THE GEOCHEMICAL NEWS

Quarterly Newsletter of The Geochemical Society

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Harmon Craig Wins Balzan Prize

In this issue:

9th V.M. Goldschmidt Conference
Harvard University, Cambridge, MA, U. S. A.
August 22-27, 1999

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(see page 9 for more information)

THE GEOCHEMICAL SOCIETY

The **Geochemical Society** is a nonprofit scientific society founded to encourage the application of chemistry to the solution of geological and cosmological problems. Membership is international and diverse in background, encompassing such fields as organic geochemistry, high and low-temperature geochemistry, petrology, meteoritics, fluid-rock interaction, and isotope geochemistry. The Society produces a **Special Publications Series**, ***The Geochemical News*** (this quarterly newsletter), and (jointly with the Meteoritical Society) the journal ***Geochimica et Cosmochimica Acta***; grants the **V.M. Goldschmidt, F.W. Clarke** and **Clair C. Patterson Awards**, and, jointly with the European Association of Geochemistry, the **Geochemistry Fellows** title; sponsors the **V.M. Goldschmidt Conferences**, held in North American in odd years and elsewhere in even years, jointly with the European Association of Geochemistry; and co-sponsors the Geological Society of America annual meeting and the spring meeting of the American Geophysical Union. The Society honors the memory of our first President, F. Earl Ingerson, and our first Goldschmidt Medalist, Paul W. Gast, with the **Ingerson** and **Gast Lectures**. The Geochemical Society is affiliated with the American Association for the Advancement of Science and the International Union of Geological Sciences.

Members of the **Organic Geochemistry Division** are individuals with interests in studies on the origin, nature, geochemical significance, and behavior during diagenesis and catagenesis of naturally occurring organic substances in the Earth, and of extraterrestrial organic matter. GS members may choose to be affiliated with the OGD without any additional dues. The OGD presents the **Alfred E. Treibs Award** for major achievements in organic geochemistry, and **Best Paper** awards (student and professional) in organic geochemistry.

Editor's Corner

Several readers noted the tardiness of the last issue. This was caused by a delay at the printers. We have changed printers in the hope of avoiding such delays, and other production problems, in the future.

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Letter from the President of the Geochemical Society

Dear Colleagues:

1998 was a great year for geochemistry, capped off by the outstanding Goldschmidt Conference held in Toulouse, France. Our thanks go the local organizers, led by Jacques Schott and Eric Oelkers. We have a lot to look forward to in 1999, especially the next Goldschmidt Conference, to be held in Cambridge, Massachusetts from August 22-29, 1999. Check http://cass.jsc.nasa.gov/meetings/gold99 for details or contact Stein Jacobsen at goldschmidt@eps.harvard.edu. I encourage all of you to attend what promises to be a fine meeting.

As most of you know, Karl Turekian is stepping down after one term as Editor of Geochimica et Cosmochimica Acta at the end of 1999. Karl has a unique style involving high standards and a strong will, which has led to a bumpy ride for many authors. But I think all of us applaud Karl for his vision of an intellectually first rate journal, and I thank Karl for the enormous time and effort he has put into editing Geochimica et Cosmochimica Acta. The next Editor has a hard act to follow.

Other changes are in progress. The Mineralogical Society of America has joined the Geochemical Society in cosponsoring the North American Goldschmidt Conferences. We look forward to North American meetings which rival those in Europe in size and scope.

One of the most important things our Society does is confer awards on its most accomplished members. Awards honor both the individual receiving the award, and the Society by pointing to the importance of our field. Please be proactive in nominating individuals for the Clarke, Goldschmidt, Treibs, and Patterson medals of our Society. There are far more deserving candidates than there are opportunities to honor. Pay special attention to overlooked groups, by discipline, gender, age etc.

We have for many years had an extremely successful OrganicGeochemistry Division, and both its existence and success has led to periodic questions about forming other divisions. I am personally not a big fan of formal Divisions in a small society like ours, but I ask you to respond to a survey elsewhere in this issue on the desirability of forming divisions. I do favor forming a Student Division but, with rare successful exceptions such as the Organic Geochemistry Division, find that discipline-based divisions divide rather than unify. Any thoughts you have on making more geochemists find that the Geochemical Society is their natural home are welcomed. I subscribe to the "Big Tent" philosophy.

Here's to an outstanding meeting in Cambridge, MA!

Best wishes for the New Year and all of 1999,

Mike

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GCA Production Schedule

As most of the membership has undoubtedly observed, GCA has been arriving many months after the date printed on the spine, in spite of our policy of timely delivery. Elsevier has changed the way in which GCA is produced, and the journal is now on an accelerated production schedule so that it will again be delivered on time by this summer. Please be patient as Elsevier works through and solves the problems that have delayed production. We apologize for the inconvenience that this has caused the membership.

Michael J. Drake
President

A Message from Elsevier

"Elsevier apologises for the excessive delay in the publication of the remaining issues of Volume 62 (1998). It is now anticipated that 62/17 will be dispatched around February 5, 62/19 in the week of March 1, 62/20 and 21 in the week of March 8, 62/ 22 and 23 in the week of March 15 and 62/24 in the week of March 22.

Unfortunately, due to unforeseen circumstances, 62/18 was dispatched before 62/17 and should already have reached you.

Following completion of Volume 62 publication of Volume 63 will commence and it is hoped the journal will be back on schedule by the middle of the year.

For more up to date information on Volume 62 please contact Lee Mobley at mobley.2@osu.edu and for Volume 63 please contact John Eagleson at j.eagleson@elsevier.com."

Halliday Wins Bowen Award

Alexander N. Halliday, Past-President of The Geochemical Society, was awarded the 1998 Norman L. Bowen Award of the American Geophysical Union at a ceremony held at the Fall AGU meeting in San Francisco in December 1998. The N. L. Bowen Award was established in 1981 by the Volcanology, Geochemistry, and Petrology Section to recognize an outstanding contribution to volcanology, geochemistry, or petrology made during the preceding five years. Halliday was cited for his outstanding work in isotope geochemistry.

Notes from the Business Manager

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Important!!! Poll of The Geochemical Society Membership:

There has been periodic discussion of splitting Geochimica et Cosmochimica Acta into two sections (one high-T/planetary, the other low-T/aqueous/environmental). Loosely related is the question of whether the Geochemical Society would be better organized in a series of Divisions analogous to the Organic Geochemistry Division. It would help the Board in its deliberations if you could take a few minutes to respond to the following questionnaire.

1. Should the Geochemical Society be composed of divisions, in addition to the currently very successful Organic Geochemistry Division?
2. Regardless of your answer to question 1, if you had to join a Division, what would be your preferred title for the Division?
3. Regardless of your answer to question 1, would you support the concept of a Student Division? If yes, should there be a student member of the Board of Directors?
4. Do you favor splitting the journal and offering each half for half price and the whole for slightly less than the sum of the parts to members? Institutional subscriptions would remain unchanged.

Please mail or e-mail your response promptly to our Business Manager, Lee Mobley at the address shown above.

Call for ideas and interest in the formation of a Student Division of the Geochemical Society

The Geochemical Society is extremely interested in the involvement of earth science students in its activities, and in enhancing their educational experience. For several years, we have been providing funds to Goldschmidt Conference Organizers ($10K per year) to provide student travel grants to attend the Society's Annual meeting. Last year, the Board agreed to heavily subsidize the membership dues and subscription cost to Geochimica et Cosmochimica Acta for student members, with rates set at $5 and $45, respectively, as compared with $25 and $122 for professional dues and subscription rates. We think this is an exceptional bargain, and we are already seeing a significant increase in student involvement in the Society as a result.

Our Business Manager, Lee Mobley, who staffs the GS booth at international meetings such as GSA, AGU and Goldschmidt, has indicated that many students have expressed an interest in playing an active role in the Society. Toward that end, we would like to invite interested student and professional geochemists to send us your ideas of how a Student Division of the Society might be organized. A possible scenario could be the formation of an organizational structure including officers and committees, who might communicate electronically as well as at international meetings. A Student Division could address issues such as the appropriateness of earth science curricula, the problems encountered by students in the pursuit of their degrees and ultimately their job searches, financial support and working conditions, international communications, and recognition for research accomplishments. One possibility is for a student representative to attend the annual GS Board of Directors meetings and take an active role in helping identify ways in which the Society can better serve its student members.

The Organic Geochemistry Division of the GS confers an annual Best Student Paper Award. However, there is no current award or honor in general geochemistry that is conferred upon students by their peers, and this is another issue that might be addressed by a Student Division. Furthermore, the establishment of our new web site and newsletter offer unprecedented opportunities for students to develop chat groups, search for jobs, advertise their qualifications for employment, and air their ideas and problems before the international geochemical community.

We encourage students and professionals to send in your ideas and/or expressions of interest to our Business Manager, Lee Mobley, at the address given above.

The Fifth International Symposium on the Geochemistry of the Earth's Surface (GES-5)

August 12-20, 1999, Reykjavik, Iceland

Abstract deadline: February 15, 1999.

The Fifth International Symposium on the Geochemistry of the Earth'sSurface (GES-5) will be held in Reykjavik, Iceland, from Monday August 16 to Friday August 20. There will be a field excursion prior to the meeting from Thursday August 12 to Sunday August 15 and an afternoon excursion during the meeting. The symposium has been scheduled so that those interested can attend the Goldschmidt meeting in Boston, U.S.A (August 22-27), after GES-5 in Iceland.

GES is a Working Group of the International Association of Geochemistry and Cosmochemistry (IAGC). Past meetings of the Working Group have been held in Granada, Spain (1986), Aix-en Provence, France (1990), University Park, Pennsylvania, USA (1993), and Ilkley, Yorkshire, England (1996).

Symposium themes

1. Geochemical record of terrestrial environmental change
2. Human geochemical impact on the terrestrial environment, local to global
3. Environmental geochemistry and health
4. Chemical weathering and climate, river catchment, and global cycles
5. Organic geochemistry
6. Marine and sedimentary geochemistry
7. Chemistry, physics and mineralogy of weathering processes
8. Geochemical thermodynamics and kinetics
9. Geochemistry of crustal fluids
10. Geochemistry of catastrophic events

For further information regarding the conference and to be added on the mailing list for further announcements, send an e-mail to the organizers (ges5@raunvis.hi.is) or visit the web site at http://www.raunvis.hi.is/ges5.html

C O N T A C T

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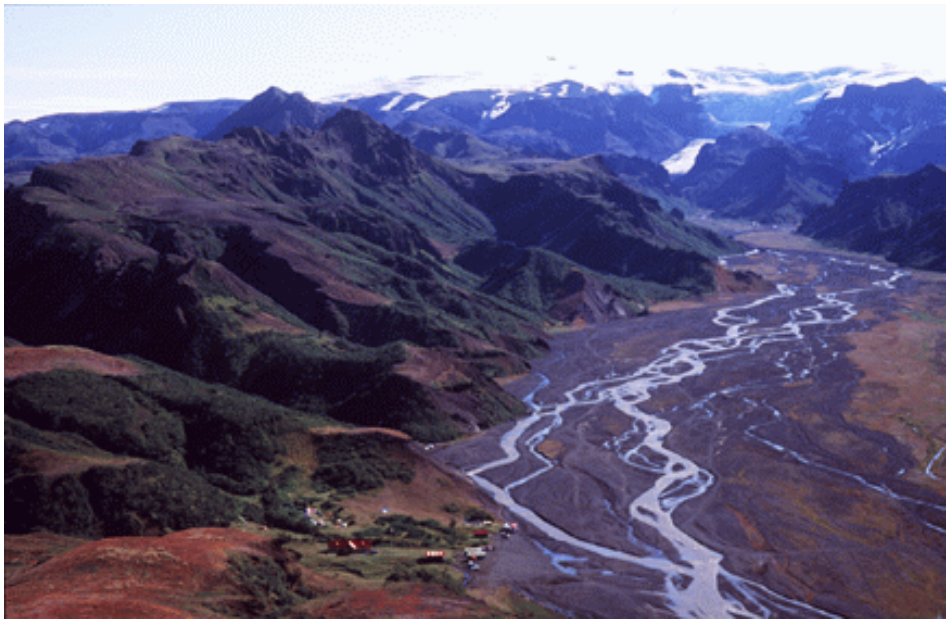

THE SYMPOSIUM IS SPONSORED BY:

University of Iceland
International Association of Geochemistry and Cosmochemistry

CANDIDATES SOUGHT FOR THE EXECUTIVE EDITORSHIP OF

GEOCHIMICA ET COSMOCHIMICA ACTA

Karl Turekian, Executive Editor of *Geochimica et Cosmochimica Acta*, has announced that he will not seek or accept reappointment beyond his current term, which runs through the end of calendar year 1999.

