Letters

MURA: The Importance of Encouraging Scientific Enterprise

The fate that is apparently about to overtake the Midwest Universities Research Association (see *Science*, 31 Jan., p. 450) represents a deplorable deviation from a policy our government has hitherto followed with great success—the policy of encouraging and financing independent groups of scientists who have taken the initiative in scientific pioneering, rather than confining its support to its established institutions in the hope that these would foresee all possibilities and meet all needs.

This country's success in developing radar when it was needed resulted from this policy. The atomic bomb was developed under this policy. The citizen-scientists who foresaw and best understood the possibilities were given the responsibility to create new laboratories and to do things their way so that the best ideas in the country could be brought to focus on the problems. We backed these projects as the dean of a graduate school backs a competent research professor—the dean relies on his confidence in the professor's ability and is not inhibited by his own limited understanding of the research.

Before MURA there was no organization in the Midwest interested or competent in the advancement of high-energy physics. MURA was the result of a grass-roots movement by scientists in that area of the country; it now has a scientific team of great potentiality, and its accomplishments have dominated the development of accelerator science for 8 years. It has developed into a powerful combination of university staffs and scientists which is the custodian of detailed knowledge, techniques, and skills necessary to forge ahead to a new frontier in the physics of high-energy particles, namely, the generation and handling of very intense beams of high-energy particles.

The principle of backing the competent pioneers has not been applied to MURA. While others throughout the world have looked to MURA to carry a major share of the responsibility for the United States high-energy program, constituted authority has wished several times in past years that MURA would go away. The productivity and enthusiasm of the organization has enabled it to survive until the most recent federal budget. MURA's plans have been reviewed and revised to pieces, and the budget is being used to stop the enterprise. As a result the country may lose an energetic scientific enterprise and disperse a talented team. It will be a discouraging thing for enterprising scientists in the future and a dangerous thing for the progress of our country's science if this is allowed to happen.

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Mohole

I have read with considerable interest the series of articles on the Mohole Project (10, 17, and 24 Jan. 1964). The author, D. S. Greenberg, is to be congratulated on a fine accomplishment in the telling of this story. The task of trying to condense so complex a history into a few pages must have been immense and I am sure that many of the facts were difficult to come by. Perhaps I can supply a few explanatory facts which will straighten out certain points and help to fill out the picture.

1) On page 224, column one, reference is made to a "paper" of June 1961 from an erstwhile chairman of the AMSOC Committee to the National Academy, and it is stated that I "told a congressional committee last spring that the paper clearly supports the position that AMSOC intended an intermediate *program* to be carried out by an intermediate *ship*." Actually, I did not make such a statement. The letter in question seemed to me also a rather ambiguous communication which, as Greenberg aptly says, came "to mean all things to all partisans," and I simply told the congressional committee (last fall, not last spring) that in June 1961 "the AMSOC Committee had recommended an intermediate drilling program and had even included the prompt construction and operation of an intermediate vessel in their budget for the fiscal year 1962." This was simply a factual statement of a matter of record with no attempt at interpretation. I might agree with the view attributed to Dr. Haworth that this letter "called for an intermediate program but *not necessarily* [italics supplied] for an intermediate ship to carry it out."

The point is in any case immaterial since, regardless of the interpretation of this somewhat ambiguous letter of June 1961, the AMSOC Committee had early in 1962, before the June 1962 contract was signed by NSF with Brown & Root, transmitted to NSF its very clear and unanimous recommendation for both an intermediate program and an intermediate vessel. There could have been no doubt of AMSOC's views long before NSF was ready to sign the final contract. The AMSOC concept of the whole project should also have been clear to NSF, since it was presented to NSF several times early in 1962 and was later published in the July-August 1962 issue of *Geotimes*.

2) On page 334, column one, the statement is made that "AMSOC itself had developed a split on the issue of an intermediate versus an ultimate ship." This is not correct. As the Committee record shows, AMSOC voted repeatedly, unanimously, and without exception in favor of an intermediate vessel for the Mohole Project, and never voted in any other way. I do not doubt, however, that many attempts have been made to create the impression of such a split.

3) Page 334 might seem to imply an official conflict between AMSOC and Brown & Root. In my opinion, there has been no such conflict nor any basis for the existence of such a conflict. AMSOC's only official contact with Brown & Root was through NSF, and presumably Brown & Root as a contractor did only what it was told to do by NSF. AMSOC recommended to NSF, and if Brown & Root did not carry out AMSOC's recommendations

it was only because NSF did not ask them to do so.

