Response on Receiving the Kemp Medal

Gerald J. Wasserburg

Recently Columbia University awarded Kemp medals for 'distinguished public service by a geologist' to Gerald J. Wasserburg, professor of geology and geophysics at the California Institute of Technology, and the late Paul Werner Gast, who was professor of geology at Columbia and chief of the Division of Planetary and Earth Sciences in NASA's Johnson Space Center in Houston. Professor Wasserburg's response at the presentation constitutes an appropriate tribute to the memory of Paul Gast, who at the time of his death was president of AGU's Section of Volcanology, Geochemistry, and Petrology. (Ed.)

IT is an honor for me to receive the James Furman Kemp Medal. I have a particular feeling of personal satisfaction because this award is shared with Paul W. Gast. When Paul and I heard of the award, we were both pleased. Since Paul was a Columbia man (both as a student and a professor) he knew about the Kemp medal, but I was puzzled; I just knew something about Kemp. When I received the message, the only thought that came to my mind was a book and a rock classification system that are still engraved on my memory from a sophomore course in petrography.

It seems that both pleasure and puzzlement are parts of any award. The recipient is pleased to get it, and surprised to find that he has so many friends, puzzled as to why he was chosen, and possibly puzzled about what it really means. Most of the interested world are just puzzled as to why the lucky fellow got the award at all. Columbia University and the Kemp committee have the obligation to explain things to the outside world, but I have to decipher my own private puzzlement. Part of the puzzlement about what a Kemp medalist is, forces one to think of two things: what it is that he has done as an individual, and what the Kemp medal stands for.

This has prompted me to think about the past several years and has brought to mind a kaleidoscope of conflicting experiences. There is no simple way for me to synthesize them, and I suppose the best way to say something meaningful to you is to tell you a little history and to

meander through considered prejudices and conclusions.

Most of my professional career has been spent as a university professor (a teacher and researcher with only very modest involvement either in consulting or in large-scale programs). I participated in some small studies of space missions and had an intent and a commitment to work on the lunar samples if and when the Apollo missions were successful. The view that they might be successful was held in some doubt or ignored by the majority of our profession until it 'really' happened. This peculiar viewpoint persisted well after it was an established national program that was being pursued with great vigor and large fiscal expenditures by both government and industry.

In 1967, Paul Gast and Johannes Geiss suggested that I serve as an advisor to the Science and Applications directorate at the Manned Spacecraft Center in Houston. Later, after Apollo 11, I was to even the score by convincing Paul that he had to go down to MSC and serve within NASA in a role of science management. This suggestion was followed up, and an official advisory group was brought together to serve only in the special area of 'lunar samples.' It was called the Lunar Sample Analysis Planning Team. In the acronymic argot of NASA we were spelled LSAPT and were called 'Less Apt.' Sometimes what you are called is more important than how you spell it. The total number of members was typical of committees, being around a dozen, although it has always been my opinion that there were exactly 13 members, enough for a coven.

At first the committee existed as an educational scheme (preschool) for some of the scientific community, to enable us to learn about what was happening, and also to provide some limited advice on what to do with the moon rocks after they were returned, and finally to add some scientific prestige to a small science-directed organization within a much larger structure that was dominated by flight systems engineers and managers. The committee was hard working and serious; they began to get educated in the complexities of the enormous undertaking and became deeply involved in working together and worrying about the problems that were exposed to them. In general, they were outsiders, but they still served as a useful forum and filter to equipment and procedural problems that were presented to them by MSC staff; they thus allowed MSC management to obtain independent evaluation of matters that required internal decisions.

Time went on, and as the incredible Apollo flight successes continued, LSAPT worked more intensively but with very little impact on the major and critical science decisions that were already made very early in the game by NASA. These decisions were often based on earlier scientific advice that was not updated and was for the most part received from people who were not knowledgeable in detail or who were not in continuing contact with the program. Some of this advice had originated from the National Academy of Sciences with no commitment on their part to follow through or reevaluate. Some of it originated from an internal high-level NASA committee that was unaware of the real questions and problems. Since the purpose of the Apollo program was to land men on the moon and have them return safely, it is not surprising that the role of science in the decision-making process was not larger. And, even within this hierarchy, since Apollo was a flight mission, flight experiments (e.g., flight hardware) deserved and got higher priority and attention than other important 'experiments.'

Somehow, in the heat of the missions, the enormous importance of the lunar samples and of lunar science became more evident, and

within the LSAPT membership a distinctive sensibility of broader responsibility developed that was controlled in part by a love for the lunar samples but was much broader and encompassed concepts of the overall lunar science program. Of this group there were the four horsemen, alphabetically, J. Arnold, P. Gast, R. Walker, and myself. Somewhere, in the leaky trailers that were our quarters, it was found that when we could identify matters of exquisite sensitivity and importance (distinct from questions of vested interest), it was possible to get the attention of the very highest-level administrators and to ultimately establish a dialogue with the management at intermediate and lower levels when it was needed.

