than usual in speaking to any points he deemed faulty in the author's argument.

I do not recall an instance where our reviewers failed to meet the exceptional requirements imposed on them, despite (alas) the insubstantial foundations the authors had almost invariably elected to build on. The reviews were courteous and addressed directly and instructively to the author's primary assertions. While it turned out that our authors overturned no phlogiston theories in that 7-year period, I am reasonably confident that the editors had not missed any opportunities to do so, either.

Since we tried to limit the reviewing burden to about two per year per reviewer, our principal actual traffic with our most select reviewers was associated with the merciful extermination of hopefully conceived but hopelessly misconstructed theories and experiments. I believe they took pride in accepting the rather special moral and intellectual burdens we felt a conscientious profession owed the "crackpot."

I see no reason why a grant administrator should not respond to the unconventional proposal with some comparable shift in evaluation strategy. Indeed, is there any evidence that the good ones don't?

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The letter on majority rule by research-grant review committees ("Grants to nonconformers," 24 Jan., p. 309) indicates a lack of understanding of the review processes, at least of those of the Public Health Service. When two or more members of a study section dissent from the majority opinion regarding an application for a grant, a split vote is registered and the opinions of both the majority and the minority are noted. When the application comes before the National Council for its second review, it is presented as a special case. In a number of instances the National Council has reversed the decision of a study section or has returned an application to it for reconsideration on the basis of the minority opinion.

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Mohole Fanfare

The account in your issue of 10 January 1964 entitled "Mohole: the project that went awry" reads as though it were written by a press agent for "the oceanographic engineer who, to unanimous acclaim, carried out a preliminary phase that set a record for drilling at sea."

"Unanimous acclaim" is hardly accurate. The preliminary phase of Mohole merely proved that with minor modifications existing equipment could be used to lower drill pipe to bottom and to make a short penetration of the sea floor on a no-reentry basis. None of the major problems was solved by this stunt, which in all probability could have been accomplished by private enterprise in less time, with less expense, and with infinitely less fanfare.

Now that the Mohole planning is up against the hard realities of the project, it is inevitable that signs of strain should appear among the personnel who have so gaily committed themselves to this undertaking. It will take more than press releases and self-serving propaganda to effect the transition between a wine-breakfast inspiration and an extremely difficult if not virtually impossible engineering accomplishment. Surely there are better places in the broad field of scientific research in which this money can be spent. But if we must have a Mohole, we should reexamine the wisdom of choosing an oceanic rather than an on-shore drill site. And, in any case, a more restrained, realistic, and scientific tone to the project publicity would be a welcome improvement.

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Cigarettes: Testing on Mice

At a recent meeting of statisticians the point was repeatedly made that, while the data support the thesis that inhalation of cigarette smoke is positively correlated with pulmonary malignancy, the mechanism of the relationship is by no means established. In particular, it was stated that tars from cigarettes may induce tumors when painted on mouse skin but that no evidence of pulmonary malignancy has been found from inhalation of cigarette smoke. May I offer some comments on this.

As I understand the literature on carcinogenesis and on induction of mutant cell lines, the probability of inducing a viable, self-sustaining line of carcinogenic cells should be a function of the number of cells in mitosis at any given time, the amount of radiation to which these cells are exposed, the kind of radiation, and the duration of the trial. The number of cells in mitosis will be related to cell type and to the demand for cell reproduction. In the case of any local trauma, of which inhalation of cigarette smoke is an example, cell reproduction rates increase.

It is one thing to give cigarette smoke to a small animal, with small lung volume, in the absence of radiation (indoors, in shielded rooms and cages), for a few weeks or months. It is another thing for a human to inhale deeply, irritating most of the mucosal and epithelial lining of his large lung volume, while exposed to radiation from cosmic rays, potassium decay, and x-rays of various sources over a period of years. Multiply volume by incidence of radiation, by time, and by a probability constant, and one must obtain a population probability.

It is therefore suggested that if the inhalation of smoke by small animals be supplemented by radiation, to compress the time and volume factors, the causal relations between smoking and lung cancer might be clarified.

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More on the 1953 Fallout in Troy

Ralph Lapp suggested [*Science* **142**, 448 (1963)] that I "cite the pertinent statistics" to support my previous statement [*ibid.* **141**, 1109 (1963)] that there had been no increase in the incidence of cancer or leukemia over the past 10 years in the children of the Albany-Troy-Schenectady area of New York State. By law and regulation, physicians, hospitals, and pathologists are required to report all cases of cancer to the local health officer, who forwards copies to the New York State Department of Health, except in New York City, which maintains separate

Table 1. Cases of leukemia in children under 15 years of age reported in Albany, Rensselaer, and Schenectady counties, New York.

