24–27	1994
New Orleans	November 6–9	1995
Denver	October 28–31	1996
Salt Lake City	October 20–23	1997

For general information on technical program participation (1993 or beyond) contact Sue Beggs, Meetings Manager, GSA headquarters.

Robert L. Fuchs and Bruce B. Hanshaw

IGC Fund Sends Geologists to Japan

In August 1992, 23 young residents of the United States attended the 29th International Geological Congress in Kyoto, Japan, with the aid of travel grants from the GSA Foundation. The travel-grant program for the 29th and future IGCs came about because of a $330,000 donation to the Foundation from the Organizing Committee of the U.S.-hosted 28th IGC held in Washington, D.C., in 1989. The money represents the residuum of the funds ($6.2 million) that were used to run the 28th IGC. The Organizing Committee had observed that it was generally difficult for younger geoscientists to attend these important quadrennial IGCs and therefore asked the GSA Foundation to oversee the fund and use it to encourage U.S.-resident younger scientists to attend future congresses.

The first travel-grant program was widely advertised; 59 scientists responded. The Selection Committee ranked all applicants and recommended that 23 grants be given; 13 others were selected as alternates, and three of these were eventually given grants. The grants consisted of a round-trip air coach ticket and payment of the registration fee; the value of the average award was $1496. Because this was the first travel-grant award program, the decision was made to invade the corpus of the original donation in order to award a reasonable number of grants. In the future, only interest income will be used in order to preserve the corpus of the fund. However, because IGCs only meet every four years, the program should generally have about $70,000 for travel-grant awards.

Recipients of the first grant program are listed below.

Recipient	Affiliation
Richard J. Behl	University of California
Nicholas J. Butterfield	Harvard University
Timothy B. Byrne	University of Connecticut
Steven D'Hondt	University of Rhode Island
Gabriel M. Filippelli	University of California
Benjamin P. Flower	University of California
John Joseph Flynn	Field Museum of Natural History
Andrew Norman Fox	Cornell University
Virginia C. Gulick	University of Arizona
Tekla A. Harms	Amherst College
Roland Hellmann	University of South Florida
Alfred G. Hochstaedter	University of South Florida
Alan Jay Kaufman	Harvard University
Caroline Klug	University of Oregon
Ian W. Moxon	Amoco Production Company
Paula Noble	University of Texas
Naomi Oreskes	Dartmouth College
Brian Ellsworth Patrick	University of California
Greg Ravizza	Woods Hole Oceanographic Institution
Kathryn A. Schubel	SUNY Binghamton
Carl C. Swisher III	Institute of Human Origins
Karl R. Wirth	Macalester College
An Yin	University of California

The next IGC travel-grant program will be for attendance at the 30th IGC, to be hosted by the Chinese in Beijing, China, in 1996. Requests for applications will be advertised in 1995, and grants will be awarded in early 1996. Plan now to apply if you are a resident of the U.S. and will be under 40 years of age at the time of the Chinese-hosted 30th IGC.

Pooled Income Fund Exceeds $100,000

The GSA Foundation Pooled Income Fund has grown above the $100,000 level as a result of gifts during 1992. Assets of the Fund on December 31, 1992, totaled $105,990 and consisted of shares of Warburg Pincus Counsellors Fixed Income Fund and corporate bonds.

During 1992 the Pooled Income Fund provided holders with a cash return of 7.0%. Total return for the period was 8.1%.

The Pooled Income Fund is a form of planned giving whereby the donor makes a gift to the fund but reserves the income from the gift for life. Upon the donor's death, his or her respective share of the fund is transferred to the Foundation's endowment. Thereafter the income can be used for the general mission of the Foundation, or for any purpose that might have been specified by the donor.

The GSA Foundation Pooled Income Fund is a good way for a GSA member to obtain retirement income, reduce current taxes, and at the same time make a gift to GSA in support of its ongoing programs. At the time of the gift, the donor receives a charitable deduction for income tax purposes, the amount of which is determined by the donor's life expectancy and the expected income from the fund. Quarterly income is distributed to the donor or beneficiary out of the earnings of the fund.

