"COMMENTS ON "LEUKEMIA MORTALITY IN SOUTHWESTERN UTAH"

1. One of the fundamental problems inherent in the study is that since 1954 the association between acute radiation exposure and leukemia has been so well publicized that in the particular area under study it is extremely unlikely that cases would go undiagnosed. One might expect therefore relative to the country as a whole some bias in medical reporting of leukemia in an area which has been publicized as having received relatively high radioactive fallout.

2. Another problem is the extremely difficult one of finding an adequate control population.

3. Using the E. B. Lewis estimate of radiation leukemogenesis, that is, one case/rad/million population, one would expect an excess in a population of 21,000 of a total of 1.5 cases over the 15 year period if (a) everyone had been irradiated, (b) with the maximum 5 roentgen dose, (c) at the beginning of the period. This excess obviously could not be distinguished from the observed rate of 28 cases. Lewis' estimates have been used by the UNSCEAR as the upper limiting case for estimates of leukemia induction by fallout.

4. Radiation-induced leukemia among the Japanese survivors, and among the English Spondylitis cases has been principally granulocytic leukemia. It is interesting to note that the apparent "excess" in the study is attributable almost entirely to lymphocytic leukemia about equally distributed between acute and chronic lymphatic leukemia 3.87 and 3.32 respectively. Chronic lymphatic leukemia has never been alleged to be associated with radiation exposures.

5. The fact that the "excess" deaths occurred predominantly in persons over 35 (a 2 to 1 ratio) would on the face of it appear to be contrary to the generally held belief that children and foetuses are more sensitive to radiation-induced leukemia than adults.

6. The population data does not appear to include the actual population distributions by age and sex that apply to Washington and Iron Counties. In such a small population group differences here could lead to a relatively large error in estimates of expected cases. In any event, it would be helpful to know the basis of the numbers given.

7. We wonder whether the data on leukemia cases are as shown by title of Table 1 applicable to Southern Utah, Southwestern Utah or only to Iron and Washington Counties for which population values have been used. See par. 5, page 2 of the draft report.

8. Great stress is placed in par. 3, page 3, on applying a Poisson distribution to the 28/15 - 2 calculation. Although in a single observation, one may attach statistical significance to a variance of more than two standard deviations, when 20 observations are made, one would expect, by chance, one observation that exceeds the 5 percent confidence. In this light, one is not surpised to find an "excess" number of deaths in one of 15 years (1959) simply from chance alone, just as one is not surprised to find a year, 1952, with no leukemia deaths at all. The concept of clusters of cases in 1959 and 1960 as put forth on page 4, par. 3, would therefore seem to have little substance.

9. Stress has also been laid on the fact that in McMahan's study of leukemia in Brooklyn the "national rates and those from Brooklyn yield numbers in almost perfect agreement." With some 3000 counties in the USA it is not surprising that in an occasional one the rates agree with the national average. By the same token, it should be expected that the rates in a number of counties would deviate from the national average by as much as is seen in these data from Iron and Washington Counties, Utah.

10. The second paragraph on page 5 of the article disclaims any conclusions as to cause of the "excessive" cases in the counties, nevertheless the whole tone of the report is one of accepting the data as having clearly demonstrated excessive leukemia in the two counties. In the end, it highlights a so-called "cluster of cases of primary interest" in 1959 and 1960. It is hard to be impressed by six leukemia deaths in 1959 and not be almost equally impressed by the fact that five of those six deaths were in males, one of which incidentally was in a boy conceived more than a year after the fallout in the spring of 1953 which totalled more in these areas than all preceding and succeeding years combined.

11. In summary, it is difficult to accept the idea that the observed number of cases in Iron and Washington Counties, Utah, 1950-1964, is indeed excessive especially in the absence of information on the number of cases during the same period of time in equally small population groups around the country.