Sponsorship of a professional journal is one of the most important functions of a scientific society, and editorship of the journal is accordingly one of the most important ways in which an individual may serve the scientific community. The job is time-consuming, sometimes involves hard choices, requires attention to both detail and the big picture, but is not thankless or without gratification. On behalf of the Geochemical Society and the Meteoritical Society, which jointly co-sponsor *Geochimica et Cosmochimica Acta*, I would like to extend our sincere gratitude and appreciation to Karl for his effort, sacrifice and leadership in performance of this critical responsibility.

Nominations are now invited for Karl's successor as Executive Editor of *Geochimica et Cosmochimica Acta*, for a term formally beginning January 1, 2000. Any member of either society may submit a nomination, including self-nomination, through January 31, 1999. Nominations should be sent to the undersigned, preferably by e-mail (sposito@nature.berkeley.edu); but, if that is not convenient for the nominator, by express mail [Hilgard Hall #3110, University of California, Berkeley, CA 94720-3110; telephone: (510) 643-8297] or by FAX (510-643-2940). All candidates will be given serious consideration, and there are no restrictions on race, gender, disciplinary specialization or geographic location. Candidates will initially be evaluated by the Joint Publications Committee of the two societies, which will present recommendations to the societies' governing boards; the societies will then propose the new Executive Editor to the publisher, Elsevier.

The Executive Editor will receive a modest honorarium from Elsevier, along with financial support for office assistance and journal-related travel. Although the formal term of office begins on January 1, 2000, in order to allow an orderly transition it is expected that the new Executive Editor will begin to assume responsibilities in the latter part of 1999.

Garrison Sposito, Chair
Joint Publications Committee of the Geochemical Society and the Meteoritical Society

In Memoriam

Hans Oeschger

Roger Revelle would be proud to learn that Hans Oeschger was to receive the medal bearing his name because Hans' research encompassed so many important aspects of the earth's carbon cycle. So would the late Fritz Georg Houtermans who served as Oeschger's professor and inspired him to a career of leadership in isotope geochemistry. During his 40-year long career in the Physics Department at the University of Bern, Hans together with his colleagues and students, pioneered many innovations which have led to a better understanding of how the carbon cycle currently operates, how it might have differed during glacial time, and how human activity might alter it in the future.

A few of the accomplishments covered by his 200+ professional publications include: The measurement of ^{14}C first in the mid-1950s by ultra low level counting and then by accelerator mass spectrometry in the mid-1980s in cooperation with the Eidgenossische Technische Hochschule (ETH) Zurich group; together with Siegenthaler, the modeling of bomb ^{14}C, and fossil fuel CO2 uptake by the ocean using the box diffusion model; together with Berner and Stauffer, the first measurements of the CO2 content of glacial age air trapped in ice cores; together with Loosli , measurements of the ^{39}Ar distribution in the deep ocean; and the realization that the record preserved in Greenland ice required that the Earth's climate system have multiple states of operation.

In addition to his superb scientific contributions, Hans can take credit for nurturing the careers of many young scientists. Among these are Martin Heimann now at the Max Plank Institute in Hamburg; Martin Wahlen now at the University of California, San Diego in La Jolla; Hugo Loosli in Bern; Jurg Beer at the Swiss Federal Institute for Environmental Science and Technology in Zurich; and of course the late Uli Siegenthaler.

In his quiet way, Hans consistently pushed for two goals in the world's scientific-political arena. First, he was a strong advocate of ice core research in both polar regions. Second, he felt strongly that by loading the atmosphere with greenhouse gases, mankind was putting the Earth's climate at risk. In hopes that this loading might be eased, he pushed for the strongest possible language in international documents such as the International Panel on Climate Control report.

I might sum up by saying that Hans' career provides an exemplary portrait of what society expects of us; namely, excellent innovative research, but with an eye toward discoveries which yield benefits to mankind and to the preservation of the environment.

Wallace S. Broecker
Lamont-Doherty Earth Observatory of Columbia University

Meeting Report

Biogeochemistry of Iron in Seawater

The 24th General Meeting of the Scientific Committee for Oceanic Research (SCOR) as well as the Third International Symposium on Iron in the Sea took place in Amsterdam, The Netherlands in early November 1998. The Royal Netherlands Academy of Arts and Sciences provided the colorful setting. The Academy is housed in the so-called Trippenhuis, one of Amsterdam's many souvenirs from the days when the Dutch explored the world, traded and carried back treasures from several continents. Quite appropriately, the Trip brothers who had this twin set of houses built, were arms dealers and the metal iron held plenty of significance for them. The symposium on iron was organized by SCOR Working Group 109 Biogeochemistry of Iron in Seawater. Each year, the SCOR General Meeting assigns two or three working groups to focus on a specific topic. WG 109 was proposed by Keith Hunter (SCOR, University of Otago, Dunedin, New Zealand) and David Turner (IUPAC, Göteborg University, Göteborg, Sweden) and was approved in 1996, by SCOR and IUPAC jointly. The group's tasks included the production of a major publication (IUPAC series, John Wiley) and one of the goals of the November meeting was to work on this book. Part of the collective know-how going into the book was communicated to a wider audience in the form of presentations and posters.

More than 50 participants from 12 countries flocked toward the iron meeting. All of them brought knowledge to Amsterdam, somewhat reminiscent of 17th-century merchants shipping back valuable goods, as was cleverly observed by Hein de Baar, chair Netherlands SCOR Committee, in his introduction. The symposium started on Sunday, November 1 and lasted throughout the week. On Tuesday evening, the learned colleagues went to the 'Concertgebouw' where in addition to Wagner and more, they were treated to a kind of ceremony most Dutch citizens never witness. The Concertgebouw Orchestra celebrated its 110th anniversary and Beatrix, Queen of The Netherlands, awarded a knighthood to the orchestra's Italian conductor of the last 10 years, Riccardo Chailly. A trip to the Netherlands Institute for Sea Research NIOZ was scheduled on Wednesday, but those working on the book remained in Amsterdam, doing just that.

Iron is believed to be limiting for marine algae in three large oceanic areas, called High Nitrate Low Chlorophyll (HNLC) regions, and in coastal upwelling regions. Solubility, diffusion, speciation, siderophores and other ligands, atmospheric input, photoreduction, and protozoan grazing are some of the factors that determine iron limitation. For obvious reasons, there is a steady increase in the degree of co-operation between biologists and marine geochemists on the subject. Examples of organisms being studied within the iron limitation scope are *Emiliania huxleyi*, *Prochlorococcus* and *Phaecocystis*. E hux is one of the major calcifiers and Cemal Saydam et al. demonstrated how wet dust deposition over the Mediterranean and the Black Sea appears to be able to induce major E hux blooms, with a patchiness that closely resembles atmospheric patterns. Atmospheric iron input is not always linked to blooms, however, a fact which perhaps can be explained by mineralogical factors (solubility). Atmospheric input is the predominant form of iron supply to the oceans, wrote Lucinda Spokes and Tim Jickells. In coastal upwelling areas however, the influence of rivers cannot be ignored, as became evident in results from Ken Bruland and Eden Rue, for waters off Central California. The presentations at the symposium embraced many other themes, such as classification and characterization of iron limitation regimes, photosynthesis, and solubility and speciation. A presentation by Frank Millero and Xuewu Liu showed solubility data for Fe(OH)3 (in NaCl solutions, in order to avoid Mg precipitation at higher pH). Their data between pH 6 and 7.5 exhibit a trend that resembles seawater solubilities but which cannot be explained by speciation alone and is suspected to involve a phase change (solid state). At pH>8, their solubilities in NaCl are lower than those known for seawater. Future research will incorporate effects of Mg and sulphate on solubility in NaCl solutions at pH 8 and of pH, T and S in Gulf Stream water. By the way, if there had been prizes for sparkling presentations, with the latest in presentation technology, they would very likely have gone to Cemal Saydam and Frank Millero.

Following the symposium, there was a small 'iron certification' meeting, during which the route towards international analytical standardization was considered, and after that, the inevitable canal tour. A private meeting, dedicated to ironing out any wrinkles in the reviewing and report drafting processes related to the book, wrapped things up. In summary, one could say that the symposium was a big success, undoubtedly providing inspiration to many. Those regretting to have missed this meeting may decide to catch up by contacting Hein de Baar at debaar@nioz.nl and request a copy of the symposium volume with abstracts.

Angelina W. M. G. Souren
Vrije Universiteit, Amsterdam

Harmon Craig Wins Balzan Prize

Harmon Craig, a professor of oceanography and geochemistry at the Scripps Institution of Oceanography, University of California, San Diego, has been awarded the Balzan Prize for his fundamental contributions to the field of geochemistry. The Balzan Prize of the International Balzan Foundation of Milan, Italy, has several times been given in astrophysics and geophysics, but this is the first award in geochemistry. Craig was presented with the award by the President of Italy at a ceremony held in the Palazzo del Quirinale in Rome on Nov. 23, 1998.

The Balzan Prize is considered the equivalent of the Nobel Prize in the fields of natural sciences, humanities, social sciences and international affairs that are not in Nobel awards categories. The Balzan Prize was established in 1961 by the late Italian heiress Lina Balzan in memory of her father, publisher Eugenio Balzan. Three awards were made this year. Andrzej Walicki of Poland and the United States was awarded the prize for history and Sir Robert May of Australia and the United Kingdom received the award for his work in biodiversity.

Craig was recognized by the Balzan Foundation for his work as "a pioneer in earth sciences who uses the varied tools of isotope geochemistry to solve problems of fundamental scientific importance and immediate relevance in the atmosphere, hydrosphere and solid earth."

Born in New York City on March 15, 1926, Craig did his thesis on carbon isotope geochemistry under Nobel Laureate Harold Urey. After receiving a Ph.D. in geology-geochemistry from the University of Chicago in 1951, Craig stayed on as a research associate at the Enrico Fermi Institute for Nuclear Studies at the University of Chicago. During this time he and Urey discovered that meteorites fall into discrete groups based on their oxidation states and content of iron. He went on to study the distribution of heavy hydrogen (deuterium) and oxygen isotopes in natural waters, establishing the "Global Meteoric Water" relationship of these isotopes which has become fundamental for studies in hydrology and climatology.

A faculty member at Scripps since 1955, Craig has ventured to some of the remotest spots on Earth in search of elusive gases, rocks and other materials that provide clues to the composition of the Earth's interior. In his quest, he has descended into the crater of an active underwater volcano, led the first dives into the 2-mile-deep Mariana Trough, and sailed atop an erupting undersea volcano to collect rock and gas samples. He has led 28 deep-sea oceanographic expeditions and has made 17 dives to the bottom of the ocean in the ALVIN submersible. His daresome adventures have yielded a host of significant scientific findings that have greatly enriched our understanding of the workings of the oceans, atmosphere and deep Earth.

In recognition of his scientific achievements, Craig has received many honors. He was elected to membership in the National Academy of Sciences in 1979. He received the V.M. Goldschmidt Medal of the Geochemical Society in 1979, the National Science Foundation "Special Creativity" Award in Oceanography in 1982, the Arthur L. Day Medal of the Geological Society of America in 1983, and the honorary degree of Docteur (Honoris Causa) of the University de Paris (Pierre et Marie Curie) in 1983. In 1987, he was awarded the Arthur L. Day Prize of the National Academy of Sciences and was co-recipient of the Vetlesen Prize from Columbia University. In 1991, he was awarded an honorary doctorate degree from the University of Chicago, and in 1993 he was named an honorary fellow of the European Union of Geosciences.

Valerie and Harmon Craig at work in the office, Scripps Institution of Oceanography, La Jolla, California

Ninth Annual V. M. Goldschmidt Conference

August 22–27, 1999 Cambridge, Massachusetts

http://cass.jsc.nasa.gov/meetings/gold99/

Sponsors

Geochemical Society
European Association of Geochemistry
Lunar and Planetary Institute
Harvard University
National Aeronautics and Space Administration

The Ninth Annual Goldschmidt Conference will be held at Harvard University, Cambridge, Massachusetts, August 22–27, 1999. The conference will be hosted by the Department of Earth and Planetary Sciences. Professor Stein B. Jacobsen is the Conference Chair.