Further information about the GSA Foundation's Pooled Income Fund can be obtained by calling or writing the Foundation office at GSA headquarters. ■

In Memoriam

Lee C. Armstrong
Edina, Minnesota
July 9, 1992

James P. Pollock
Hendersonville, North Carolina

John T. Galey
Somerset, Pennsylvania
May 1992

Sheridan A. Thompson
Houston, Texas

Donors to the Foundation, December 1992

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Rosalind Munro

J. Hoover Mackin Award
Peter W. Birkeland
Charles G. Mull*
C. F. Stewart Sharpe

Hydrogeology Division Award
Susan J. Altman
Thomas Corbet
Claire B. Davidson*
George H. Davis
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David M. Diodato
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Gerald Meyers (in memory of T. C. Chamberlin)
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*Second Century Club members (gifts of $100 or more).

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The Byword is Change

Margaret Goud Collins
1992–1993 GSA Congressional Science Fellow

Bill Clinton rode into town on the winds of change. The word is in the air, and science in policy is caught up in it. From environment to competitiveness, people are looking to the scientific community for input on how the U.S. should deal with its problems and with a dramatically changing world. It's a great year to be in Washington.

This is not to say that beginning a fellowship in a Presidential election year when the party in the White House changes is a monotonically positive experience. In late September, after a three-week orientation, the 25 Congressional Science Fellows were let loose to negotiate an assignment with one of the 500+ personal, committee, or subcommittee offices in Congress. The essence of the selection process consists of exchanges with senior staff members about your interests and experience and their office's immediate legislative priorities. You and the office use the interview to determine whether there is sufficient overlap between your interests and theirs for them to offer you a position and for you to accept it.

Office selection is a daunting task at the best of times, but this year the Fellows were beginning the placement process as the 102nd Congress was racing toward adjournment, after which most offices were obsessed with the coming election. Because there was such a high turnover in Congress this year, many committees were uncertain of their membership and, therefore, priority issues for the coming year. Even after the election, uncertainty reigned, as President-elect Clinton raided Congress for Cabinet appointments, and many Congressional staffers went to work on planning for the Clinton-Gore Administration, some to be swallowed into Administration jobs.

The Office and the Issues

The first day of the Transition, the time between election day and inauguration day, was also my first day on the job in the office of Montana Senator Max Baucus. Senator Baucus is involved in a number of issues that are of central concern to the geological community: he was the author of the Vertebrate Paleontological Resources Protection Act introduced in the 102nd Congress and likely to be reintroduced in the 103rd; as a senator from a mining state, he is interested in the perennial debate on amendments to the Mining Act of 1872; he helped fashion the (unsuccessful) Wilderness Bill legislation of the last Congress; and he has been a major player on the environmental legislation for years as a member of the Environment and Public Works Committee.

I hope to contribute some geological perspective in all these areas, but my decision to work in the office of Senator Baucus hinged directly on his interest in international trade issues. My goal in my search for an office was to make international environmental issues a part of my working portfolio for the year. More specifically, I am interested in finding out how the American scientific community can be involved in strengthening the capacity of developing countries to make sound environmental decisions. During the interview process, I became aware that Congress has begun to link the issues of environment and international trade, most notably in the North American Free Trade Agreement (NAFTA). Max Baucus is very interested in finding ways to use trade to promote environmentally sound development worldwide, and his staff made it clear to me that I could play an active role in their work on those issues.

It seems odd even to me that, as a geologist-oceanographer, I would seek a role in the arcane legal-economic-international issues that comprise the governance of trade. But environmental management is, at base, a question of understanding natural systems. That understanding is the province of scientists. Usually we wait to be consulted, at such time as those in charge feel our input is needed in implementing the policy goals. By becoming involved in this issue while the policies are being formed, I hope I will be able to spot places where scientific contributions can be integrated into trade-related environmental policies from their inception.

Upheavals Major and Minor

The changes that Clinton wrought reached down even to my level. The appointment of Senator Lloyd Bentsen as Secretary of Treasury opened up the chairmanship of the powerful Finance Committee, which was assumed by Senator Daniel Patrick Moynihan. Because no senator can head more than one committee, he gave up the chairmanship of the Environment and Public Works Committee (EPW), a post he had assumed only in September when Senator Quentin Burdick died. The EPW chair fell to Max Baucus.

The chairmanship of a full committee in the Senate confers a great deal of power over legislation concerning the issues in the committee's jurisdiction, and over staffing for that committee. Two of the top staff members from Senator Baucus's personal office moved to the EPW Committee staff; and since most of Baucus's environmental work will now emanate from the committee, they asked me to join them. The cascade effect of Clinton's Cabinet appointments has left me in a new office, with a more powerful boss, and with an opportunity to learn the workings of the Senate from the perspectives of both a personal and a committee office.

