Letters

"Brain Drain" Figures

The figures you give in your comment entitled "Brain Drain—The View from This Side of the Atlantic" (21 Feb., p. 787) can be misleading. These scientists and engineers are neither all British nor necessarily seeking U.S. citizenship. They are people whose last country of residence was Britain and who entered the United States on immigrant visas. Thus the figures you give include an unknown number of non-British subjects moving from Britain to the United States. Nor can entry on an immigrant visa be regarded as evidence of intent to seek U.S. citizenship. Many foreigners who enter the U.S. on immigrant visas have every intention of returning to their home countries.

While the British Government is concerned at the loss of British scientists by emigration, it should be recognized that many of the scientists included in the figures you quote will return to Britain within a few years.

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Mohole

As ranking minority member of the Senate Appropriations Subcommittee which passes on the National Science Foundation's requests for funds, I have taken a particularly keen interest in Project Mohole. I believe that I am probably more conversant with the subject than is any other member of Congress. From this vantage point, I would like to commend you, in general, for the series you have printed recently on the subject [News and Comment, 10, 17, and 24 Jan., 1964], and comment on some pertinent points.

Greenberg says that I have been "blasting" NSF since one of my constituents was passed by for the Mohole contract. If any one of the bidders could be considered a constituent of mine, it would be Brown & Root, because that company has a subsidiary corporation located in Colorado. My "blasting" of NSF has been based on the fact that I do not think Brown & Root was the best qualified of the bidders. Further, I feel that Brown & Root has not progressed at all well since the contract was let. I am concerned with the direction the Project has taken, the apparent loss of time in getting the Project under way, and, above all, the continued escalation in cost estimates.

A great deal of the testimony about Project Mohole before our subcommittee this last fall revolved around the question whether an intermediate ship had been contemplated for the project as originally conceived. While I am convinced that the intermediate step has been contemplated from the beginning, the more important question is whether the intermediate ship is now necessary or desirable. I believe that it is. Greenberg points out in his article that Haworth appears to agree, basically, with this position but thinks it is now too late to build the intermediate ship. Greenberg also mentions, just in passing, that the National Science Board convened a special study of the question. He did not say what the recommendations of the special committee were. Interestingly, I am informed that the committee is now planning to make no final written report. However, its "preliminary" report said that "the panel unanimously urges that an intermediate drilling vehicle be constructed promptly. . . ." And I would point out that the great majority of people knowledgeable in the subject have taken the same position. There is no question that, from the scientific viewpoint, the intermediate ship is desirable.

From a purely economic view of the project, I think the intermediate ship is worthwhile. Everyone involved agrees there must be a period of testing and experience-gaining before the ultimate hole is started. To carry out this work with the platform at an operating cost of roughly $8 or $9 million per year is folly. This is particularly true in view of the offers which have been made to the NSF to construct and operate an intermediate ship—offers which have been neither accepted nor rejected by NSF. Further, carrying out this phase of the project with the ultimate platform will involve a renegotiation of the contract with Brown & Root. Presumably this renegotiation will include a renegotiation upward of Brown & Root's $1.8 million fee. I might add that Brown & Root was the only bidder for the contract who asked for a fee. This was another factor in the selection of Brown & Root which I found disturbing.

I would like to set the record straight on one point: The Senate Appropriations Committee did not retreat from its position that Mohole funds should be withheld. A conference report on an appropriations bill is written by the managers on the part of the House. In this case, the chairman of the House subcommittee, Representative Albert Thomas (of Houston, Texas), was responsible for preparing the report. Chairman Thomas felt that Brown & Root (of Houston, Texas) was doing a fine job on Project Mohole, and this feeling, not surprisingly, was reflected in the conference report.

But the most disturbing factor to me in this whole project has been the attempt by NSF, at least until recently, to treat the entire subject as if there were no problems. It may be, as Greenberg intimates, the "traditional concern [of the scientist] for maintaining an appearance of dignity and keeping spats out of public view." But, as a United States Senator directly responsible for reporting to the Senate on the conduct of NSF and making recommendations for funding its activities, I resent the attitude, which I have seen displayed by some, that the Senate, or more generally the nonscientific community, must be kept in the dark if things are not completely harmonious in the household of science. I recognize that there may sometimes be a thin line between unwarranted intrusions on administrative decisions and legitimate concern for the wise management of government funds. But, occupying as I do a position of trust with regard

to the public funds, I feel entitled to candid and complete answers to my questions on the use of those funds. When I fail to receive such answers, I become suspicious.