The Goldschmidt Conference is an annual international conference of geochemists, and is held alternately in Europe and North America. The meeting is a forum for presenting and discussing new chemical and isotopic measurements, experimental and theoretical results, and discoveries in geochemistry and cosmochemistry.

The program committee welcomes your suggestions for potential symposia topics, and solicits volunteers to act as symposia organizers. Please send your suggestions for topics and offers to organize to goldschmidt@eps.harvard.edu.

Schedule

March 5, 1999	Second announcement mailing, including logistical and registration information and call for abstracts
May 21, 1999	**Abstract deadline**
June 28, 1999	Final announcement mailing
August 22–27, 1999	Ninth Annual Goldschmidt Conference
August 28–29, 1999	Field trips

Contact

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In Memoriam

Mitsunobu Tatsumoto

With the passing of Mitsunobu Tatsumoto, or "Tats" as he was affectionately known, geochemistry has lost one of its most dedicated and determined geochemists ever. Tats, a native born Japanese, was brought up in Korea where his father was a silk scientist. He was quite an athlete and was a member of the Korean Olympic hockey team as World War II broke out ending his major sports career. He was drafted into the Japanese army where he served as a lathe operator. After the war, he went to what was then the Tokyo University of Education and did his graduate work on marine geochemistry with Prof. Miyaki, who led the team that worked out the makeup of the hydrogen bomb from fallout on the fishing ship "Lucky Dragon."

After receiving his Ph.D. he went as a post-doctoral fellow to Texas A & M for a year and then joined Ed Goldberg at Scripps Institution of Oceanography. Ed recommended Tats to his old class mate Claire C. ("Pat") Patterson in 1959 which is when I met him. Tats worked out the procedures of purifying very small quantities of lead using ion exchange resins, a technique Pat thought wouldn't be any good and told him to stop. At night Tats would break out the fraction collector he had scrounged from somewhere and set to work on his ion exchange columns again. He told me that he worked 100 percent for Pat during the day and another 100 percent on things he thought were important at night. In a relatively short time he succeeded and proceeded to determine the lead content of ocean water, finding lead in the nanograms/liter range (with Patterson, 1964). The analytical work was successful, but unfortunately the ocean water samples collected in open buckets were contaminated from the ships wake, ships being essentially floating blocks of lead where lead geochemistry is concerned. Later others repeated the experiment with "bottles in bottles" that were lowered and raised sealed through the ships wake and found lower lead contents from samples collected only opened below the wake, but this did not detract from Tats' fine pioneering work. Also with Patterson was a very fine paper on beach sands modeling their lead isotopes. Although others had modeled lead isotopes before, this was the first paper where the parameters had identified physical significance (also 1964). Tats and Pat also did a small study of zircon in sandstone, a kind of investigation resumed years later by Allegre and his colleagues with great effect looking for very old zircons.

In 1960 the U.S. Geological Survey started forming their famous Branch of Isotope Geology, now deceased. Although Tats was not an American citizen, he was able to obtain a temporary appointment because so few American citizens were trained in the field. I rejoined Tats in Denver in 1963. A.E.J. Engel had identified a type of basalt in the ocean basins called oceanic tholeiites that came to be called Mid-Ocean Ridge Basalts or MORB. Tats' fame had spread and Engel sought him out to convince him to do a lead isotope study on these rocks that Engel thought represented a homogeneous mantle under the oceans. This request seemed simple enough because we had a clean laboratory in Denver (known at the time by the buildings people in Denver as the Gold Brick) and a new National Bureau of Standards mass spectrometer earned by doing a study on 234-U in soil profiles with John Rosholt (1966). Also, Tats was already analyzing basalts from Hawaii and Japan. The lead contents in most samples of MORB turned out to contain well below one part-per-million lead, a range in silicate rocks that had never been successfully analyzed. So laboriously, Tats set to work devising new techniques to determine such low lead contents. His success in this endeavor was published in 1964 (as was the work on Hawaii and Japan). It was in this paper on MORB that Tats came up with the lead isotope mantle isochron. In a paper published in 1969, Tats expanded upon his lead isotope work in Japan and found a gradient in the isotopes across Japan from the tholeiites on the Pacific Ocean side to the alkali basalts in the Japan Sea that he interpreted to represent decreasing contributions of pelagic sediment lead to the basalt magmas formed at deeper horizons across the island arc.

Some time in the 1960s found Tats and I having dinner at the Patterson's in Pasadena. They had their own swimming pool. The four Patterson children, all rather young at the time, challenged Tats that they in tandem could swim farther under water than he could by himself. Tats, still proud of his athletic ability took the bet. I tried to restrain him and reminded him that this was their pool. They probably had been practicing and knew what they were doing. But Tats wouldn't be deterred. Tats won but as he sat panting by the side of the pool said that he wished he had given up.

Then the Apollo lunar sample program began, and Tats was accepted as a principal investigator to study lead isotopes in lunar samples because of his previous work on ultra-low lead contents in ocean water and MORB. The
lunar samples proved to have unusually high ratios of uranium to lead making lunar samples rather similar to the mineral zircon in this regard (interestingly a characteristic that had been proposed by Leon T. Silver). The 204-Pb content thus proved to be unusually low because the uranium content was much lower than in most zircons, causing more new methods to be developed with painstaking care, assisted by Daniel Unruh who has subsequently developed his own career. Sometimes there was more 204-Pb in our lead blank than in the sample analyzed!

In 1969, Tats had a severe heart attack. He gave up smoking and began serious exercising. For a number of years, he would occasionally also go fishing, often with his wife Kimiko who was an expert fisherman. I had had to resign my co-investigator status with Tats in lunar samples because I had taken a temporary position at National Aeronautics and Space Administration headquarters to help completing setting up of the Lunar Sample Program. After Tats' heart attack, I returned to Denver and resumed my co-investigator status at Tats' request until he recovered. This period resulted in a number of coauthored papers between us. In 1979, Tats' health was failing again, and he had a triple by-pass operation. Males in Tats' family tended to die early. Tats was the first to live past his 50th birthday, and he worked well into his 70s.

Tats had a great sense of loyalty and a goal of passing the best of geochemistry along to the coming generation. He had

set up a lead isotope facility in Paris for Claude Allegre, and the French wanted to give him an honorary doctorate for his many contributions, but Tats would not hear of it unless his mentor in lead isotopes, Claire Patterson, also received one, which he surely deserved. They both did receive honorary doctorates. There was a steady stream of young scientists from all over the world through Tats' labs and under his guidance, too many to list here, as well as established older scientists. Many of these younger scientists have gone on to their own highly successful careers as leaders in geochemistry. But one in particular deserves mention. Tats for many yearly tried to
analyze the luticium-hafnium isotope system. Eventually, he was joined by John Patchett who was successful for which Patchett received the Clarke Medal of the Geochemical Society. These young people (young by my standards) would tell you that Tats was a stern taskmaster, but he didn't ask anything of others that he would not do himself. If you run across one of these people, I am sure they will portray their strong devotion to Tats. Thus a special symposium was held in Tats' honor at the 1996 Goldschmidt conference in Tucson, Arizona, and Monograph 95 of the American Geophysical Union was published the same year, dedicated to Tats and George Tilton. Tats is survived by his wife, a daughter (Marie, a lawyer) and a son (Kumi, a biochemist).

Tats had a dry sense of humor that often might be overlooked because it was delivered with a blank or even stern face. In 1963 just after I had moved to Denver, our boss, Samuel Goldich, came for a visit. Tats took us to a Chinese restaurant for dinner in the VFW Club in Denver. We were sitting in the lounge having a drink and talking. Finally Sam Goldich turned to Tats and said, "Say, how can you take us here. This if the VFW Club, that means Veterans of Foreign Wars." Without missing a beat Tats replied, "No problem, I am a veteran of the Japanese army."

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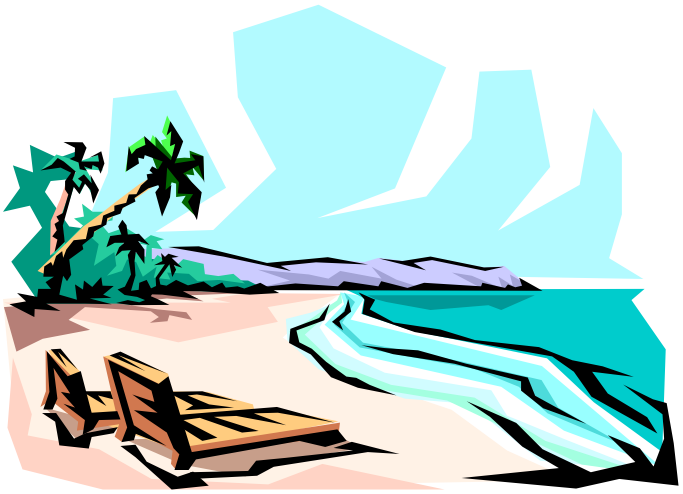

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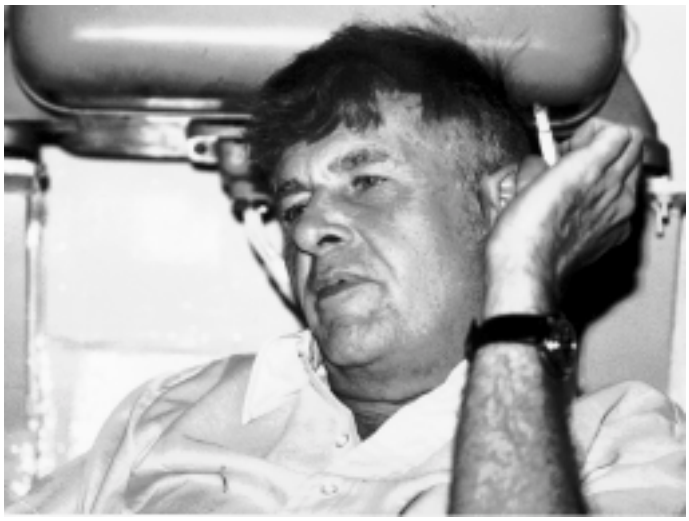
A Conversation with Harmon Craig



The following is excerpted from a conversation between Harmon Craig and Neil Sturchio (representing The Geochemical News*) that took place on Sunday, December 7 1998, at the bar of the San Francisco Marriott Hotel.*

On the early days with Harold Urey at the University of Chicago:

NCS: You were working on a dead-end geology thesis for a year, and realized it was headed nowhere, and then another grad student suggested that you speak with Harold Urey.

HC: That was Sol Silverman. He was the first geology student to go into Urey's lab and he worked on ^{18}O in silicate rocks. He got there by way of Peter Baertschi, a Swiss geologist who had come over to work with Urey before my time, who was working on oxygen isotopes in rocks. Then Sol got interested in it and decided to go for it as his thesis. Sol was working in Urey's lab when I decided that my thesis wasn't working out at all. It was under Walter Newhouse, the chairman of the department, who had gotten from the geochemists at Chicago, Tom Barth and Hans Ramberg, the idea that granites were formed by solid diffusion. He was convinced that the gneisses in the Laramie Range where he was working were formed by solid-state diffusion. I was originally working under Tom Barth, but he was offered the directorship of the Mineralogisk Museum in Oslo and when he left to go back there, I transferred over to Newhouse.

If you want to read about the dialectic that was going on at Chicago at this time, there's no better place than Francis Pettijohn's book *Memoirs of an Unrepentant Field Geologist.* There's a chapter about Chicago at that time and the opposing groups of Newhouse and his gang who would sit on one side of the table and all the other geologists at the other end of the table. I had been studying petrology and chemistry, petrology mainly with Barth, Ramberg and Robert Balk, with whom I was doing structural petrology, and my thesis was to look at the gneisses in the Park Range, which is the westernmost of the three fingers of the Rockies that stick up into Wyoming.

We had a summer field camp near there at Centennial, Wyoming, which we had all been to. And I was studying the chemistry of the gneisses, by their mineralogy, with the petrographic microscope, because there were no facilities for chemical analysis in those days, so it was all microscope work, correlating the fabric of the gneisses with their composition. And of course I loved the field work, but I didn't find any correlations. In fact, these rocks did not form by solid-state diffusion.