The Work So Far

The pervasive uncertainty has called for flexibility, but it has had its advantages. I came into the office with no experience in trade issues, and I have had time to study the subject and its environmental aspects, and to meet many of the representatives of environmental groups who have taken an interest in the subject. I also took advantage of an opportunity to spend a week in Montana with an EPW staff member. We visited five cities, meeting with state and local government officials, local business people, and local environmental groups. The purpose was to gauge the effects of legislation (notably in this case, Superfund toxic-waste cleanups and the Clean Water Act), so that reauthorizations of the acts, which are scheduled for this year, can take into account the problems of the people who have to abide by and enforce them. It was an interesting lesson in the convergence of national and local politics.

With Clinton's inauguration, the action began. "Hit the ground running" was nearly as commonly heard as "change," and everyone, from the new Administration to the new committee chairs (like my boss) to the new members of Congress, wants to show progress immediately. I'm grateful for the time I've had to learn my way around, but now comes the good part. ■

Margaret Goud Collins is the GSA Congressional Science Fellow for 1992–1993. She is serving on the staff of the Senate Committee on Environment and Public Works, and can be reached at (202) 224-6176. The one-year fellowship is supported by GSA and by the U.S. Geological Survey, Department of the Interior, which supports 47% of the program with a $23,000 grant under Assistance Award No. 1434-92-G-2251. The views and conclusions contained in this document are those of the author and should not be interpreted as necessarily representing the official policies, either expressed or implied, of the U.S. Government.

More GSA Representatives Needed

In the mid-1980s, GSA launched a new representative program, targeting companies, agencies, and consultants throughout the country. The purpose was to broaden GSA's representation to include all employment sectors. The program was modeled on the successful campus representative program that began in 1979 and now includes 547 representatives at colleges and universities throughout North America.

We now have 144 company, 92 agency, and 46 consultant GSA representatives. However, we need more volunteers. Our goal is to designate a representative at all major company offices and governmental agencies throughout the country. For example, we hope to have a GSA representative for ARCO in Anchorage, for the Geological Survey of Canada in Vancouver, for the U.S. Geological Survey in Tucson, etc. We want to develop a similar liaison with GSA members who are self-employed and serve as consultants. They would also represent major cities and geographic regions.

Representatives serve as liaisons between GSA headquarters and their constituency in a particular city or region. They provide information on the programs and benefits of the Society to other members in the region and explain to prospective members the advantages of joining GSA. Each representative receives a notebook containing complete information on all GSA programs, activities, publications, meetings, and other benefits that the Society provides its membership.

We need your help to continue this communications link between GSA headquarters and the membership of the Society. If you are a Member or Fellow (not Student Associate) and are interested in serving GSA as a representative for your company, agency, or group of the employment sector, please contact T. Michael Moreland, Manager, Membership Services, Geological Society of America, P.O. Box 9140, Boulder, CO 80301, (303) 447-2020.

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GSA GeoVentures 1993

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Plaza Resolana en Santa Fe, Santa Fe, New Mexico
Five Days and Six Nights: May 29–June 3, 1993

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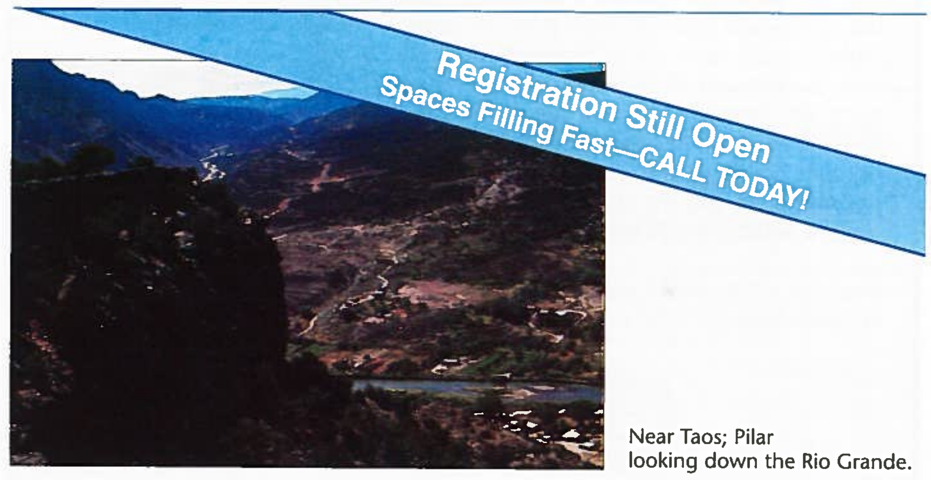

Near Taos; Pilar looking down the Rio Grande.