4) Reference is made on page 334, column three, and page 335, column one, to a poll of the AMSOC Committee in August 1963 which resulted in a 12-to-5 vote, and this is perhaps interpreted as a split in AMSOC with respect to the intermediate vessel. This is not correct. The poll in question was not a poll on the desirability of an intermediate vessel for the Mohole Project. That had already been reaffirmed time and again by AMSOC. The poll of August 1963 was the result of my being told by Dr. Haworth that in his opinion funds could probably be committed at this time for only one vessel for the project and my being asked by him whether in such a hypothetical case AMSOC would prefer to get the intermediate-size vessel built now and take its chances on getting the ultimate vessel later, or to get the ultimate vessel built now and take its chances on getting the intermediate-size vessel later. Rather than attempt to speak uninstructed for the Committee, I preferred to poll their views for the benefit of Dr. Haworth. Surprisingly, perhaps, the great majority (12) felt strongly enough about the value of the intermediate vessel to vote in favor of going ahead with it, even if this meant no assurance of ever getting a Mohole vessel. Only a minority (5) preferred in such a possible contingency to go to the Mohole vessel first and leave the chances for the other vessel to the future. The division in voting was in no sense a division on the desirability of an intermediate vessel for the Mohole Project; it was merely a division on a hypothetical contingency raised by Dr. Haworth. Most members took occasion to deplore the idea of possibly having to make such a choice at all. So far as I know, the AMSOC voting record on the desirability of the intermediate-vessel approach to the Mohole Project has remained unanimous. Incidentally, the largest geological society in the country recently published a statement which, while expressly dissociating itself from any commitment to approbation of the Mohole idea, stated that, if the project is carried on, its research committee would favor the intermediate-stage-and-intermediate-vessel approach rather than any other.

Likewise, the quotation given on page 335, column one, from the AMSOC drilling panel may wrongly give the impression that it was a statement opposing the intermediate vessel. In fact, it is only a statement favoring a platform type of vessel for both intermediate and ultimate objectives of the Project. The AMSOC Committee's recommendations refer to an intermediate *vessel* without attempting to specify whether this should be a platform or a conventional hull.

5) Reference is made on page 335, column 3, and on page 336, to my testimony before congressional committees. I should like to make it clear that I appeared before these committees on 28 and 29 October only after receiving written requests from them. At the insistence of the president of the Academy, I had last June withdrawn a statement which had been prepared for public release to explain the position of the AMSOC Committee. Moreover, the director of the NSF had ignored my letter of 2 October trying to explain the basis for AMSOC's stand and requesting an opportunity to discuss the situation with him before he himself made final recommendations to Congress. It seemed essential in the public interest, as well as because of AMSOC's own public responsibility as originator of the Project, that in some manner the AMSOC viewpoint be clearly understood by all concerned. Also, under the circumstances it did not seem that the requests of the congressional committees could reasonably be denied, although, even so, I emphasized that I wished my remarks to be considered only as *personal* views.

6) Referring to page 335, column 3, since I have been frequently and rather sensationally misquoted by the popular press as wanting the Project "killed," I should have liked to see quoted in full the sentence in my testimony on which this seems to have been based:

> Personally, I would far rather see this project killed where it now stands than see it carried out in a manner not worthy of its potentialities or in any way which will not insure that the country gets its maximum money's worth in scientific and engineering achievement in return for the large expenditure which must necessarily be involved.

Terrible thing to say, isn't it? But who would like to go on record as opposing this view?

I might say that a major reason for the recommendation of the intermediate vessel has been to save the taxpayer's money while at the same time guaranteeing him a goodly return for a relatively modest expenditure. Ocean-drilling exploration is inevitably going to be a long-continuing operation for many years into the future. It should be planned carefully with a modest, orderly, and progressive approach to the more difficult aspects, and it should go no faster nor at any greater rate of spending than experience and achievements justify. There is no need for a wastefully expensive crash program when at least equally valuable results can be attained sooner by a more systematic procedure involving relatively modest annual expenditures which need go on no longer than results justify.

Finally, it seems to me that from between the lines of Greenberg's narrative two issues emerge: (i) whether it is in the national interest for the government agency created to assist science financially to be privileged to expropriate the scientific concept and guidance of a project merely because it is the immediate supplier of government funds for the project; and (ii) whether the scientists proposing a project of great public financial concern, who have been publicly charged with responsibility for its scientific aspects, should be free to express themselves on the objectives and conduct of the project, without censorship. Both are issues far more important than the Mohole Project itself.

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From afar I have followed your series on Mohole with interest. You presented a fair statement of the complicated events, with one important exception in part III (24 Jan., p. 334). In that piece you stated that I "had taken to public sniping," and you gave a so-called "quote." That is not so. The fact is that in several dozen speeches about Mohole I have meticulously avoided making any direct or implied statement about the difficulties of NSF or Brown & Root —before the contract was let, during our contract, and after our dismissal. To make such statements would be beneath my dignity, and that I have not done so can be substantiated by thousands of listeners.