So I was pretty disappointed in this after a year, and then Sol Silverman, a good friend of mine, said "Why don't you go over and talk to Urey, he's looking for students?" Urey couldn't get students from the chemistry department. They didn't want to take a chance on something like paleotemperatures or isotopes, which hardly any of them had heard of, as far as variations in nature. So Urey was getting students from the geology department. I think I was the first after Silverman. And Urey right away said, "Fine. You'll work on carbon." He said, "I only demand two things of you. You must learn glass-blowing and take organic chemistry." I had had a lot of chemical thermodynamics and physical chemistry, but I had never taken organic chemistry because I disliked it. Still do. Then I said, well, on the first one, of course, I want to learn glass-blowing, but on the second one my theory is you can read books about organic chemistry. And Urey hesitated and said, "Yes, I guess that's right. You can read organic chemistry". So he let me off on that one, and I just had to learn glass-blowing, whichSam Epstein taught me. But I never read any organic chemistry.

NCS: Can you tell me about some of the other students who followed you into Urey's lab?

HC: Cesare Emiliani came over about the same time or maybe a little after. He was from Bologna and he was working in the Geology department. He got a second Ph.D. from Chicago. Urey and Sam Epstein were working on oxygen isotopes in belemnites, and in oysters and other modern mollusks, because they had to compare them with the mollusks of the Cretaceous. The assumption here was that the ^{18}O in ocean water hadn't changed since the Cretaceous, which even at that early stage I think a number of us recognized as a very questionable assumption. We had no idea what controlled the $^{18}O/^{16}O$ ratio in the ocean. But they sort of made light of that in their papers and discussions. Heinz Lowenstam persuaded Urey that they should have someone working on microfossils. That was the real area. Nobody knew whether they would exchange rapidly in the sediments or what. But Cesare happened to be an expert on microfossils and it was a golden opportunity.

So Cesare came into the lab, and had the great idea that you should look at the Pleistocene and the Recent. The Pleistocene was in a fervor then because people were just starting to take long cores. Hans Pettersson, Fritz Koczy and others were taking the first long marine cores. Cesare made arrangements to get those core samples, and his work ended up showing that there were 36 glacial periods in the late-Pliocene and Pleistocene. And I think that, more than anything else, that work established isotope geochemistry and really set it off.

Stanley Miller came to the lab somewhat later. He had heard Urey lecture on Oparin's ideas about life originating in a reducing atmosphere early in the earth's history. Stanley wanted to do an experimental thesis on sparking a reducing atmosphere to see if he could make organic compounds. And Urey was very reluctant about this being a good thesis problem. He said it was too risky for a thesis: you might waste two years and get nothing. So Stanley went away and thought about it, and he came back and said "Let me try it for six months, and if I get nothing I'll stop and do something else". Urey finally agreed to this, and of course Stanley produced amino acids almost immediately in his very simple system. The Chicago Chemistry Department was very skeptical about this, and they became believers only when Stanley repeated his experiments after autoclaving his entire system for a very long time to slaughter any bacteria that might initially have been present. This was a revolutionary experiment that began a whole new field of science, and Urey was very happy that he had finally allowed Stan to do it. Urey had been uncharacteristally cautious about this, and I think he learned something from the experience. He was certainly less cautious in his later years, even somewhat reckless with some of his ideas.

NCS: What about some of the work with lead isotopes and potassium-argon being done at that time?

HC: That began at Nier's lab in Minnesota. Two great things happened to originate isotope geochemistry in 1947. These were Nier's mass spectrometer and Urey's calculations. Urey's calculations actually were first published in 1935, by Urey and Greif. They were primitive and only covered a few species. But they showed that isotope fractionation should occur in nature and in the laboratory. And then, after the war was over the Institute for Nuclear Studies was formed at Chicago, with Urey, Fermi, Szilard, Teller, Libby and all these great guys. Urey wrote his classic paper of 1947, in which he showed that not only would isotope fractionation take place in chemical reactions, but also in phase changes; for example vaporization of water. He showed that carbon in graphite would be different from other carbon. So these were the two main events in 1947. But in 1947 I was just back from the Navy and had no idea about any of this stuff going on. And, of course, Cesare Emiliani didn't know the first thing about mass spectrometers or isotopes, either.

Urey got very excited that he had calculated the temperature coefficient for carbonate-water isotope exchange. He said, "Suddenly I have a geological thermometer in my hands." And typically, Urey, when he went into something got tremendously excited and passionate about it. He got money from ONR to support his work. They were the first people to support this kind of research, because there was no NSF. ONR was supporting it mainly because Roger Revelle had been head of ONR during the war, and one of the things he had persuaded them to do was to fund research in universities. They were justifying it on the grounds that they were supporting new developments in modern technologies, such as mass spectrometry, and it had something to do with the ocean, even though it was the Cretaceous ocean.

At that time Nier had also done some work on carbon isotope ratio variations. Of course, he also covered the periodic table. If you want to read about Nier's history, read my presentation to him of the 1984 Goldschmidt Medal (see GCA 49, 1661-1665). It's a very funny one, because I talk about Buffon in Paris estimating the age of the earth by cooling down iron balls. He'd heat them to high temperatures and let them cool down to the temperature at which young women could hold them in their hands without being burned. And I said in this speech that all the lovely young ladies of Paris flocked to hold Buffon's balls in their hands, which brought tremendous and raucous laughter from Hugh Taylor.

Nier also made the first precise measurements on lead isotopes, which led Holmes and Houtermans to calculate the first realistic age of the Earth. And that led to Clair Patterson's ultra-precise lead measurements in meteorites. So Nier was doing all that, and it was a hot topic. And when Urey wanted to measure paleotemperatures he went to Nier, and Nier gave him the designs for the 1947 mass spectrometer, which revolutionized geochemistry and started isotope geochemistry. We had a lot of help from Nier in those days, and he was very generous.

Jerry Wasserburg and Irving Friedman also came over to Urey's lab. Friedman came to work on hydrogen isotopes. He got the plans for a deuterium machine from Nier, and he measured deuterium in ocean water. Sam Epstein was measuring ^{18}O in ocean waters. And Jerry worked on a split thesis between Mark Inghram, who had been a student of Nier's and was now at Chicago in Physics, and Urey. He wanted to do potassium-argon dating. The mass spectrometry was done with Inghram at Argonne Laboratory, and the laboratory work and extraction of the argon from the samples was done in Urey's lab. That was the group. There were a few others later, including Al Tudge, who did some nice work on ^{18}O in phosphates, developing the technique.

By that time, of course, Urey had gotten interested in the moon. By the time I finished my thesis on carbon, paleotemperatures were going well under Sam Epstein. There was no need for Urey to be involved, and he got on to the moon and he and I did the meteorite paper then because he was interested in meteorites. And then he turned his attention entirely to the moon and the meteorites and cosmochemistry.

NCS: Can you say a few words about working in the lab then at the University of Chicago, in terms of, say, getting a good vacuum in your vacuum line or making a measurement, compared to what you can do now? Was a major effort required to get the data?

HC: It would have been a major effort, but we had a major facility, and that was Sam Epstein. Sam had worked in Canada on rare gases from fission. He had done high-vacuum work with rare gases, which were the most difficult experiments you could do, requiring great care and precision. So the high-vacuum work wasn't very difficult once we learned glass-blowing. Of course, we were all young and eager in those days and Sam was a superb teacher and a great experimentalist.

A problem with the paleotemperature work was that Urey wanted to make a mechanical automatic system. And he had this poor chemistry Postdoc working on a system in which you ground up the carbonate from the fossil and sprinkled it onto Scotch tape. The Scotch tape would be drawn by rollers into a vacuum system

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containing phosphoric acid. And as it went through, it would react with the phosphoric acid and the CO2 would come out continuously to be measured in the mass spectrometer. And this poor guy spent months testing different kinds of Scotch tape, because the phosphoric acid would eat up the Scotch tape. It was a terrible problem for a Postdoc to work on, to produce knowledge of what type of Scotch tape would hold up in phosphoric acid. So he quit and went to work for the CIA. He was a guy who loved solving puzzles and he had a great career with the CIA.

I don't remember any major technical problems. The mass spectrometers worked very well because Nier had designed them well, and McKinney, Sam, and Urey improved them. The vacuum systems worked. The only problem we had was when Jerry Wasserburg blew up a big flask and damn near killed the glass-blower. Glass-blowers are very temperamental and if you do something they don't like, they don't want to work for you anymore. Jerry rinsed a five-liter flask with acetone and then gave it to this guy to blow onto his system. And, of course, the minute the torch got near it, the damn thing blew up and he was lucky he didn't get killed. Wasserburg was forced to learn glass-blowing, which he hadn't done up until then, because after that time this guy refused to blow any glass that was given to him by Jerry Wasserburg.

Toshiko Mayeda was a great resource, too. She helped everybody and did everything superbly. But I don't remember any great technical problems. Sam Epstein was so good at keeping the lab running. Urey had nothing to do with the technical side of it after the mass spectrometers were built. He was out of all that by then, and Sam kept it all going. Of course, we had a lot of money in those days. We had two electronics technicians full-time, and a couple of other technicians. And we always had a group of four or five students or post-docs, as we had plenty of money.

On funding:

NCS: Do you have any comments on the issue of obtaining funds for your research – how that evolved with time and whether it is any better or worse now than ever before?

HC: It's a lot harder now, because there are so many more scientists and such a small amount of money per capita. I never had any trouble with grants until the last few years. But it is obvious that the funding agencies don't care much for funding old guys, and, with some correctness, they say that there's so little money that you've got to split it up among the young guys. The opposite side of the argument, of course, is that you should give it to the best science. If the young guy is doing good science, okay, but if the young guy is doing poor science and an old guy is doing good science…. And it is also a fact that there are a lot more reviewers who know very little; there's a lot of bad reviewing of proposals. It is a thing that has to happen when a lot of people come into the field. In geochemistry and earth sciences in general in the old days there was always enough money for the major people to get funding. But now it has become such a dog-eat-dog thing that there are a lot of nasty reviews by people who are underqualified.

The problem is that you can't counter these reviews. There's no easy way you can write back to NSF and say, "Please tell this reviewer this." They won't do that. They don't like to contact the reviewer and send back your comments as it requires too much personal interaction. So then you say, "Well, at least these program directors will know not to use this guy next time." But the problem is that by the time your next proposal goes in, there may be a new program director and he doesn't look back in the files and read this, so that's the kind of problems you get into, reviewers who don't know their business. I don't know how to beat that. What you need, of course, is really good program directors. You need program directors who can make up their own minds about things. The program directors hesitate to do this because they can be seen as autocratic and not paying attention to the panel. It's rare, but I've seen it done when they override a panel.

One of the best program directors I ever knew was John Snyder, in charge of Mantle Geochemistry and Petrology at NSF. He was willing at times to either override panels or to go to back to them with things that should be done. Neil Andersen was good in Chemical Oceanography for a long time. But then the time came that he could no longer support our helium work and said it had to go physical oceanography, because we were tracing ocean currents. For GEOSECS, Feenan Jennings was outstanding. You've got to have a program director who has enough vision to see what things are and then is willing to push for it. When Feenan ran ONR before GEOSECS, he funded the first two Clarke-type mass spectrometers for helium isotopes from a proposal that was written on the back of an envelope when we were in a bar. That's vision!

On the scientific method:

I've had one recent proposal rejected twice. It deals with dredging some newly-discovered seamounts in a big gap in the Austral islands at the point where the Austral fracture zone intersects the chain. It's a high-^{3}He hot spot, and it's a wonderful place to dredge, but Marine Geology in NSF Ocean Sciences has rejected it twice. The tenor of the reviews was that "Craig doesn't follow the scientific method. He doesn't lay out exactly what he expects to find from this and what it will mean." I wrote to the Program Director and said, "I've never used the scientific method in my life." I don't know any good scientist who ever worked with the "scientific method". A lot of times you work on something for completely wrong reasons. As the Dark Lady of the Sonnets said, "After all, the wrong road always leads somewhere." So many times it's happened in my own work that serendipity and just plain good luck intervened – you start up some problem with a completely wrong idea and it takes you to something even more interesting than if the original idea had been correct. Nature is bountiful with her rewards if you are willing to take a chance. This has happened to me so many times that I realize it's ridiculous to talk about the scientific method, which is what you learned from *My Weekly Reader* when you were a child.