Year of birth	No. of cases	Cases by year reported													
		49	50	51	52	53	54	55	56	57	58	59	60	61	62
1943	4*			1						1					
1944	3†						1				1				
1945	6‡		1				1				1				
1946	2										1		1		
1947	3	2		1											
1948	6	1		1	1	1		1					1		
1949	4	1		1				1			.1				
1950	2											1	1		
1951	4						2			1			1		
1952	2										2				
1953	8						1		2	2			1	1	1
1954	5						1					3			1
1955	1									1					
1956	6									1		2	2	1	
1957	6											2	1	2	1
1958	6												1	3	2
1959	2													1	1
1960	1														1
1961	1													1	

* Including one case reported in 1944 and one in 1945. † One reported in 1946. ‡ Two reported in 1946, one in 1947.

records. All death certificates are regularly screened for diagnoses of cancer, whether as a primary cause of death or as an associated diagnosis. In a study made several years ago by the United States Public Health Service, the records of all hospitals in several counties were studied and all physicians in those areas were visited to determine the completeness of our cancer report files. Their findings indicated that approximately 85 percent of all known cases are reported.

In reference to the statistics on leukemia and thyroid malignancy in the tri-city area affected by the 1953 fallout, I quote from a personal communication from Edward Wieben, statistician of the Bureau of Cancer Control of the New York State Department of Health:

> In our attempts to prove any increased incidence, we have subjected the cases from the three counties (Albany, Rensselaer, and Schenectady) to several types of analysis. Using cases under 15 years of age reported from these counties, we have been able to locate only a single reported case of thyroid malignancy (classified as a lymphocytic lympho-sarcoma). The individual was 2 years of age in 1953 and was diagnosed in 1958.
>
> The leukemia cases reported from the same district were also reviewed. Comparing these cases both by year of report and by year of birth we find no significant excess of cases. There follows a tabulation by year of birth of the total cases reported through December 1962, and . . . . the year the case was reported. If we compare the cases born in 1953 with the cases born in 1956, and compensate for exposure time in years of survival, the 1956 cases are in excess of the expected incidence based on the 1953 cases. This tends to rule out the fallout as a major factor in the leukemia incidence in this area for the 1953 births.

The portion of the tabulation that covers the cases reported among those born in 1943 and after is shown in Table 1.

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Ultimate Failure of Rhythm

A. J. de Bethune's ingenious analysis of the possibility of spacing children by the rhythm method [*Science* **142**, 1629 (27 Dec. 1963)] is a welcome addition to the literature on birth control. But his conclusions, pessimistic as they are, err (I believe) on the optimistic side. Implicitly, his model assumes a constant human physiology. If we assume, as indeed we must, that human physiology is subject to evolution by natural selection, then it becomes almost certain that the rhythm method cannot possibly work in the long run.

Let us imagine a population which employs only the rhythm method for contraception. Such a practice sets up an evolutionary system in which natural selection favors those for whom the method fails. The reasons for failure we can presume to be both environmental and genetic. Women for whom the method fails would contribute more children to the next generation than would women for whom the method works; consequently the frequency of whatever genes favor failure would rise continuously, until ultimately the theory of the rhythm method would be only a historic curiosity.

We can reasonably postulate at least three possible reasons for failure. (i) There may be inheritable factors for irregularity in the menstrual cycle. (ii) Some women may ovulate "on demand," that is, after coitus. (iii) There may be inheritable differences in sex drive, which would certainly operate against the success of the rhythm method.

It may be objected (i) that we have no evidence that menstrual irregularities are inheritable. This is quite true: no one has sought the evidence. In the meantime, where should the burden of proof lie? It may also be objected (ii) that no instances have been found of women who ovulate on demand. Evidence either for or against this possibility is hard to gather, and all that is needed for natural selection to raise havoc with the rhythm method is that *some* women ovulate on demand *sometimes*. That this is within the physiological possibilities for a mammal we know from the example of rabbits, which habitually ovulate on demand. Finally (iii), that there are differences in sexual behavior among human beings surely no one would deny after reading the text and the tables of the Kinsey volumes. The sexual temperance required for the success of the Ogino-Knaus method is found in only a minority of the population. And if anyone objects that we have not yet proved that "passion" has a genetic component, it may be pointed out that even if the inheritance of behavior is wholly social and not at all biological, the result would be the same: passionate failures would replace phlegmatic successes.

At the present time there are many who say we should expend large sums of money on research aimed at perfecting the rhythm method. Perhaps we should, for political reasons. Besides, if the research is competently performed we no doubt will learn something worth knowing. But we should not fool ourselves. In the long run, the "natural" method, no matter how perfected, will be frustrated by natural selection.

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