At the base of the Sangre de Cristo Mountains lies the city of Santa Fe, founded in 1610 by Pedro de Peralta acting on instructions signed in Mexico City by Viceroy Luis de Velasco, the King of Spain's representative in New Spain. The romantic and historic aura of Santa Fe, "The City Different," is unique among American cities and is seen in all aspects of the Santa Fe experience. Santa Fe is also part of and central to a richly diverse geological (and cultural-historical) vista that is challenging both to contemplate and to visit.

Spanish settlers began to arrive in the vicinity of Taos Pueblo to the north of Santa Fe at about the same time that Santa Fe was being established. Through the early years of the 20th century, Taos developed into a haven for artists, potters, and writers, and it remains so today. The Rio Grande Gorge near Taos is 200 m deep and 400 m wide and developed in thick basalts. In the area, Precambrian rocks form the basement beneath late Paleozoic age marine and nonmarine sedimentary rocks.

The north-trending Sangre de Cristo Mountains are about 30 km wide and 320 km long and separate the late Cenozoic Rio Grande Rift on the west from the Raton Basin on the east. In the Santa Fe region, the basement rocks are Precambrian metasedimentary, igneous, and metaigneous rocks. The Jemez Volcanic Field separates the Espanola Basin on the north from the Albuquerque Basin on the south. Volcanism began after formation of the Rio Grande Rift. The Valles and Toledo calderas are major structures within the Jemez Volcanic Field. The Sandias, granitoid rocks capped by Paleozoic sedimentary rocks, rise majestically above Albuquerque.

To the west of Santa Fe lies the San Juan Basin, rich in mineral wealth, especially coal, oil, and uranium. Equally rich in fossil-containing rocks and cultural history, the San Juan Basin preserves the magnificent world-renowned archaeological ruins seen at Chaco Canyon.

Program Schedule

May 29, Saturday .... Welcoming get-together
May 30–June 3,
Sunday through Thursday .... Classes and field trips
June 3, Thursday .... Farewell party

Fee and Deposit

Cost: $550 for GSA members. Nonmembers $595.
$125 deposit, due with your reservation, is refundable through March 30, less $20 processing fee.
Total balance due: April 1
Minimum age: 21 years. Limit: 28 persons.

Fee includes classroom programs and materials, field trip transportation, lodging for 6 nights (double occupancy, dormitory rooms), breakfast and lunch daily through Thursday, and welcoming and farewell events. **Not included** are transportation to and from New Mexico, transportation during nonclass and field trip hours, meals or other expenses not specifically included.

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The Geological Society of America
BULLETIN
Volume 105, Number 3, March 1993

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GEOLOGY

VOLUME 21
NO. 3
P. 197–288
MARCH 1993

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Applications are invited for the position of Assistant Professor of geology in Fall 1993. Candidates must be in possession of a Ph.D. degree in geology, and be prepared to teach a wide range of courses, including introductory physical and historical geology. Specializations should include sedimentation, marine geology and coastal processes. Familiarity with the Atlantic coast would be helpful. Candidates must be willing to develop programs in geology, environmental science, and earth science education. Applications, resumes, and three letters of recommendation should be addressed to Dr. Robert Harrison, Chairman, Geology-Geography Department, C. W. Post Campus of Long Island University, Brookville, New York 11548. Applications must be postmarked no later than April 15, 1993. Long Island University is an equal opportunity/affirmative action employer.

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Primary responsibilities involve routine maintenance, calibration, and operation of the SX-50 microprobe, its support equipment and computers in the microprobe laboratory, user training, and analytical services for non-USC personnel.

Required skills include (1) principles and operation of electron optical systems, (2) principles and application of x-ray spectrographic analysis, (3) fundamentals of mineralogy and mineral analysis using electron microprobe techniques, (4) ability to operate a variety of computer systems. Basic programming skills are desirable.