For example, we started out looking for ^{4}He excesses in the deep ocean, because Revelle and Suess had said that radiogenic ^{4}He should be coupled with heat flow, and nobody understood why the heat flow looked the same in the continents and the oceans. Brian Clarke and I said, "Okay, let's look for this." You

couldn't measure ^{4}He very accurately, first of all. Secondly, the solubilities were not well known until Ray Weiss measured them. And thirdly, there were bubbles injected into the surface of the ocean, and we discovered that they carried down a significant amount of gases. So it was difficult to look at the absolute concentrations of helium. So we decided to look at the ^{3}He/^{4}He ratio, and we thought there would be excess ^{4}He near the ocean floor that would lower the ratio. What we found was a great excess of ^{3}He that turned into a whole new research field. As Urey always said, "If you go into a project, it's got to be a scientific problem that has rooms that continue into other rooms."

Urey always talked about tektites. He would give us this lecture in which he said the problem with tektites is that it's a closed room. He said, "You go into the tektite room, you analyze the tektites, you figure out their origin. What have you got? Then what do you do next?" It doesn't open doors into any other rooms. He always used that as an example, and I never forgot it. He said, "Choose scientific problems that have the probability of opening out onto other things."

The same thing happened over and over again with us. When Krishnaswami and Somayajulu and I worked on ^{210}Pb in the deep ocean, at that time it was thought that the residence time of lead in the deep sea was 5,000 years or even more. Clair Patterson was working on that, and Ed Goldberg and other people. They thought that the deep ocean was the sink where trace metals collected after they were scavenged from the surface ocean waters. This was important to Patterson because he was always arguing about lead pollution and how much lead would get into the deep water and stack up there and then come back into the surface water. We looked at ^{210}Pb because we were measuring ocean-bottom radon like the Lamont group, to get mixing rates in the bottom layer. Krishnaswami, who was a Postdoc with me then, suggested that ^{210}Pb would be even better, because it had a much longer half-life and we could see the profiles of excess ^{210}Pb that would diffuse up from of the sediment and we could see it mixing way up. But when Somayajulu measured it, we found it was only half the ^{210}Pb that should be there for equilibrium with ^{226}Ra.

This was very strange, because Goldberg had measured ^{210}Pb in the deep ocean and said it was exactly in equilibrium with radium. Well, he had the wrong radium data. He was using Fritz Koczy's radium profiles, which scattered all over the place, and he selected those that he thought were the right ones from the Pacific data, and the ^{210}Pb activity looked like the radium, so he assumed that ^{210}Pb was just simply in equilibrium with the radium and there was no use to do any more work. But we had our own ^{226}Ra data because Yu-Chia Chung was measuring radium in my lab, and so we found the ^{210}Pb anomaly.

We went into it with the idea of looking for excess ^{210}Pb at the bottom, and instead we found a large deficiency. I had previously done calculations using an advection-diffusion model for radiocarbon, which had radioactive decay and particulate interaction in it. I used the same equations to model ^{210}Pb, and we found that the deep-ocean residence time of lead was only 50 years! This led to a big battle with Clair Patterson, who couldn't believe that his residence time of 5,000 years could be lowered by a factor of a hundred. But then we looked at the copper data that Brewer and Spencer had measured on GEOSECS, and we found that the deep-sea residence time for copper was only 1,000 years, when people had said it was much longer. Peter Brewer found the same result. So it was clear that there was deep-ocean scavenging. But it was discovered for the wrong premise again. Pasteur said that "Chance favors only the prepared mind," and he was absolutely right. If you go into something and you have a prepared mind, which fortunately I acquired at the University of Chicago (if nothing else), then you suddenly see that you can find something new, and geochemistry is fantastic that way.

Wrestling a fish caught with the ALVIN at the edge of Loihi caldera, 1000 m deep. The species was named after the then director of the Scripps Institution of Oceanography -- "the biggest mouth in oceanography".

On field work in Africa:

NCS: During your expeditions to Africa, did you have any particularly adventurous times?

HC: We had some exciting times. As the Chinese say, "May you live in interesting times." We were lucky in being able to get

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to the African rift so much. A lot of it was due to oceanographic expeditions, especially when we were on GEOSECS, and when I did the two expeditions to Lake Tanganyika for the UNDP-FAO where we used the GEOSECS techniques. So we had a lot of chances to explore the rift, and we covered the Arabian–African Rifts all the way from north of the Dead Sea down to the end in southern Tanzania and Malawi.

But exciting times, yes. We got kidnapped on Lake Tanganyika by gunboats from Zaire which were old U.S. Mekong River gunboats. We had Ray Weiss and John Edmond onshore with tents and analytical equipment. And we had two boats, and we would move the boats on the lake and then we would move the camps. Val and I lived on the bigger boat, the Lady Alice, with two African Burundi boatmen who were wonderful guys. We lived on that boat with them all the time on the lake. The other boat was a smaller one that we used to transport all the shore-based equipment.

We were out in the northern part of the lake one night when the engine stopped running and we were drifting in circles, and it got dark, and we knew that was going to be bad. The fuel was always dirty. You couldn't get good fuel and we always had to clean up the engine. We had drifted over the central line of the lake, which was the boundary between Tanzania and Zaire, south of Burundi. And suddenly we looked up and this huge gunboat was standing way up above us, with guys shouting at us to heave to. They wanted to board us. So all these guys piled onto our boat, and they all had automatic rifles and they were pretty scary.

One of the reasons they nailed us was because we had a radar to find out where we were. We could range on the two coasts and we had a map of the lake, and we could find out where we were taking stations. And these guys immediately assumed we were doing radar surveys of the Zairian coast. This was at the time when the Zairian rebels came over and kidnapped Jane Goodall's students in Tanzania and took them to Zaire and held them for ransom. A lot of diplomatic work was going into getting them back, and the Zairian rebels were being very nasty at that time.

They said, "Do you have any cameras onboard?" And we all said, no, we didn't have any cameras. So then they searched our boat, the Lady Alice, and we had forgotten that Val had a camera stowed down in a cabin locker and they found it. So, of course, they assumed we were lying. Then they said, okay, we're going to tow you to Baraka peninsula on the northwest corner of Lake Tanganyika that juts out into the lake, where they had their base. So they put this short tow rope on, which couldn't have been more than 10 meters at the most, and they started towing us north. And, of course, their big boat would hit waves and slow down and then we would come up and almost hit it. And then it would speed up and then it would slow down again. So we were up all night working on keeping our boat from colliding with theirs. Fortunately we had a great boatman.

But it was a scary time because this was just after the massacres by the Tutsis of the Hutus. They had actually taken our UN boat, the Lady Alice, and gone out around the coastal swamps and hunted out the Hutus and massacred them. So they towed us north all night long. One guy aboard their boat had been at the Monterey Training School in the U.S., learning to use radar and being trained for the Zairian navy. He was asking me what I had done in the U.S. Navy, and I made the mistake of telling him I had been a radar officer. And so they decided that was the proof we were mapping the coast with radar.

Twenty-year-old Harmon Craig enjoying the view aboard the U.S.S. E-LSM-445 between radar simulation tests.

When they finally came into their base, they had two other boats join them, and they had all their flags flying and all their horns blowing. It was a great capture. They had captured an enemy ship, dragged us into this place, and they were going to intern us for God knows how long. And we were sitting in our boat looking up at their stern, and all you could see was these guys with big boots that they had gotten from surplus without bootlaces, and these automatic rifles pointed at us. We were busy filling our pockets. I was filling mine with cigarettes and toilet paper, because I knew that those were the two basic things I would need. And Val was stocking food in her pockets.

So they held us there for a day or two, and nobody in our camp knew where we were. We kept telling them we had a protocol signed with UN that we could operate this ship anywhere on the lake, and we showed it to the new guys, because the original guys couldn't read. So they finally got in touch with Kinshasa, and they were told to let us go. So they let us go one night and we worked all the way back to our camp. That was quite an experience.

The best thing I remember is John Edmond, who was on the Lady Alice with us. This was John Edmond at his best. None of us had passports. We had left them all with the shore party. But the minute the commander of the gunboat came on with his gun pointed at us and said, "All right, line up and identify yourselves," John stepped forward and said, "I'm not with these people. I'm separate from this group. I'm a British citizen, and here's my passport," which he had with him. He said, "I'm along for observation, but I have nothing to do with this group." So they waved

Edmond into one group, with his red U.K. passport, and the rest of us into another group, and we didn't know what was going to happen. I never forgot that. But it all worked out okay, and it just made an exciting trip.

Another time we got in trouble was when Val and I were mugged by the Masai. We were working from Kenya to Tanzania, and we drove down across the border and spotted an outcrop of a beautiful volcanic ash exposed near the road, a couple of hundred yards from the car. So I parked on the side of the road, we looked at it, and when we started walking back to the car, two Masai warriors jumped up. They're everywhere, and whenever you stop they rise up out of the bushes. And they have these big spears and machetes and henna-dyed hair all standing up. My God, they're awful looking guys. The old ones are great, but the young ones hang around in the towns and drink beer and rob people. Just like the U.S.

Anyway, we go to the car, and these guys are right there. And we're smiling and saying, "Jambo," and getting into the car. One guy put a spear against my chest and the other guy a machete, and they demanded money. So I pulled out some coins and handed them to the spear carrier, and he didn't want coins. Slammed my hand down and the coins went everywhere. So I opened my wallet and I had two 20 shilling Kenya notes, and I pulled them out and handed them over.

Now these guys had clearly never seen Kenya banknotes. They were Tanzanians and they had only seen Tanzanian notes. So they're sitting there looking at these Kenyan notes and looking at each other to see if they thought they were real money, and I in the meantime had managed to get the car key into my left hand and put it into the switch. I figured we've got to get away from these guys; if they don't like these notes, I've got nothing else to give them. And so while they were looking at these notes, I turned the key with my left hand and jammed on the gas with my foot. The door's still hanging open and the car starts lurching forward in first gear; just lurching, lurching, lurching. And I knew I didn't have enough time to shift into high gear, because it would slow the car down, and these guys were right behind us, yelling and waving spears. So I said to Val, "Can they run fast?" And she was very cool about the whole thing. She said, "I don't know, but one of them won the Olympics." So we didn't shift, we just kept going.

And gradually we pulled away from these guys, and we drove down to Arusha to our hotel. One of the Ministry officials came, and said, "Yes, that's a problem we have. These Masai are hanging around these points where people stop and robbing people." But he said we didn't need to worry, they would have done nothing. "You could have even refused to give them money and they would have done nothing but smile and let you go." So then he left and I went over to the African hotel manager, who was a very smart young guy, and said, "Was he right? What would have happened if we hadn't given them money?" He said, "They would have killed you." So we had two opinions.

We were there in the good times. It was not dangerous. Except for the one Masai incident we never had trouble with anybody. Val and I would camp in the Serengeti or anyplace we were and people would come and get around real close and put their noses against the van windows and look in at us. She'd go off in the bushes to use them for a bathroom and they would follow and

Collecting samples at La Perouse Pinnacles, in the French Frigate Shoals, westernmost Hawaiian Islands.

sit around her and watch her. They were that friendly and curious. There were places in Ethiopia we went into where they had never seen a white woman, and they would take her arms, poke her skin and look at the white skin.

We had problems in Tendaho in Ethiopia. They had to send army guys with guns with us. But they spent most of their time hunting gazelles. It was a little scary with all those Afar tribesmen with their big guns and their swords. But most of the time it turned out these guys don't have any bullets. They've got guns, but they don't have any bullets. They can beat you to death with them or they can cut your throat, but they can't shoot you. That was an exciting time and we used to watch at the hot springs we were sampling for gases, when they drove the herds of camels in to purge them by making them drink the hot spring waters. But in those days it was a paradise, it really was. You could go to Serengeti and not be mobbed by tourists. You could sit up on a high hill and watch thelines of wildebeests and zebras peel off from their rotating circles where the migration began, and move north like precision military maneuvers. You could see the geology all along the rift.