At U.C.L.A. I spoke entirely about another pet project of mine: E-Quest (the search for the ancient equators of the earth). Although this would also involve deep-sea drilling, I made no mention whatsoever of Mohole. After the speech, a Department of

Geophysics staff man arranged for me to speak on the telephone to a reporter from the Los Angeles *Times* about E-Quest. I did so for about 15 minutes, as he took notes. The reporter repeatedly tried to draw me out on Mohole, and I refused. I kept saying, "Just look at the record and judge for yourself," and he kept saying, "When are they going to do something?"

At that time, after repeatedly having made it clear that I had no remarks on the subject for public record, I did say, "I don't know when it will get off the ground." On the following day when the story appeared I was exceedingly angry, called the *Times*, and emphatically said so. I may also have written them a letter about this irresponsible reporting. Their piece made little or no mention of the subject for which the interview had been arranged.

This does not constitute "sniping," and I wish you would print this letter or otherwise withdraw the remark.

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Unknowns in Entomology

In "Trends in scientific research" (*Science*, 17 Jan., p. 222), I find the following statement: ". . . the rate of discovery and description of new species has slackened." Information on total numbers of new species of animals described is not readily available, but data on the numbers of new genera are and should show a similar trend. In recent volumes of the *Zoological Record* I find: in 1945, new genera and subgenera 1619; 1950, new genera 1587; 1955, new genera 1963; and in 1959 (last on shelf), new genera 1863. Except during wars, there has been no slackening but only some fluctuation since long before 1900. A glance at any file of the *Zoological Record* will verify this fact.

In the tropics many groups of the smaller animals are almost unknown, and in such groups as Acarina even new families are being described from the United States. Recently in a short period I collected, reared, and attempted to get identification for all insects feeding on or associated with corn in Guatemala [*J. Econ. Ent.* **48**, 36 (1955)]. Qualified taxonomists in the respective groups were able to identify only about 80 percent of the species. Here within a few hundred kilometers of our borders, on one of our most important crops, about 20 percent of the insect species are completely unknown. On a less well-known plant the numbers of undescribed insect species would be much higher.

If the numbers of papers on taxonomic subjects has decreased proportionally to those on other aspects, it has not been because of lack of work remaining to be done. It is rather because of the lack of "glamor" of the work for many people and because of lack of financial support. For example, the staff of taxonomists in the Department of Entomology of the U.S. National Museum has actually decreased in numbers since 1932, whereas the amount of identification required has increased. This has resulted in marked decrease in research time. What is needed is not a disparagement of this type of work but rather an encouragement. Classification is basic to all other kinds of biology.

The immature stages of insects are still largely unstudied, in some whole families unknown. Certainly there remains much to be done in biology, in addition to that in molecular biology!

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How History Is Made

It is common practice among scientist authors to supply the readers of their monographs, textbooks, articles in handbooks, encyclopedias, dictionaries, and so forth, with bits of historical information. I have a strong suspicion, however, that the majority of such authors present names, dates, and technical data, associated with more or less memorable events in the development of the sciences, without having consulted original sources.

The collected information obtained from different scientist authors can be bewildering. Anyone who would care to look for himself to find out what they have to say about the origins of the law of Grotthuss and Draper should be able to verify that the following collection represents the situation pretty well.

One is told that this "first law of photochemistry" was proposed first by Grothus, Grotthus, Grothuss, or Grotthuss in 1817, 1818, 1819, or 1820 and rediscovered by Draper in 1839, 1840, 1841, 1842, 1843, or 1845. (That makes 96 different sets of four "data"!) One is informed, furthermore, that Grotthuss arrived at the absorption principle in question "in studying the fading of solutions of ferric chloride and other iron salts" or, alternatively, "on the basis of certain theoretical considerations." Again, "The 'law' was a simple phrase in his book *Abhandlungen über Elektrizität und Licht*." (If I am not much mistaken the book alluded to is that collection of some of Grotthuss's publications which appeared in 1906, 84 years after his death!) One can easily be misled to believe that Grotthuss collaborated with the boy Draper, when reading that the "law was formulated by these scientists when they noted that natural coloring matter bleached when exposed to light." Other scientist authors can tell, however, that Draper rediscovered the law "in the course of investigations on the photochemical combination of hydrogen and chloride," or, again, through experiments on daguerreotype plates.

The inclusion in books, say, of references to supposedly original sources of information is suggestive of reliability. Appearances are often deceitful, however. Thus, one can find authors who lead their readers to look in vain for Draper's formulation of the absorption principle in a paper which turns out to be a description of a photometer, and in vain for Grotthuss's explicit proposition in a paper which is but an excerpt of the relevant publication.

If the exposition of a deplorable situation is a prerequisite to its being improved, this letter may not be entirely worthless. The phenomenon commented on is by no means a new one, however. Scientist authors of today appear to be neither any worse nor any better than their predecessors. As much as 120 years ago, Draper, while accusing a fellow scientist of presenting historical misinformation, stated: "It is time that scientific men should set their faces against these things" [J. W. Draper, *Phil. Mag.* **25**, 49 (1844)]. Unfortunately, his words do not seem to have had a lasting effect.

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