Applicants must hold an M.Sc. degree in natural science or engineering, or B.Sc. and at least 3 years experience as an electron probe operator. Starting salary commensurate with degree and experience.

Send resume, copies of university transcripts, and 3 letters of recommendation to Dr. John Shervais, Department of Geological Sciences, University of South Carolina, Columbia SC 29208. Telephone: (808) 777-4534; FAX (803) 777-6610. Applications should be received by 15 April 1993.

The University of South Carolina is an Equal Opportunity employer. Women and minorities are encouraged to apply.

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University of Miami, Coral Gables

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The position is located at the main campus in Coral Gables.

Applicants will be expected to teach undergraduate courses in Invertebrate Paleontology, Historical Geology and Evolution of the Biosphere. The successful applicant also will be expected to collaborate with other faculty, guide graduate students, advise undergraduate students, seek extramural research funds, develop and maintain an active research program and participate in the general activities of the University.

The department works closely with the Division of Marine Geology and Geophysics at the Rosenstiel School located on the Key Biscayne campus approximately seven miles from the main campus.

Research interests of the current five faculty members range from coastal and climate evolution, to isotopic studies of the mantle, Caribbean ore deposits, volcanism, tectonics, coastal and shallow marine sedimentation, carbonate processes, sediment diagenesis and hydrology.

Applicants should submit a letter summarizing their research interests, a curriculum vitae and the names of three references to Dr. Harold Wanless, Chairman, Faculty Search Committee, Department of Geological Sciences, University of Miami, P.O. Box 249176, Coral Gables, FL 33124.

We expect to fill the position by August 15, 1993.

The University of Miami is an equal opportunity affirmative action employer and a smoke/drug free workplace. We do not discriminate on the basis of race, religion, color, sex, age, national origin, or disability. We encourage applications from females, veterans, the disabled, and minority group applicants.

MOORHEAD STATE UNIVERSITY invites applications for a fixed-term position in Earth Science at the Assistant Professor level (pending funding). Opportunity for tenure track position may exist starting Fall 1994. A Ph.D. in geology or related earth science is required. The applicant must provide evidence for potential excellence in teaching in classroom, laboratory, and field settings. Teaching responsibilities include a one-year geology sequence with laboratory appropriate for science and non-science majors, introductory meteorology, upper level geomorphology and an additional course in an area of interest. Participation in outdoor summer programs is required. This is a service program with a normal teaching load of twelve contact hours. Active research interests that may involve undergraduates preferred.

Applicants must submit a resume, three letters of reference, unofficial undergraduate and graduate transcripts and a Moorhead State University standard application form. Moorhead State University is an equal opportunity/affirmative action employer and educator. Screening date for consideration of applications is April 1, 1993 or until filled. Position may start as early as April 26, 1993 and continues through May of 1994.

Apply to: Dr. Water E. Worman, Department of Physics and Astronomy, Moorhead State University, Moorhead, MN 56563 (Phone: 218-236-2452; FAX: 218-236-2168).

HYDROGEOLOGIST / ENVIRONMENTAL HYDROSCIENTIST
Stanford University

The Department of Geological and Environmental Sciences at Stanford University is seeking to hire and environmental hydroscientist or hydrogeologist with broad geologic interests in the physical and chemical processes of subsurface fluid flow.

This is a tenure-track position for an Assistant or Associate Professor who would be an essential part of an interdisciplinary, quantitative hydrogeology program. The candidate should have strong commitments to graduate research and teaching, and demonstrate an enthusiastic willingness to participate in the undergraduate teaching program in the environmental sciences. The candidate is expected to interact closely with researchers studying subsurface fluid flow in the Department of Geophysics and Petroleum Engineering, as well as those in the Environmental and Water Studies Program in the School of Engineering.

The application deadline is March 19, 1993. Please send a letter of interest, a statement of teaching and research objectives, a curriculum vitae, a publication list, and the names and addresses of three references to: Professor Steven Gorelick, Department of Applied Earth Sciences, Stanford University, Stanford, CA 94305-2225.

Stanford University has a strong institutional commitment to the principle of diversity. In that spirit, we particularly encourage applications from women, members of ethnic minorities, and individuals with disabilities.

TECTONICS

The Department of Earth Sciences at Dartmouth College invites applications for a full-time tenure-track position, at the assistant professor level, in the broad area of quantitative tectonics, starting July 1993. A Ph.D. is required.