As so many have noted, Africa gets into your blood. Of all the places I have worked, more than China, more than Tibet, more than the volcanic islands (except perhaps Pitcairn), the two that remain foremost in my memory are Ethiopia and Lake Tanganyika. Ethiopia is sui generis: a majestic and awesome country with wonderful people and a fascinating history. There is simply no other country like it. And Lake Tanganyika is almost indescribable in its beauty and magnificence. Anchored out at night in the southern part of the lake, you look up at the high western cliffs of the Rift Valley and there are thunderstorms and bright flashes of lightning along the cliff tops illuminating the sky like a battlefield. And on the quiet lake below, the fishing boats are drifting in to shore, with lanterns in their bows and the sounds of the slow rhythmic cadence of the drums and the soft singing of

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the boatmen. In the daylight there are hippos wallowing along the shore, and water cobras slithering down the banks and swimming along the bottom. The trading boats move excitedly along the coasts, filled with people in their most colorful clothing carrying goods for barter and stopping at each little village along the waterway. It is another world, and the memories never leave you. These experiences have been a major part of the joy of science for me.

On unexplored terrains:

NCS: Do you think that the earth has been fully explored yet? Are there more things to discover?

HC: Oh, there will be more things to discover for a long time. In the gross sense, of course, it's fully explored. But people thought hydrothermal vents were only along the Mid-Ocean Ridge spreading axes, and we found them in Loihi and in the Mariana Trough. There are always new things to find. There's a lot of Africa that hasn't been explored yet. And there's a lot to do by drilling volcanic islands that are covered by younger post-erosional basalts. Trying to understand the interaction of the lower mantle and the upper mantle is seriously hampered by seeing only the tops of islands in hotspot and arc chains. In the Societies, you only get the lower mantle helium at Mehetia, which is the youngest island; it's gone when you get to Tahiti and Moorea and the older islands. In the Hawaiian islands, you get the high ^{3}He signal at Loihi and Kilauea in the shield lavas, and then as you get away from the hot spot, they're covered by post-erosional basalts. When you get out to all the volcanic islands at the end of the chain, to Necker and Nihoa, they have been eroded down enough so that you're seeing the shield volcanics again. But in most of the islands, like the Society Islands beyond Mehetia, you never see high ^{3}He or shield rocks after the youngest two or three in the chains.

There are important discoveries that are going to be made by drilling the islands, as Stolper and DePaolo are doing now on Hawaii. One of the big things for mantle geochemistry is going to be drilling through the post-erosional basalts and finding out what's down below. There's a lot more to do on volcanic islands. There are about 40 hot spots that are proposed (some people propose a lot more), and out of those there are 17 high-^{3}He sites, so we've been able to get the shield volcanics and see the high-^{3}He signals. We've done about 40 of the places that are proposed hot spots, but it's sparse sampling in many cases. We've got two or three samples from Fernando de Noronha and Trindade and places like that. In other places we have extensive coverage. You can't really tell much about whether these are going to have high-^{3}He or strange lead and neodymium ratios until you get detailed coverage. And that's difficult to do unless you're involved in oceanographic expeditions and can arrange stops at these islands.

In the old days there were many islands where you didn't have to get permission to land in advance. We landed in many places without permission, asking the chief if we could come ashore and they would welcome us. Nowadays, it's difficult. Your own university or the agencies won't let you do that. So it's much more formal now. Once we landed on Rimatara when they were having a spat with the French, and they welcomed us with a great ceremony, because they thought we had come to incorporate them into the U.S. They were very sad that we couldn't do that, and so were we.

But there are still lots of places. What particularly needs to be done is Africa, looking at the hot spot problem. We established there is a high-^{3}He hot spot in Ethiopia in the Afar and that's the only place along the entire African-Arabian rift that you find a high-^{3}He signature. But the big igneous eruptions that need to be looked at in Africa are in Hoggar and Tibesti and Darfur, the big swells across Africa, that may be part of this same plume. But Hoggar is in southern Algeria, Tibesti is on the border between Chad and Libya, and Darfur is in the western Sudan, and there aren't any places I know of more dangerous for an American geologist to go than these places. This is virgin territory, all of northern Africa, and the plume that the seismologists are mapping underneath Africa is a horrendously big thing. Africa has a very thick keel, as Dziewonski and his colleagues have shown,

Preparing to dine on fish caught during an ALVIN dive to hydrothermal vents on the East Pacific Rise.

Pursuing risky science a mile below the surface of the East Pacific, during an ALVIN dive to sample hydrothermal vents.

that goes down 1300 kilometers deep. This plume is coming up under the keel and splitting, and God knows where it's going. So most of Africa outside the Rift Valley is essentially unexplored, as far as looking for the surface expressions of a lower mantle plume injection.

The trouble is it's difficult to work in many areas. I still remember the early days working in northern Ethiopia. Giorgio Ferrara, my good friend from Pisa, and the Italians worked there a lot. They were there just before we were, and there was a Swiss team already there, and they went to their camp and saw these guys all lying in their sleeping bags. It was early in the morning, and they thought, "Well, we'll go and wake these guys up." And Ferrara said, "What a nice outfit. They have red sleeping bags." But the red was their blood. At night the tribesmen had come in and cut all their throats. That's not so dangerous anymore now, but to work in western Africa or the Sudan or Chad, I don't know how it's going to be possible. Our problem is that most geologists take what they call a hand-specimen. But for our work with noble gases you need to take 10 or 20 kilograms of lavas because you need several grams of olivine crystals. So you have to get your own samples.

Advice for the young geochemist:

NCS: Do you have any advice for young geochemists who are just starting out?

HC: Go for longevity, that's the best way to make it in this business. No, the best advice is what Harold Urey said – choose significant problems. Even if your ideas are wrong, if you are thinking of something that is not at all correct, if it is a significant problem, almost always, Urey would say, something good will turn up. And that will lead you to something else. Work on problems that open up new fields — the helium has been a beautiful example. Look at what Ken Farley's doing now, studying comets and the history of impacts, and helium coming down in interplanetary dust particles. Things grew out of this.

And secondly, you have to figure out how to write proposals that are good enough to get funded if you work on significant problems. A lot of times you may not know what they are going to lead to. I was surprised when they wouldn't fund dredging a whole new set of seamounts discovered in an island chain. You'd think that would be something that would be worthwhile simply from the viewpoint of exploration and finding out what is there, and from filling in gaps. It's hard, you can't read the minds of the reviewers and the panels they have. These are people whose minds are often opaque to me. It is often difficult to get exciting new work funded, if it's not in the preconceived notions of what we're looking at. And one has to find ways to do that.

Argue with NSF and communicate with them. I had this experience with Polar Programs when I first proposed to do helium and rare gases in the Greenland ice core. The reviewers said this was "risky science", because "What are you going to find?" I proposed that one major thing we might find was that ^{3}He might have increased greatly in the atmosphere during geomagnetic reversals. Atmospheric geophysicists thought that the atmosphere might gain a lot of ^{3}He during these times from the solar wind. And if the reversals were long enough, and it was thought

Performing shipboard titrations on the 1961 MONSOON expedition to the Antarctic Ocean.

that the null-field periods lasted about 5,000 years, you could get a lot of ^{3}He. If you could find the ^{3}He from geomagnetic events, reversals, and there were three events that we could look at in the cores, you would immediately have dated horizons that you could correlate between Antarctica and Greenland. It would be a gold mine for glaciology.

I also thought that there was a probability that you could see some radiogenic ^{4}He coming into the ice-sheet at the bottom. And I said we would do other rare gases. We didn't know about gravitational separation in the firn at the time, but we wanted to do all the rare gases. But the reviewers didn't agree with the maxim that if you do a good problem you may find something else. So this was rejected at first.

I wrote a five-page defense of "risky science" and attached it to a resubmission of the proposal. I said, "My god, you're spending millions on GISP and you can't afford $150,000 on risky science?" The ^{210}Pb work we did on GEOSECS was possible because we had established a very good thing at the beginning of GEOSECS. Every chief scientist, on his legs of the expeditions, was allowed to have his own program funded by NSF, and GEOSECS couldn't tell him that it was too risky. We had just discovered the ^{210}Pb anomaly on a GEOSECS trial cruise, and so we took as our risky program the ^{210}Pb problem. So we were able to make profiles all through the Pacific and the Atlantic because I was Chief Scientist on a lot of GEOSECS legs. I thought the same

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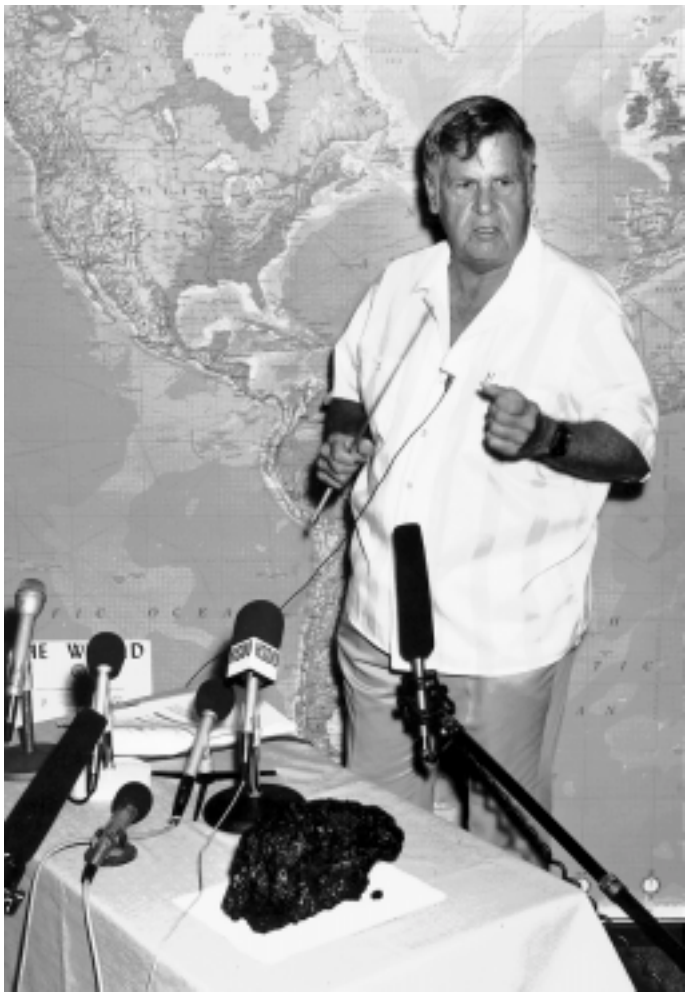
thing should be done with GISP. There should be some risky science built in. What does it cost you for 10 percent? It's Pascal's wager, in a way; taking the confession and accepting the Catholic religion when you die. You don't risk anything. What are you going to lose? But if there is a God and a heaven and you want to get up there, the reward is going to be infinite relative to the risk. And this persuaded them, so they funded it.

Then, of course, it was a little embarrassing that there was no ^{3}He excess in the GISP core. The null periods must have been so short that it didn't get up to a high level. Whatever, there was nothing. But what we did find was that in the bottom of the core there is a huge ^{4}He profile coming in from the bedrock, and it is not a smooth profile. It is broken with big cusps at three layers, which must be due to horizontal movement. And, in fact, these cusps match diffusion curves at depths where helium has been injected with some ice that must have been resting on bedrock elsewhere, loaded with helium, and the diffusion time is about 20,000 years. You can tell a whole lot about the movement of the ice. Now, it's obvious that this is going to be very important for the West Antarctic ice sheet studies. Everybody is afraid it's going to jump off into the ocean and flood Miami, and there's going to be no more University of Miami.

So this was the opposite of the ^{3}He in the ocean. We went to the ocean to look for ^{4}He excess — nothing, but we found excess helium-3. Here, we went to look for a ^{3}He excess — nothing, but there was a huge ^{4}He excess. It was exactly the opposite, but it was the same result of serendipity and choosing a good problem. It opened up into something more. And that, of course, is very exciting.

So, getting back to the question, one thing I would advise young geochemists: whenever you have anything that looks risky, don't just hope that people will not say it is too risky and that they'll think it is a good idea, say, "I know this is risky science but we have the feeling that there should be some risky science in all these programs, and the returns from this would be so great that they justify a small investment of money". Lay it all out to begin with. Don't be afraid to write the NSF Program Manager up front, too. That, I think, is the best advice I can give anybody. Incidentally, the new SGRE funding program in NSF is beautifully set up for risky science proposals.

The final advice I could give is that if you really want to enjoy geochemistry, do field work along with sample analysis. This is not true for everybody, obviously – the guys working on the moon can't go there, or to Mars. But the greatest enjoyment to me has been being able to go in the field and collect samples and do the geology, and then bring them home and analyze them. We have a rule in my lab that we (generally) don't analyze samples that other people collected, and for the most part we've stuck to this, and that's made science very enjoyable. I still remember Pat Hurley, one of the great men of our profession, telling me in the days when I was a student and he was working in a new tunnel drilled through the Rockies in Colorado, "You've never seen fresh granite if you can't hold a piece of that granite and it cuts your hand so you're bleeding." That was a very prescient statement for me.



