The best service... behind every detail... hammer out as one may and to feel the great forces that are variety of advances in geology and petrology.

This is the first presentation of the Harry Hess Medal. Harry Hess was long Professor of Geology at Princeton. He is most renowned as a founding father of seafloor spreading but contributed significantly to a variety of advances in geology and petrology. Harry Hess died in 1969 after seeing man land on the moon, an event of interest to him as chair of the Space Science Board.

It is an honor that I, a geophysicist, am asked to present the Hess Medal, named for a geologist, to Gerald Wasserburg, a geochemist. I hope I was asked because we share a spirit expressed by Oliver Wendell Holmes, Jr.: "The best service... to see so far as one may and to feel the great forces that are behind every detail... to hammer out as compact and solid a piece of work as one can, to try to make it first rate, and to leave it unadvertised." Both Hess and Wasserburg always showed great concern for the significant underlying causes, and both strove to direct not only their own science accordingly but also to urge earth and planetary science in general toward solution of broad important problems.

Hess and Wasserburg differed greatly, not only in primary scientific method—a field geologist and an experimental isotopist—but also in personal manner: one soft-spoken, the other stimulatingly assertive, but they were similar in their concern that others realize the potential of isotopic techniques required a lot of money and effort. The Apollo project provided the money but only because Wasserburg provided the effort. The results in print included only one paper on the programmable mass spectrometer but many papers on the isotopic character of moon rocks and Allende and other meteorites and what they meant. To Jerry, the spectrometer is clearly just a means to the end of understanding nature: a means that can consume much time and effort not evident in the final product.

Anyhow, after being mustered out in 1946, Jerry Wasserburg moved rather directly through undergraduate school at Rutgers [University] and graduate work at [the University of] Chicago to achieve a faculty appointment at Caltech in 1955. Since then, he has 370 publications on a diversity of topics, a record daunting to summarize. I shall take the escape of describing a half dozen papers, or series of papers, that have particularly influenced me.

The first is a paper in 1964, coauthored by MacDonald, Hoyle, and Fowler, that pointed out that the consistency of the earth’s heat flow with chondritic composition of radioactive materials must be a coincidence, since earth rocks differ from chondrites in their potassium: uranium ratio by a factor of an eighth. So the earth must be relatively enriched in refractories. This “Wasserburg” model has prevailed in evolutionary studies ever since, despite oscillations in opinion as to the relative contributions of radioactive and primordial heat.

The third is the great effort with Papanastassiou, Tera, and many others to establish the lunar chronology by several techniques: Rb/Sr, Pb/Pb, K/Ar, etc. This work demonstrated both Jerry's proficiency as a mathematical modeler and his readiness to go with a good graduate student on a problem removed from his own primary research interests.

The fourth is the long-term attempt, since the late 1950’s, to outline the grand chronology of solar system material, in collaboration with Schramm and several others. This attempt necessarily entails models of nucleosynthesis throughout the evolution of the galaxy. Probably the most conclusive achievement is the demonstration in 1969 of a formation interval of about 200 million years between cession of iodine and plutonium nucleosynthesis and retention of xenon in meteorites.

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The fifth is the bonanza of isotopic findings that developed when the Apollo-funded capable of being turned into a 670-km discontinuity, which is the most important property of originality. Here I fall back to another guru from outside science, when DePaolo and I, with Richard, Shimizu, and Allegre, presented the first Sm-Nd data on terrestrial processes of nature and advancing science. The present award symbolizes the integration of all approaches—including isotopic and petrochemical—that provide a fundamental view into the struc
ture and evolution of planetary bodies. The past decades have produced a series of scientific revolutions in which the centripetal force of discovery, analysis, and ideas has driven us together. We are using diverse methods to understand the earth and the planets. We have been progressing in terms of the scale and characteristically, the emphasis in publication has been on the implications as to evolution of the mantle. In particular, Wasserburg and DePaolo established that the depleted reservoir of the basaltic mantle must have been separated from the richer reservoirs of continental and ocean island basalt for more than 2000 million years. The enriched reservoir is somewhat larger than the depleted. The evidence suggests that the former is the lower mantle, while the latter is the upper, above the 670-km discontinuity, but there are several arrangements of pipes, valves, and tanks which could satisfy the data, and it will be some time before the fluid dynamicist’s models catch up with this challenge.

One of the rock suites examined in these studies of earth evolution is the Stillwater complex: that intricate layering with which geophysicists who think nature is indiscrimi
nating in its ways. A piece of Stillwater forms the base of the Hess medal, which is quite appropriate since Harry Hess also has been one of many to ponder this puzzle.

Some years ago I thought when I spoke of Jerry’s urging others to clarify these assertions that he could do more to make his papers less obscure. Perhaps, but striving for clarity may sometimes sacrifice the more important property of originality. Here I fall back on another guru from outside science, Marshall McLuhan: “Most clear writing is a sign that there is no exploration going on.” Clear prose indicates the absence of thought.” With Wasserburg, there is always new thought going on, so when he speaks or writes, one can never relax to familiar ideas but must be alert to the new and hence often difficult. Thus upon presenting this first Harry Hess medal, I am waiting, with some trepidation as well as anticipation, to hear what he has to say now.