Teaching requirements include undergraduate structural geology and related undergraduate and graduate courses, including field studies. The successful candidate will be expected to develop a vigorous process-oriented research program in disciplines such as, but not restricted to, structural geology, regional tectonics, geophysics, or active tectonics. The candidate will be required to direct and support M.S. and Ph.D. research and should be prepared to interact with existing faculty and research programs in the department.

Send letter of application, statements of research and teaching interests, c.v., and names and addresses of three references to: C. Page Chamberlain, Chair, Department of Earth Sciences, Dartmouth College, Hanover, NH 03755. Applications will be processed on a rolling basis; however, we expect to begin interviewing in early April.

Dartmouth College is an equal opportunity/affirmative action employer.

Services & Supplies

LEATHER FIELD CASES. Free brochure, SHERER CUSTOM SADDLES, INC., P.O. Box 385, Dept. GN, Franktown, CO 80116.

Opportunities for Students

The Archaeological Geology Division of the GSA is pleased to announce a $300 travel grant for a student to attend the GSA Annual Meeting in Boston in October, 1993. The grant is competitive and will be awarded based on the evaluation of an abstract and 200 word summary of a paper prepared by a student for presentation at the GSA Annual Meeting. The abstracts and summaries should be submitted to the awards committee no later than July 1, 1993. The winner is expected to present his or her paper at the GSA Annual Meeting. Applications should be sent to E. James Dixon, Awards Committee Chairman, University of Alaska Museum, 907 Yukon Dr., Fairbanks, Alaska, 99775-1200 or faxed to (907) 474-5469.

JOI/USSAC Ocean Drilling Fellowships. JOI/U.S. Science Advisory Committee is seeking doctoral candidates of unusual promise and ability who are enrolled in U.S. institutions to conduct research compatible with that of the Ocean Drilling Program. Both one-year and two-year fellowships are available. The award is $20,000 per year to be used for stipend, tuition, benefits, research costs and incidental travel, if any. Applicants are encouraged to propose innovative and imaginative projects. Research may be directed toward the objectives of a specific leg or to broader themes.

Applications are available from the JOI office and should be submitted according to the following schedule: Leg 153: MARK 5/1/93; Leg 154: Ceara Rise, 5/1/93; Leg 155: Amazon Fan, 5/1/93; Leg 156: North Barbados Ridge, 5/1/93; Leg 158: TAG, 5/1/93; Shorebased Research (regardless of leg) 12/1/93.

These legs will be staffed during the next few months. Students interested in participating as shipboard scientists must apply to the ODP Manager of Science Operations in College Station, TX. An application form is available in the JOI/USSAC Ocean Drilling Fellowship application packet. For more information and to receive an application packet, contact: JOI/USSAC Ocean Drilling Fellowship Program, Joint Oceanographic Institutions, Inc., 1755 Massachusetts Ave., NW, Suite 800, Washington, DC 20036-2102 (Andrea Leader: 202-232-3900).

Department of Earth Sciences, Carleton University. The Department of Earth Sciences at Carleton University wishes to nominate an eligible applicant for an NSERC Women's Faculty Award, particularly in the following areas of specialization: Environmental Earth Sciences or Applied Geophysics with a major interest in environmental issues. The successful candidate will have a strong research background in the Earth Sciences. NSERC will give preference to nominees not more than five years from the doctoral degree. A nominee must be a Canadian citizen or permanent resident. This appointment will lead to a tenure-track position. The appointment is to commence July 1, 1994.

The department has sixteen faculty members, a strong honours program and an excellent graduate school (about fifty students evenly split between M.Sc. and Ph.D. programs). At the graduate and research level, close co-operation exists with the Department of Geology at the University of Ottawa (which jointly with Earth Sciences at Carleton forms the Ottawa Carleton Geoscience Centre) and the Geological Survey of Canada.

Applicants should send a curriculum vitae, a statement of current and future research interests, and the names of three references to: Dr. F. A. Michel, chairman, Department of Earth Sciences, Carleton University, 1125 Colonel By Drive, Ottawa, Ontario K1S 5B6. Application deadline is April 30, 1993.

GSA SECTION MEETINGS

South-Central Section, Texas Christian University, Fort Worth, Texas, March 15–16, 1993. John A. Breyer, Department of Geology, P.O. Box 30798, Sid Richardson Building, Corner of Bowie and Cockrell, Texas Christian University, Ft. Worth, TX 76129-0001, (817) 921-7270.