This, then, helped to establish another committee, the Science Working Panel, which attempted to

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bring the various science disciplines together with the flight people and NASA headquarters, so that existing experiment plans would be improved and more complex and new experiments could be discussed and assigned priorities. It was there we often learned that the scientists were not always willing to take a broad overview when they were forced to operate under the severe time constraints of a mission in which every minute counts. We also found that serious and critical hardware responsibilities often lay in no man's land, with no good interface between science and engineering. One of the new experiments that we aided in getting on a mission failed to function because the principal investigator gave the very simple device to a subcontractor and did not himself verify that it really functioned—a screw was too long, so that a sliding gate did not close. This happened a little too

far from the local hardware store to get a fix.

Both before and after Apollo 11, when we staggered back to our universities, we often met colleagues who posed questions and criticisms with the phrase, 'Why don't they do thus and so?' My gracious response was, 'Who in the hell are "they"? Why don't you...?' This even occurred with some LSAPT members who later worked on lunar samples but quit as advisors because they did not want to work on 'NASA's' problems. This isolationist attitude on the part of some of the scientific community did not aid in establishing priorities, or cause or alter actions, and was due either to an intrinsic unwillingness to accept responsibility or because there was no vehicle that would enable them to become knowledgeable about the actual problems and aid in their solution.

In spite of the normal intense competition that characterizes scientists, a spirit developed that recognized the real issue, which was to do science with Apollo, and saw that the success of so enormous an enterprise could not and should not be associated with any one individual's name. The real experiments of Apollo are an extremely complex matrix of overlapping observations and concepts that have given us our first deep understanding of another planet. This has resulted in a new cosmopolitan scientific community that is deeply committed to lunar science and will undoubtedly grow into an even broader community of solar system scientists.

Well before the Apollo missions drew to a close, it was clear, in certain circles in the NASA administration and the scientific community, that the flight program and the science program were different. When the flight program was over, there still remained the great problems of Apollo Lunar Surface Experiment Package (ALSEP) data acquisition and evaluation, the study both of the reduced data and of the lunar samples, the synthesis of these data, and the preservation of these treasures for future generations. Although the National Aeronautics and Space Administration has in its charter a specific commitment to space science, it is also preeminently com-

mitted to flight programs. It was (and is) thus necessary to establish the importance of reaping the scientific fruits of the flight missions by supporting a prolonged, but not indefinite, phase of postmission study. This matter was addressed in a summer study that outlined the basis for such a postmission program (a document called the yellow peril).

We are now starting this postmission mode, and the concept is apparently accepted as a policy. There is, however, no such policy defined for postmission phase studies for Mariner Mars or for any of the other space exploration programs. Nor is the scientific community willing or ready to answer the difficult but legitimate question—When is it reasonable to terminate such studies?

One hard lesson must be learned, remembered, and put into practice: Adequate scientific management must be an intrinsic part of all stages in large-scale technical endeavors. This participation must begin with the definition of the mission itself and continue on an interactive, discursive basis into the mission and beyond to the final evaluation. The scientific community must be willing and able to participate in the planning and engineering as well as the decision-making functions. This will take a great deal of time on the part of the general scientific community in the universities and of the government or industrial sponsors or participants. To a large extent, the integrity of the university scientists will be dependent on the extent to which financial and moral support of the universities from nongovernmental sources can assure a basic independence.

However, along with the basic independence, the university scientists must not look upon the agency scientific establishment as simply the people who watch the technical details while they have the great thoughts. Good science requires that active capable scientific work go on within the agency involved. In balance, the agency management must, at all levels, honestly and openly solicit the scientific participation that I have described and must not consider the scientists as high priests who are called in at some magical moment to bless the venture. Further, the 'in

house' agency scientists must not consider themselves as totally privileged, with special access to the mission science, but should welcome participation from the outside scientists and recognize that they also have an obligation to support their colleagues at large.

In all this discussion I have tended to emphasize the problems that weighed on us, for the purpose of trying to identify for you critical problems in the interaction between the science community and major programs in government agencies. It is clearly my opinion that some of these problems may be addressed with good will and with appropriate management procedures in both communities. I have not emphasized the incredible successes of the Apollo missions and of Apollo science. The excellence of both are manifest to the world at large, and the vigor of Apollo science, which is really the beginning of a new planetary science, is displayed in the literature and the activity of first-rate scientists throughout the world. Some distinguished work in lunar science has, of course, been carried out through the participation of members of Columbia University, including their many contributions to the fundamental measurements of lunar heat flow and lunar seismic activity. Of course, many of the science management problems that I have referred to have been apparent on a smaller level to those members of the Lamont-Doherty Laboratory who have been doing planetary exploration using floating ships, not space ships.

