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,
United States Senate

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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