Presenting results from the HELIOS expedition, that involved shipboard sampling of lavas and gases over an erupting undersea volcano (Macdonald Seamount).

The Birth of *"Reviews in Mineralogy and Geochemistry"*

Nearly every researcher in the earth sciences has heard of "Reviews in Mineralogy" (or RiM, as it is affectionately known), the enormously successful series of books produced by Paul Ribbe and the Mineralogical Society of America since 1974. With nearly 40 volumes published and well over 100,000 books sold, RiM is one of the great success stories among all society-produced book series.

We are delighted to report that starting next year, the Geochemical Society and the Mineralogical Society of America have announced the beginning of a joint venture in the form of a new book series entitled "Reviews in Mineralogy and Geochemistry", or RiM&G. This means that as of the year 2000, RiM will cease to exist as such, to be replaced by RiM&G. Both Societies feel that this will make a great series even better, one that will exist in a dynamic and growing state well into the future, and one that will better serve our closely related societies and science at large.

Under the merger agreement, RiM&G will be jointly owned and operated by both MSA and GS, although MSA will be fully financially responsible for volumes contributed to the series by them, and GS will be fully financially responsible for volumes they contribute. MSA will distribute each volume of RiM&G, including those volumes contributed by the GS, to all libraries around the world that receive American Mineralogist. (American Mineralogist is currently sent to more libraries than GCA. It is possible to add libraries to the distribution list if the GS so wishes, that is libraries that receive GCA but not American Mineralogist). Individual sales of RiM&G volumes will be made by both Societies at meetings and through orders placed by mail. MSA is responsible for obtaining and continued maintenance of the series records, copyrights, ISSN and ISBN numbers, "books in print" notification, and archival materials for all volumes in the RiM&G series. The RiM&G contract between GS and MSA is subject to change, as adjustments are needed in the future, with the approval of both the GS Board and MSA Council.

The Executive Editor of the GS portion of RiM&G will be Prof. Scott Wood, Special Publications Editor of the GS at the University of Idaho. The Managing Editors of RiM&G will be Prof. Paul Ribbe of Virginia Tech and Dr. Jodi Junta Rosso in Richland, WA. Both managing editors, experts in desktop publishing, are anxious to bring new innovations to RiM&G in terms of production and amenities. The goal of RiM&G will be the same as it has always been with RiM, that is to produce the highest quality scientific volumes at the lowest possible price. We are looking for ideas and scientific editors for future RiM&G volumes. Contact Scott Wood if you are interested (swood@iron.mines.uidaho.edu).

Michael F. Hochella, Jr.
Vice President of the Geochemical Society

From the Coordinator of Internet Resources

Web Site Facelift

Our web site is showing its age! With the help of Lee Mobley in the Business Office we're bringing the site up to date with a new look and new information. New pages for the membership directory, the V.M. Goldschmidt Conference, The Geochemical News, and Geochimica et Cosmochimica Acta are already online. Watch the web site for an improved home page and announcements for new features. The old "extended" home page will be removed soon.

The browser battle continues. Most of our membership services are compatible with older browsers, but everyone is encouraged to upgrade to at least version 4.0 of Netscape Navigator or Internet Explorer. With still newer versions either already available or scheduled for release soon, new additions to the site may be browser-challenged if you are still using those older browsers.

Thermodynamic Database

Oops! I really am very sorry. The searchable thermodynamic database is on-line as announced in the October issue of The Geochemical News, but the promised data sets are still under development. Some, but not all, users have been experiencing a database error. A new error page has been installed that will help track down this elusive bug. If you arrive at the error page, please take a few moments to fill in the brief form. And, most important of all, please contact me if you have electronic data sets that can be added to our database.

Mark Bloom, Coordinator of Internet Resources
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Conference on Stable Isotopes and Isotope Effects

June 20-25, 1999, Carry le Rouet, France

As the number of participants that can still be accommodated is limited, we urge those interested in attending, and presenting a poster communication, to communicate their intentions, as soon as possible, either to:

Mrs Laurence Boyer DRECAM Batiment 524, CEN Saclay F-91191 Gif-sur-Yvette, France; e-mail: boyer@drecam.cea.fr; Phone 33 (0)1 69 08 70 38; Fax 33 (0)1 63 08 22 89, or

Dr. Edgar Soulie, Secretary of the Organizing Committee, SCM, CEA Saclay, F-91191 Gif-sur-Yvette, France; e-mail: bsoulie@drecam.cea.fr; Phone 33 (0)1 69 08 47 37; Fax 33 (0)1 69 08 66 40.

American Chemical Society National Meeting
Anaheim, CA, March 24-25, 1999

The Role of Water in Organic Reactions

Interest in the special role of water as a solvent, catalyst, and reagent in organic reactions has grown enormously over the past decade. Nevertheless, this field of study is still new with a great deal remaining to be learned. A wide range of applications in the areas of fossil fuel refining, conversion, cleaning and upgrading are being explored. In the field or organic geochemistry, it is quickly becoming clear that an understanding of the role of water on controlling the molecular thermal evolution of kerogen, hence the timing of oil and gas generation and destruction, is crucial. In industrial applications, the potential use of water as an environmentally benign solvent and reagent is promising. The symposium on the Role of Water in Organic Reactions brings together a wide range of international chemists and geochemists to present papers bearing on this exciting and emerging subject. Papers covering both experimental and theoretical aspects, with either fundamental or applied scope, are included in the program.

1. Overview of the Reactivity of Organics in Superheated Water: Geochemical and Technology Implications
   Siskin, Michael and Katritzky, Alan R.
2. Structure-reactivity Relationships for Organics in Sub- and Super-Critical Aqueous Media: Development of a Knowledge Base
   Katritzky, Alan, Siskin, Michael, and Nichols, Daniel
3. Quantum Molecular Modeling of Reactions in Water: A Dielectric Continuum Approach
   Truong, Thanh N. and Eyring, Henry
4. Transitions in the Solvation Structure about Ions in Supercritical Water and Their Effects on Reactivity
   Fulton, J. L., Hoffmann, M. M., and Darab, J. G.
5. Hydrothermal Decarboxylation of RCOOH Studied in Real-Time by Vibrational Spectroscopy
   Brill, T. B., Belsky, A. J., and Maiella, P. G.
6. Reactivity of Monocyclic Aromatic Compounds under Hydrothermal Conditions
   McCollom, T. M. and Seewald, J. S.
7. High Pressure Hydrothermal Chemistry of Citric Acid and Related Acids
   Cody, George D., Blank, Jenifer, Brandes, Jay, Hazen, Robert, and Yoder, Hatten
8. Reactions of Model Compounds of Phenol Resin in Sub- and Super-Critical Water
   Tagaya, H., Katoh, K., Karasu, M., Kadokawa, J., and Chiba, K.
9. Experimental Investigation of Organic Compound Stability Under Hydrothermal Conditions
   Schulte, M. D.
10. The Role of Water During Decomposition of Oil at Elevated Temperatures: Constraints from Redox Buffered Laboratory Experiments
   Seewald, Jeffrey S. and Saccocia, Peter
11. Release of Biomarkers from Sulfur-Rich Kerogens with Hydrous Pyrolysis
   Koopmans, Martin P., Sinninghe Damste, Jaap S., Lewan, Michael D.
12. Investigating the Constitution of Macromolecular Material in Meteorites using Hydrous Pyrolysis
   Sephton, M. A., Pillinger, C. T., and Gilmour, I.
13. The Investigation of Deuterium Exchange Kinetics in Supercritical and Subcritical D2O
   Yonker, Clement R. and Palmer, Bruce J.
14. Hydrogen Stable Isotope Ratios of Kerogen, Bitumen, Oil, and Water in Hydrous Pyrolysis
   Schimmelmann, A., Lewan, M. D., and Wintsch, R. P.
15. Base-Catalyzed Reactions in Near-Critical Water for Environmentally Benign Chemical Processing
   Glaser, R., Brown, J. S., Nolen, S. A., Liotta, C. L., and Eckert, C. A.
16. Abiotic Synthesis of Organic Compounds in Hydrothermal Systems: Dehydration Reactions by Smectite Catalysts
   O'Day, P. A., Williams, L. B., and Holloway, J. R.
17. Role of Water in Prebiotic Nitrogen Cycles
   Brandes, J. A., Boctor, N. Z., Cody, G. D., Hazen, R. M., and Yoder, H. S., Jr.
18. A Physical-Organic Chemist Looks at Hydrous Pyrolysis
   Larsen, John W.
19. Processes Leading to Increase of Alkyl Chain Lengths under Hydrous Pyrolysis Conditions
   Barth, Tanja and Hansen, Birthe, G.
20. Importance of the Polars as Source of Free Hydrocarbons and Interaction Medium with Water During Hydrous Pyrolysis of Woodford Shale
   Burkle, V., Michels, R., Langlois, E., Landais, P., and Brevart, O.
21. Competitive Chemistry Between the Hydrocarbons and Water in the Hydrogen Transfer Reactions During Artificial Maturation
   Michels, R., Burkle, V., Mansuy, L., Langlois, B., and Landais, P.
22. The Effect of Supercritical Water on Vitrinite Reflectance as Observed in Contact Metamorphism and Pyrolysis Experiments
   Barker, Charles E. and Lewan, M. D.
23. Thermodynamic Constraints on the Generation and Maturation of Petroleum in Sedimentary Basins
   Helgeson, H. C., Richard, L., McKenzie, W. F., and Norton, D. L.
24. Thermodynamics of Reactions Involving H2O and Hydrocarbon Radicals between 27 and 374 °C
   Lewan, Michael D.

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GeoSoilEnviroCARS

The University of Chicago

Argonne National Laboratory • Advanced Photon Source

GeoSoilEnviroCARS is a synchrotron-based, earth science research facility under development at the Advanced Photon Source, Argonne, IL. A variety of techniques will be made available to the general scientific community for state-of-the-art research on earth materials. These include but are not limited to:

- high pressure research with the laser-heated diamond anvil cell and multi-anvil press
- x-ray absorption fine structure spectroscopy
- x-ray fluorescence microprobe
- microtomography
- microcrystallography
- powder diffraction
- surface diffraction

APS Aerial View

Diffractometer (foreground) and x-ray fluorescence microprobe (background) in experimental station 13-ID-C on the undulator beamline at GeoSoilEnviroCARS

Although construction and commissioning activities are still in progress, we are initiating our user program in a limited fashion for three techniques that are currently operational: microprobe/microspectroscopy, microtomography and energy dispersive diamond anvil cell with laser heating. To apply for beamtime, visit our web site (http://cars.uchicago.edu, follow the GSECARS link) and complete the online application form. If you have any questions, particularly with regard to experimental feasibility, please contact Steve Sutton (sutton@cars.uchicago.edu) or Mark Rivers (rivers@cars.uchicago.edu)

GeoSoilEnviroCARS online at http://cars.uchicago.edu

Meetings Calendar

March 15-18, 1999: 30th Lunar and Planetary Science Conference, Houston, Tex., U.S.A. Abstract Deadline: January 8, 1999. Sponsor: Lunar and Planetary Institute (LPI) and the NASA Johnson Space Center. Contact: L. Simmons, Conference Administrator, LPI Publications and Program Services Department, 3600 Bay Area Boulevard, Houston, TX 77058-1113 USA. Tel: 1+281-486-2158; Fax: 1+281-486-2160; E-mail: simmons@lpi.jsc.nasa.gov

March 28-April 1, 1999: European Union of Geosciences 10th Biennial Meeting, Strasbourg, France. Abstract Deadline: November 1, 1998. Contact: EUG Office, EOST, 5 rue René Descartes, 67084 Strasbourg cedex, France. Tel.: 33 (0)3 88 45 01 91 or 33 (0)3 88 41 63 93; Fax: 33 (0)3 88 60 38 87; E-mail: eug@eost.u-strasbg.fr

April 15-17, 1999: Oceanic impacts - mechanisms and environmental perturbations, Bremerhaven, Germany. Sponsors: AWI and others. Abstract deadline: January 31, 1999. Contact: Dr. Rainer Gersonde, Alfred Wegener Institute, Postfach 120161, D-27515 Bremerhaven, Germany. Tel: +49-471-4831-203; Fax: +49-471-4831-149: E-mail: rgersonde@awi-bremerhaven.de; Dr. Alexander Deutsch, Institut fur Planetologie, Universitaet Muenster, Wilhelm-Klemm-Str. 10, D-48149
Muenster, Germany. Tel: +49-251-83-33484; Fax: +49-251-83-39083/36301; E-mail: deutsca@uni-muenster.de. Web sites: http://psri.open.ac.uk/esf (ESF-impact program) and http://www.awi-bremerhaven.de (AWI institute).

