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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