

Honorable Luther L. Terry
Surgeon General
~~U. S. Public Health Service~~
Department of Health, Education
and Welfare

SEP 10 1965

Dear Dr. Terry:

This is by way of follow-up on the meeting of September 2, 1965 between Mr. Ink, Dr. Dushman and Dr. Dunning of our staff with Allen M. Pond, Dr. Chadwick and others from the Department of Health, Education, and Welfare and representatives of the Office of Science and Technology and the Federal Radiation Council.

The meeting concerned a draft report entitled "Leukemia Mortality in Southwestern Utah" by Mr. Edward Weiss, a proposed study "Environmental and Genetic Factors in the Oral Dental and Medical Characteristics of Utah School Children" and a draft press release on these studies. We appreciate very much having had the opportunity to review these documents along with the proposal for the extension of the studies.

As to the Weiss draft report, you will note that as indicated by the Atomic Energy Commission staff comments, we have a number of problems with it. We hope that in its review of the document the Public Health Service will bring to bear all the broad experiences in epidemiology of its National Institutes of Health and its Communicable Disease Center in Atlanta. The attached comments by AEC staff scientists are offered for your consideration. As was agreed at the meeting, the Commission would appreciate learning the degree of variance among groups of 20,000 children, in small population groups, around the country in relation to the national average. These data might assist in placing the Utah statistics in perspective.

We wish to assure you that, as in the past, we are in favor of epidemiologic studies in Southern Utah, and the AEC wishes to cooperate fully in any effort that will clarify the effect or noneffect of radiation exposure from fallout. You will note by our staff comments (attached), however, that there are rather serious reservations in their minds that the proposed studies will produce unequivocal data. At this point in time it is not clear that a proposal of

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modification of the study would be fruitful. We do urge that the investigators exercise due caution in placing the study in proper perspective when presenting it to the public.

Mr. Duncan Clark and Mr. Judson Hardy of our respective staffs are currently working on the wording of the press release attempting to minimize unwarranted concern among the school children and their parents.

Sincerely yours,

s/ D. A. Ink

Ans. General Manager

Attachments:

1. AEC Comments on "Environmental & Genetic Factors, etc."
2. AEC Comments on "Leukemia Mortality, etc."

cc: Hon. Donald P. Hornig
Office of Science and Technology
Paul C. Tompkins, Exec. Dir.
Federal Radiation Council

COMMENTS ON "Environmental and Genetic Factors in the Oral Dental
and Medical Characteristics of Utah School Children"

1. If persons had continued to live in the areas of Utah indicated in the study, they would have received in general, two to four roentgens of total exposure from all fallout since tests began in Nevada in 1951. During the period of major atmospheric testing in Nevada (1951-1958) the guidelines of the International Commission on Radiological Protection and the National Committee on Radiation Protection for normal nuclear operations was 1-1/2 roentgens per year to individuals. Later this was reduced to 1/2 roentgen per year but in doing so the Commission and Committee carefully pointed out that such reduction was not based on evidence of biological damage, but rather constituted a more conservative approach to establishing radiation guides.
2. It is correct that most of the total exposure in the stated areas in Utah occurred within a few months during 1953. This does not alter the conclusion implied in the first comment that persons exposed to two to four roentgens (delivered within a few months) would not constitute a profitable group to study for radiation effects. What these data do clearly show, however, is the need to look at a specific population group. For examples:
 - a. Children conceived after this period of time did not receive the radiation exposure.
 - b. It will be necessary to check quite accurately the location of the subject in the specific 1953 time period. This is further complicated by the fact that the fallout patterns were quite sharp, i.e., potential radiation exposures from the center line of the patterns outward decreased greatly over just a few miles and even within the pattern there were variances. If the subject was not present in the specific areas at the pertinent time, he should not be considered in the study. Contrariwise, those who were present at the time of the fallout and moved to other areas afterwards must be located and examined. It would not be justifiable to assure that these two factors cancel each other. These facts, of course, raise difficulties in the performance of the studies, but are essential if there are any hopes of reaching sound conclusions.
3. The number of subjects involved is so exceedingly small, for these types of studies, that results can be statistically suspect even before obtaining them. Statistical manipulation of data has validity up to a point but no such operations are proper substitutes for inadequate input data. Wide variations may be expected in the effects sought, among groups of 2000 children in the country. Although there has been an attempt to find a comparable control group, the proposal does not give adequate assurance that this has been accomplished,

and even if so, the small number of cases involved would militate against unequivocal data.

It is also noted that some of the possible effects are to be determined by subjective criteria. If this be necessary, the requirement for an adequately designed study is all the more imperative.

4. Some additional uncertainties concerning the validity of the background data and information used by the proposal are:
 - a. The proposal submits as its first argument, in its Introduction, that the relatively high concentration of fallout debris in the air constitutes evidence of high external radiation exposures. It has been repeatedly shown that for the cases of fallout in question the whole body exposure resulted after the material was deposited on the ground. Further, relationships between concentrations of radioactive debris in the air and that deposited on the ground have not been established.
 - b. The proposal states " . . . 3 curies would be retained in the body." This is a generalized statement which may lead to false assumptions. The fallout debris from Nevada tests has been found to be in general very insoluble, at least in the nearby areas, thus the lung would be the critical organ. The estimated lung dose from inhalation of these relatively high concentrations of fallout debris is not great compared with the exposure from natural occurring materials in the air. In the case of iodine, much larger potential doses result from ingestion rather than from inhalation.
 - c. On page 2 of the proposal it is stated: "It has been estimated by one group that the 700 children residing in Washington County who were less than two years of age during the peak exposure period received, in 1953 alone, between 120 and 440 rads to their thyroid from I-131 in milk." This estimate was made by one individual whose methods of calculation received a critical appraisal by an Ad Hoc Committee of experts. Another estimate, reported on the same page of reference 10 (but not quoted in the proposal) was 68 rads with an uncertainty factor of 4. This estimate was based on comparisons with relative concentrations of beta activity in the air. The method used in the second estimate is quite unreliable as is the method used in the first estimate.
 - d. On page 4 of the proposal there is mentioned that one of the tissues of interest is the bone in relation to strontium. The levels of strontium-90 in the foodstuffs in the Nevada-Utah environs are among the lowest in the country.
 - e. It is noted on page 5 that "Experimental studies in animals have shown that a variety of maternal traumas can affect the development of feral teeth." With such variables as these, and many others, how does one delineate the problem?

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 relative 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 fetuses 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 surprised 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.