Currently, there is much concern when we look at the immediate and distant future with the obvious requirements placed upon us by our natural resources, which range from sunlight to society. Some of the problems are clear in the realm of applied science, but they are not separable from 'pure' science, just as technology is not separable from science. These problems have been around in some fashion or another since Adam. There is an excellent article in the first issue (beginning on page 1) of *Economic Geology*, which was cofounded by J.F. Kemp in 1905. This article, by Fredrick Leslie Ransome, is titled 'The Present Standing of Applied Geology.' With

minor rewording and a scale factor appropriate to present technology, his comments apply fully to all that has been said tonight. They apply today to the problems of space exploration, choosing sound sites for nuclear reactors, ocean mining, or the search for earth resources.

Ransome stated, 'From the practical point of view, the installation of a modern plant which may mean an initial outlay of more than a million dollars is warranted only when the corresponding efforts have been made to ascertain the extent, structural relations and genesis of the ore body that is proposed to exploit.' He goes on, 'Economic Geology is preeminently an appropriate field for government activity. The cost of comprehensive studies and the proper presentation of the results is such that, if left to private enterprise, the investigations would necessarily be incomplete. The results of thorough economic studies are of recognized value to the community and that value must always largely depend upon a well-founded confidence in the integrity, impartiality and ability of the members of the scientific corps. . . . But that the official work of a single government bureau, however large and influential, should represent, so nearly to the point of exclusion, the activity of an important branch of science is best neither for the bureau nor for the science.'

From a personal point of view, a big change occurred in me and in Paul Gast. We went from small laboratories and a few students to face much larger and more complex problems. Some people have even suggested that I had my rough edges knocked off—only to expose a jagged stainless steel interior. This personality trait does not seem to characterize a Kemp medalist—and certainly not Kemp, who was described by a contemporary as being of 'brilliant wit, elegant form of expression and ready tact which made him especially suitable for many occasions.'

Both Paul and I found that our participation in the Apollo missions had major effects on our families. One day when I called home to let my wife Naomi know where I was, or was going, my son answered the phone. When I told him I was at some airport going someplace else he

said, 'Well, that figures.' Paul and I were in frequent communication by phone when we were not together. Since there were so many things to do, we would stretch the day by using the three-hour time difference between New Jersey and California. Paul would call me at home when he got to the office at Columbia. Being a gentleman, he would usually wait until I finished breakfast, although I didn't always get to shave. Being a night person, I would get even and stretch the day further by calling Paul at home after I returned from work. This frequent exchange of ideas and viewpoints was intimate and important and I shall miss it very much.

Paul's recent death has taken away a deep personal friend and a creative scientist whose intelligent and wise counsel are to some extent symbolized by this medal.

The real meaning of the Kemp award became clearer to me upon finding out about the previous recipients, all of whom were earth scientists or engineers with a serious involvement in advising and partici-

pating in major engineering enterprises and public works. The natures of the involvement and of the enterprises have changed since the time of men like Kemp, who straddled the nineteenth and twentieth centuries, but the sense of these functions has characterized all Kemp medalists. The first recipient of this medal was Charles Peter Berkey, who started as a junior associate of Professor Kemp's, then became his acknowledged successor, and ultimately translated Kemp's experience in engi-

neering works in the city of New York to the engineering geological problems of Hoover Dam. Such enormous contributions humble any things that I personally might have done, and I receive this medal as a symbol of the contributions of the community of dedicated persons who participated in the science planning and guidance of the Apollo missions for a decade and who successfully sought the rich scientific fruits of the greatest technical effort so far carried out by our society.

G.J. Wasserburg, professor of geology and geophysics at the California Institute of Technology, served on the Lunar Sample Analysis Planning Team from its formation prior to Apollo 11 through Apollo 14; he served as an advisor to NASA on several other committees. He obtained degrees in physics and geology at the University of Chicago, where he took his doctorate under M.G. Inghram III and H.C. Urey.

CORRECTION

An error has been discovered in the reported recovery figures for leg 24 in the abstract 'Igneous and Metamorphic Rocks Recovered by the Deep Sea Drilling Project, Legs 1-31' by W.B. Friesen (EOS, 54, 983-985, 1973).

A revision of Figure 7 showing the correct recoveries is reproduced below. The leg 24 recovery and the cumulative recovery in Table 1 and the leg 24 recovery in the histogram (Figure 1) should be adjusted to show 74.0 m. Changes in Figure 7 should also be made in the world map (Figure 2).