Northeastern Section, Sheraton Inn Conference Center, Burlington, Vermont, March 22–24, 1993. Barry L. Doolan or Rolfe S. Stanley, Department of Geology, Perkins Geology Hall, University of Vermont, Burlington, VT 05405-0122, (802) 656-0247.

North-Central Section, University of Missouri, Rolla, Missouri, March 29–30, 1993. Richard D. Hagni, Department of Geology & Geophysics, 125 McNutt Hall, University of Missouri–Rolla, Rolla, MO 65401-0249, (314) 341-4616.

Southeastern Section, Florida State Conference Center, Tallahassee, Florida, April 1–2, 1993. James F. Tull, Department of Geology, B-160, Florida State University, Tallahassee, FL 32306-3026, (904) 644-1448.

Cordilleran and Rocky Mountain Sections, Reno Hilton (formerly Bally's Hotel), Reno, Nevada, May 19–21, 1993. Richard A. Schweickert, Department of Geological Sciences, Mackay School of Mines, University of Nevada–Reno, Reno, NV 89557-0138, (702) 784-6050; or Walter S. Snyder, Department of Geosciences, Boise State University, Boise, ID 83725, (208) 385-3645, fax 208-385-4061. *Preregistration Deadline: April 23, 1993.*

CORRECTION:

Cordilleran–Rocky Mountain Sections Meeting—Field Trip 3.
Field Trip 3, "Mesozoic(?) Contraction in Central Nevada: The Central Nevada Thrust Belt" departs 7:00 p.m. FRIDAY, MAY 14, 1993, *not* Saturday, May 15. If you have any questions, please call the GSA Registration Coordinator at (303) 447-2020.

Student Travel Grants for Section Meetings

The GSA Foundation will award matching grants up to a total of $3500 each to the six GSA Sections. The money, when combined with equal funds from the Sections, will be used to assist students traveling to the 1993 GSA Annual Meeting in Boston in October and to the 1993 Section meetings. Contact your Section secretary for application procedures.

Cordilleran	Bruce A. Blackerby	(209) 278-3086
Rocky Mountain	Kenneth E. Kolm	(303) 273-3800
North-Central	George R. Hallberg	(319) 335-1575
South-Central	Rena M. Bonem	(817) 755-2361
Northeastern	Kenneth N. Weaver	(410) 554-5503
Southeastern	Michael J. Neilson	(205) 934-2439

Call for Nominations

1993 John C. Frye Environmental Geology Award

In cooperation with the American Association of State Geologists, GSA makes an annual award for the best paper on environmental geology published either by GSA or by one of the state geological surveys. The award is a $500 cash prize from the endowment income of the GSA Foundation's John C. Frye Memorial Fund.

The 1993 award will be presented at the autumn AASG meeting to be held during the GSA Annual Meeting in Boston. Members of the selection committee are Chairman Frank E. Kottlowski, New Mexico Bureau of Mines and Mineral Resources; John P. Kempton, Illinois Geological Survey; and Diane L. Conrad, Vermont Division of Geology and Mineral Resources.

Criteria for Nomination

Nominations can be made by anyone, based on the following criteria: (1) paper must be selected from GSA or state geological survey publications, (2) paper must be selected from those published during the preceding three full calendar years, (3) nomination must include a paragraph stating the pertinence of the paper, (4) **nominations must be sent to Executive Director, GSA, P.O. Box 9140, Boulder, CO 80301.** *Deadline: March 31, 1993.*

Basis for Selection

Each nominated paper wil be judged on the uniqueness or significance as a model of its type of work and report and its overall worthiness for the award. In addition, nominated papers must establish an environmental problem or need, provide substantive information on the basic geology or geologic process pertinent to the problem, relate the geology to the problem or need, suggest solutions or provide appropriate land use recommendations based on the geology, present the information in a manner that is understandable and directly usable by geologists, and address the environmental need or resolve the problem. It is preferred that the paper be directly applicable by informed laypersons (e.g., planners, engineers).

1992 Recipients

Recipients of the 1992 award presented at the GSA Annual Meeting in Cincinnati are Edwin J. Hartke and Henry H. Gray, Indiana Geological Survey, for their report "Geology for environmental planning in Monroe County, Indiana," Special Report 47 (1989), Indiana Geological Survey.

ISSN 1052-5173

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Boulder, CO 80301

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