William M. Kaula

Acceptance

Mr. President, ladies and gentlemen, friends. It is a particular pleasure to share the podium with some old friends, one of whom has just received the senior medal of our society. It is a great honor and privilege for me to do Sm-Nd on terrestrial rocks (just with funding to support his thesis) was reviewed with a comment that it might be of some interest for a year, but that it was inconceivable that such work would, of itself, be acceptable as a Ph.D. thesis, which would obviously be of doubtful general utility.

I recommend that to preserve our youth, we must keep trying to do the new or innovative things that are not easily accepted. For example, I am one of the few who has to go through a process that keeps me young—a rejection of a proposal to study Nd isotopes and REE [rare earth elements] in sea waters and, most recently, rejection of a paper on the subject by JGR [Journal of Geophysical Research] Oceans. It is just this sort of information a person needs. I then knew that what we were doing was not mundane; it was either stupid or else new and innovative. It is our own critical judgment that tells us what to do then. I have had to tell these others that we do not mean to criticize the way but rather to say that the road is always bumpy when you try to do something new and that you should hang in there and try to use the bumps for guidance. The real satisfaction is that I get to do it, to look over some old piece of work and conclude that it is not the mountain, but it is a very beautiful stone that I helped polish so that you can see some of its structure. On the rare occasion when I feel that way, I am very satisfied that I helped polish it.

In thinking over the scientific accomplishments in our whole field, I thought about other fields, which are rather specifically focused on problems relating to well-identified equations of motion or transport. Our field is different; it is directed toward nature, which is always full of newness. There have been a stream of advances over the past decades—some observational and some theoretical. We have been moving in big steps. No textbook written 20 years ago will work to guide our students today, and those written 10 years ago are far out of date in many fundamental considerations as to be of limited use. The reason that I get to do it, to look over some old piece of work and conclude that it is not the mountain, but it is a very beautiful stone that I helped polish so that you can see some of its structure. On the rare occasion when I feel that way, I am very satisfied that I helped polish it.

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Harry Hess and I were old acquaintances and friends. I respected him enormously. We both were denied admission to Princeton. I then went to Chicago, but Harry went to Princeton by mistake. As for Princeton, there were two applicants named Hess, and both were denied admission to Princeton. I and friends. I respected him enormously. We walked down to the Geophysical Lab [of the Carnegie Institution of Washington] and DTM [Department of Terrestrial Magnetism, also of Carnegie]. Whenever Harry came to California, he would visit with me at Caltech. We would sit outside of the Arena Building on the bench and talk—sometimes of science and sometimes about our philosophy about science. For purposes of argument, Harry would like to claim that all the equipment a geologist needed was a hammer and a microscope. I used to remind the Admiral that he also used the whole U.S. Carribbean fleet, and occasionally a submarine, as logistic support for field work. Other than fencing of this sort, we mostly talked about the seafloor and convection in the earth. He was strongly of the view that the upper part of the earth (in the oceanic section) ran like a tape recorder. The real question was the mechanism. I argued that if you couldn't identify and describe the mechanism, then it was not possible to legitimately interpret things that way. I learned a lot from this. When the plate tectonics revolution later occurred and then developed into a field (and even later became an iconography), I learned that the recognition of a general process or form in nature is of itself of great importance, even when the driving and operating mechanism is not fully understood. In complex systems, the use of phenomenological cartoons is often our most powerful guide. We are seeking to understand quantitatively and physically the true mechanisms but must recognize the patterns of phenomena and processes that guide our thought. Ab initio calculations for the evolution of the solar system give great insight, but they usually go off into other universes, not our own. I confess this, even though I love ab initio calculations. In all events the testing of ideas with good critical observations must be the rule. When Papanastassiou and I published our paper on high precision isotopic dating, we were seeking to interpret things that way. When Dick told me this, I knew that we had received a great compliment.

I thank Bill Kaula for his citation and his quotation from Oliver Holmes with implications about modesty. I have a general rule about scientists. I have never known a modest scientist. Scientists cannot be modest. How can a modest person, self-effacing, charge forward to investigate and try to solve a major mystery of nature, a mystery that extends beyond the individual, or the society, in time and space? It seems to me that the activity of science itself is inmodest. However, scientists can and should be humble. They must be humble in knowing that however hard they try, that they will undoubtedly fail in their own attempt to understand the universe and will only get a somewhat better glimpse of part of the real matrix of truth, if they are very smart and work hard. It is in the immodes- tly that allows us to try to understand and our humbleness that tells us to submit to nature, to its observation and its laws, and to still keep trying. I once heard a speech by a great unified theorist (GUT) who was reporting on his studies and concluded "that the universe failed to agree with his theory." To me, that represents a lack of both modesty and humbleness.

In closing, I would like to reminisce about some changes from the earlier days of the American Geophysical Union. This is the 24th Western Meeting of AGU. I was one of the organizers of the first Western AGU Meeting in 1961, at UCLA [University of California, Los Angeles]. To announce this, we used as a cover page the October 18, 1850, Friday, 2 o'clock issue of the San Francisco Alta, which announced the admission of California into the Union and also had peripheral reports about the use of inferior gold nuggets at the Monte tables, the arrival of mess pork, Havana cigars, and French wallpaper. The city hasn't really changed. Having a west coast meeting was a major break with tradition, as all meetings had previously been at Washington, D.C., the center of the formal scientific universe in this nation. The balanced growth of activities now has us regularly sharing meetings and now formal functions on both coasts. As a geologist and geophysicist, I am honored and delighted to have participated under these most special circumstances.

Thank you.

Gerald Wasserburg

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