This is a problem which transcends the Mohole question and is important to the whole scientific community in its relation to the government. While we who are responsible for appropriating money for research do not expect that every project funded by the government will be an unqualified success, we are entitled to have the facts so as to assure ourselves that funds are not being dissipated or mismanaged. This is the context within which I asked Hedberg and others to lay aside their reluctance to make the Mohole disagreement a matter of public record, and testify before our Appropriations Subcommittee. It is a problem on which I believe a large segment of the scientific community might well reexamine its thinking.

GORDON ALLOTT
Committee on Appropriations,
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Although the oil industry is well represented in Colorado, Senator Allott is correct when he points out that none of his constituents, outside of a Brown & Root subsidiary, were directly involved in the Mohole bidding.—D.S.G.

Rhythm Method

It is unfortunate that de Bethune mars his provocative article "Child spacing: the mathematical probabilities" (*1*) by several errors of fact or assumption. The author does an effective job of dramatizing how high a "monthly security factor" is necessary in order to achieve even a one-to-one chance of avoiding pregnancy for periods as long as 2 or 3 or 5 years. He could have made his case even more dramatic by pointing to the situation of many American wives who, marrying in their late teens or early 20's, have their desired 2, 3, or 4 children before they are 30 and then must prevent further pregnancies during a total risk period that may exceed 10 years.

His Eq. 2, which relates number of exposure months n to total months N of desired spacing, is somewhat unrealistic for neglecting the period of postpartum amenorrhea and anovulatory cycles that follow a childbirth. He credits the work of Tietze (*2*) as the source of Eq. 3, which furnishes a relationship between the monthly security factor q and coital frequency. However, he incorrectly imputes to Tietze the assumption that the fertility period occurs randomly during the cycle, whereas Tietze assumes that it is coitus that is randomly distributed over the cycle.

With regard to the rhythm method, de Bethune cites a sample of 5 couples, each with 7 to 11 children, who "have found that the rhythm method, as practiced by them, results at best in spacings of 1 to 2 years between births." No space is accorded the few estimates of rates of accidental pregnancy under rhythm for clinic or probability samples. Examples are the clinic study by C. Tietze, J. Rock, and S. R. Poliakoff (*3*), as well as rates published in the Princeton Fertility Study (*4*) or from the earlier Indianapolis Study (*5*). The use effectiveness of rhythm is not known precisely and perhaps never will be, since multiple forms of rhythm are in use and the motivation to practice it—or any method of contraception—effectively varies greatly depending on whether the couple are simply spacing a desired pregnancy or trying to prevent an unwanted one. The indications so far are that in average practice rhythm is less effective than such techniques as condom or diapraghm and jelly, but certainly it is nowhere near so ineffectual as implied by the author's sample of 5 couples.

Passing mention is given a theoretical analysis by Tietze and Potter (*6*) in which it is estimated that quite high monthly security factors are attainable by women of medium menstrual variability provided that they use the Knaus or, better, the more stringent Ogino rhythm formula consistently and correctly and base their calculations of unsafe days on a history of at least 13 previous cycles. De Bethune takes particular note of the fact that the theoretical efficiency of any calendar form of rhythm declines rapidly when reliance is placed on shorter and shorter records of past cycle lengths for purposes of calculating unsafe days. In this connection he states that "many couples who use the rhythm method cannot achieve 13 cycles of observation without encountering a pregnancy first." This is quite true, but he might have noted that a recent pamphlet, "The Safe Period," published by the Planned Parenthood Federation of America, includes special instructions aimed at this problem. The woman who has less than 8 cycles recorded is instructed to use make-believe cycles of 33 and 23 days, thereby insuring a wide unsafe period until she can accumulate a sufficient record of past cycles.

Perhaps his least cautious remark about rhythm comes late in the article when he asserts that rhythm users who desire a 2-year spacing "are limited, statistically, to two acts of coitus per cycle" and "couples who desire a 4-year spacing are limited to a maximum of one act of coitus per cycle." These calculations are based on the assumption of random coitus and thus have no direct pertinence to the rhythm method at all.

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Stating the Problem

I wish all papers in *Science* stated the problems that prompted the reported investigation in so clear a way as I. Rock and J. Victor stated theirs (7 Feb., p. 594).

Would it not be possible to request and even to rule that every report should begin with a clear statement of the problem that sparked off the reported research? This might have a beneficent side effect on the philosophers of science: it would suggest to them that scientific research does not begin with gathering data but with posing problems—and that, as a matter of fact, it consists in struggling with problems all the way.

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