April 19-23, 1999: XXIVth General Assembly European Geophysical Society, The Hague, The Netherlands. Sponsors: EGS, ICT, IAEA. Contact: EGS Office, Max-Planck-str. 13, 37191 Katlenburg-Lindau, Germany. Tel: +49-5556-1440; Fax: +49-5556-4709; E-mail: egs@copernicus.org. Web Site: http://www.copernicus.org/EGS/

April 28-30, Short Course: Sampling and Analysis of Contaminated Land, Matlock, Derbyshire Peak District, UK. Contact: Sally Verkaik, Imperial College, Centre for Continuing Education, Exhibition Road, London, SW7 2AZ. Tel: +44 171 594 6882; Fax: +44 171 594 6883: E-mail: cpd@ic.ac.uk. Web site: http://www.ad.ic.ac.uk/cpd

May 26-28, 1999: Geological Association of Canada-Mineralogical Association of Canada Joint Annual Meeting, Sudbury, Ontario. Contact: P. Copper, Dept. of Earth Sciences, Laurentian University, Sudbury, Ontario P3E 2C6, Canada. Tel: 1+705- 675-1151, ext. 2267; Fax 1+705-675-4898; Email: gacmac99@nickel.laurentian.ca

May 31-June 4, 1999: AGU Spring Meeting, Boston, Mass., U.S.A. Sponsor: AGU. Contact: AGU Meeting Department, 2000 Florida Avenue, NW, Washington, DC USA. Tel: 1+201-462-6900; Fax: 1+202-328-0566; E-mail: meetings@kosmos.agu.org; Web Site: http://www.agu.org/meetings

June 20-25, 1999: Conference on Stable Isotopes and Isotope Effects, Carry le Rouet, France. Contacts: Mrs Laurence Boyer DRECAM Batiment 524, CEN Saclay F-91191 Gif- sur-Yvette France, e-mail 'boyer@drecam.cea.fr' Phone 33 (0)1 6908 7038, Fax 33 (0)1 6308 2289; or Dr. Edgar Soulie, secretary of the organizing committee, SCM, CEA Saclay, F-91191 Gif-sur-Yvette, Cedex, France. e-mail bsoulie@drecam.cea.fr' , Phone 33 (0)1 6908 47 37. Fax 33 (o)1 6908 6640

June 21-24, 1999: International Gemological Symposium, San Diego, California. Abstract (poster) deadline: October 1, 1998. Contact: Dona Dirlam, Gemological Institute of America, 5345 Armada Dr., Carlsbad, CA 92008 USA. Tel: 1+760-603-4154; Fax: 1+760-603-4256; E-mail: ddirlam@gia.edu

July 11-16, 1999: Meteoritical Society 62nd Annual Meeting, Johannesburg, South Africa. Contact: W. U. Reimold, Dept. of Geology, University of the Witwatersrand, Private Bag 3, P.O. Wits 2050, Johannesburg, South Africa. Tel: 27 11 716 2946; Fax: 27 11 339 1697 Email: 065wur@cosmos.wits.ac.za

July 19-30, 1999: IUGG '99: The 22nd General Asembly of the International Union of Geodesy and Geophysics, The University of Birmingham, UK, Abstract Deadline: January 15, 1999. Will include programs of the International Associations of Volcanology and Chemistry of the Earth's Interior (IAVCEI), Seismology and Physics of the Earth's Interior (IASPEI), Meteorology and Atmospheric Sciences (IAMAS), Physical Sciences of the Ocean (IAPSO), Geomagnetism and Aeronomy (IAGA), Hydrological Sciences (IAHS), and Geodesy (IAG).To receive the Second Circular (May/June, 1998), Contact: IUGG99, School of Earth Sciences, The University of Birmingham, Edgbaston, Birmingham BIS 2TT, UK; Fax: 44121414 4942; Email: IUGG99@bham.acMk.; Web Site: http://www.bham.ac.uk/IUGG99/

Aug. 4-13, 1999: XVIII International Union of Crystallography, Congress and General Assembly, Glasgow, Scotland, UK. Abstract Deadline: March 1, 1999. Sponsors: Glasgow Development Agency, International Union of Crystallography, University of Glasgow, others. Contact: G. Houston, Northern Networking Congress, Central Office, Bellway House, 813 South Street, Glasgow, G14 0BX, Scotland, UK. E-mail: crystal@glasconf.demon.co.uk; Web Site: http://www.chem.gla.ac.uk/iucr99/

Sept. 6-10 1999: 19th International Meeting on Organic Geochemistry, Istanbul-Turkey, Contact: Conference Chairman: Prof. M. Namik Yalcin (TUBITAK Marmara, Research Center, Gebze-Kocaeli, 41470 Turkey); Conference Secretariat: Mr. Cengiz Soylu (TPAO Research Center, Mustafa Kemal. Mah. 2. cad. No. 86, Esentepe-Ankara, Turkey. Tel: +90 (312) 2843490; Fax: +90(312)2843491; E-mail: ogc99@petrol.tpao.gov.tr); Web Site: http:// www.nemrut.mam.gov.tr

Sept. 11-16, 1999: European Research Conference The Deep Earth: Theory, Experiment and Observation, Acquafredda di Maratea, Italy. Contacts: John Brodholt (UCL, London, UK), George Helffrich (U. Bristol, Bristol, UK). To receive first circular, register at URL: http://slamdunk.geol.ucl.ac.uk/~brodholt/euroconference.html See http://www.edinet-gdp.com/villadelmare/vdm.htm for hotel information.

Sept. 12-15, 1999: Third International Workshop on Orogenic Lherzolites and Mantle Processes, Pavia, Italy. Web Site: http://www_crystal.unipv.it

September 21-25, 1999: 3rd International Symposium on Applied Isotope Geochemistry (AIG-3), Orléans, France. Abstract and registration deadline, April 30, 1999. Contacts: Jean-Pierre Girard, Phone: 33 (0)2 38 64 32 15, Fax: 33 (0)2 38 64 39 25; Marie-Odile Gérault, Phone: 33 (0)2 3864 3707; Fax: 33 (0)2 3864 3990, BRGM, 3, avenue Claude Guillemin, BP 6009, F-45060 Orléans Cedex 02, France, E-mail: aig3@brgm.fr.

September 26 - October 1, 1999: XIVth International Symposium on Environmental Biogeochemistry (ISEB). Earth System Interfacial Processes from the Molecular to the Global Scale. Huntsville, Ontario, Canada. Contact: Prof. Grant Ferris, Department of Geology, University of Toronto, 22 Russell Street, Toronto, Ontario, M5S 3B1 Canada.

Oct. 25-28, 1999: GSA Annual Meeting, Denver, CO USA, Contact: Becky Martin, GSA Meetings Department, Box 9140 Boulder, CO 80301-9140 USA. Tel: +1-303-447-2020, ext. 164; Fax: +1-303-447-1133.

November 7-9, 1999: First Latin American Workshop on Reservoir and Production Geochemistry, La Habana, Cuba. Sponsor: The Latin American Association of Organic Geochemistry (ALAGO). Abstract deadline: April 30,1999 Contact: Dr. Jose Orlando Lopez Quintero, Centro de Investigaciones del Petroleo, Washington 169, Cerro - CP 12000 La Habana, Cuba. Tel: +53-7-577309; Fax: +53-7-666021; E-mail: ceinpet@ceniai.inf.cu

Dec. 13-17, 1999: AGU Fall Meeting, San Francisco, Calif., U.S.A. Sponsor: AGU. Contact: AGU Meetings Department, 2000 Florida Avenue, NW, Washington, DC 20009 USA. Tel: +1-202-462-6900; Fax: +1-202-328-0566; E-mail: meetinginfo@kosmos.agu.org; Web Site: http://www.agu.org

May 30-June 3, 2000: AGU Spring Meeting, Washington, D.C., U.S.A. Sponsor: AGU. Contact: AGU Meetings Department, 2000 Florida Avenue, NW, Washington, DC 20009 USA. Tel: +1-202-462-6900; Fax: +1-202-328-0566; E-mail: meetings@kosmos.agu.org; Web Site: http://www.agu.org/meetings

July 16-22, 2000: ICAM 2000: 6th International Congress on Applied Mineralogy, Gottingen & Hannover, Germany. Sponsors: International Council for Applied Mineralogy, German Mineralogical Society, Commission for Applied Mineralogy, others. Abstract Deadline: September 1, 1999. Contact: ICAM 2000 Office, P.O. Box 510153, D-30631 Hannover, GERMANY. Tel: +49-511-643-2298; Fax: +49-511-643-3685; E-mail: ICAM2000@bgr.de; Web Site: http://www.bgr.de/ICAM2000

July 18-22, 2000: International Association of Volcanology and Chemistry of the Earth (IAVCEI) General Assembly 2000, Bandung, INDONESIA. Abstract Deadline: February 29, 2000. Sponsor: IAVCEI. Contact: Secretariat, Volcanological Survey of Indonesia, Jalan Diponegoro 57, Bandung 40122, INDONESIA. Tel: +1-62-22-772606; Fax: +1-62-22-702761; E-mail: iavcei@vsi.dpe.go.id; Web Site: http://www.vsi.dpe.go.id/iavcei.html

Aug. 6-17, 2000: 31st International Geological Congress, Rio de Janeiro, BRAZIL. Sponsors: International Union of Geological Sciences (IUGS), Brazilian Geological Society, The Brazilian Ministry of Mines and Energy, others. Abstract Deadline: September 1, 1999. Contact: Secretariat Bureau, 31st International Geological Congress. Av. Pasteur, 404, Anexo 31 IGC, Urca, Rio de Janeiro, RJ, CEP 22.290-240, Brazil. Tel: +1 55 21 295 5847; Fax: +1 55 21 295 8094; E-mail: 31igc@31igc.org.br

Sept. 3-8, 2000: Goldschmidt 2000, Oxford, UK. Sponsors: Geochemical Society, European Association for Geochemistry, The University of Oxford. Contact: P. Beattie, Cambridge Publications, Publications House, PO Box 27, Cambridge UK CB1 4GL. Tel: +44 -1223 -333438; Fax: +44- 1223-333438; E-mail: Gold2000@campublic.co.uk; Web Site: www.campublic.co.uk/science/conference/Gold2000/

Nov. 13-16, 2000: GSA Annual Meeting, Reno, NV USA. Contact: GSA Meetings, Box 9140, Boulder, Colo. 80301-9140. Tel: +1-303-447-2020, ext. 164; Fax: +1-303-447-1133; Web Site: http://www.geosociety.org/meetings/index.htm

Dec. 15-19, 2000: AGU Fall Meeting, San Francisco, Calif., U.S.A. Sponsor: AGU. Contact: AGU Meetings Department, 2000 Florida Avenue, NW, Washington, DC 20009 USA. Tel: +1-202-462-6900; Fax: +1-202-328-0566; E-mail: meetins@kosmos.agu.org; Web Site: http:/www.agu.org/meetings

Sept. 17 - 21, 2001: 7th International Conference on Paleoceanography (ICP7), Sapporo, Japan. Abstract Deadline: March 10, 2001 Co-Conveners: Hisatake Okada (Dept. of Earth and Planetary Sciences, Graduate School of Science, Hokkaido University, Sapporo, 060-0810, Japan. Phone: 81-11-706-3537. Fax: 81-11-746-0394. E-mail: oka@cosmos.sci.hokudai.ac.jp), Itaru Koizumi, and Tadamichi Oba

Nov. 5-8, 2001: GSA Annual Meeting, Boston, MA USA. Contact: GSA Meetings, Box 9140, Boulder, Colo. 80301-9140. Tel: +1-303-447-2020, ext. 164; Fax: +1-303-447-1133; WWW: http://www.geosociety.org/meetings/index.htm

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