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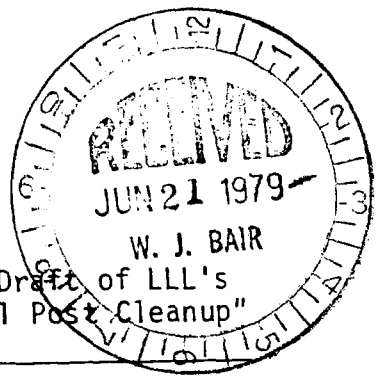

Date June 18, 1979

To W. J. Bair

From R. O. Gilbert R. O. Gilbert

Subject Review of Preliminary Working Draft of LLL's "Reassessment of Enewetak Atoll Post Cleanup" distributed June 4, 1979

My comments below are more detailed than in my memo to you dated June 12, 1979. To begin, I have three specific recommendations to make:

1. The authors should be required to include in their paper the equations used to estimate doses. In my view the dose assessment paper should be completely or nearly self contained either within the text or in appendices. A good example of the style I'm thinking of is the paper "Plutonium Transport and Dose Estimation Model" by W. E. Martin and S. G. Bloom, in Transuranium Nuclides in the Environment, IAEA, 1976, pp. 385-400.

2. Desert Research Institute statisticians at Las Vegas who are most familiar with the Enewetak radioisotope data should be used more effectively in assisting the authors with statistical aspects of the dose estimation task. This might be best accomplished by one of these statisticians spending one or more weeks at Livermore working directly with the authors, although effective assistance could perhaps be given from Las Vegas. My reasons for making this recommendation should be evident from my more detailed comments below.

3. A complete statistical analysis of all data relevant to the dose assessment should be conducted and included as an appendix to the dose assessment paper. This analysis would serve as a basis for dose assessment procedures. Included would be diet survey methodology and data, soil data obtained during the post cleanup grid sampling, an evaluation of these post cleanup data with data obtained prior to and during the cleanup operation, quality control samples, etc., i.e., a complete workup of relevant data. This would be a considerable

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task but in view of the scrutiny this dose assessment will receive, I feel LLL needs this kind of documentation to lay out the available information. Desert Research Institute statisticians at Las Vegas would seem to be the logical choices for spearheading this project. However, their work should be reviewed by statisticians outside the DOE community as well as by ourselves (Advisory Group).

General Comments on Working Draft

As acknowledged by Robison, the present preliminary manuscript is incomplete in a number of important areas. The main areas of deficiency from my statistical point of view are:

1) Inadequate description of how the diet survey was conducted and how the resulting data were statistically analyzed. Since the dose estimates are based on these diet data, it is crucial that the magnitude of sampling errors by quantified. It is discouraging, to say the least, that the authors did not indicate the number of people interviewed in the survey. Any statistician (or lawyer) worth his salt would choke on that omission. Also, no details are given on how the survey was actually conducted. This methodology must be known before we can evaluate whether the survey data are more reliable than expert opinion.

2) Completely inadequate description of how the soil data were statistically analyzed and summarized in preparation for use in the dose models. For example, we do not know how "less-than" concentrations were handled statistically. It is my understanding that on Engebi there were few "less than" concentrations obtained. However, on other islands most of the data at depth were less than detection limits. I suggested to W. A. Phillips several weeks ago that they compute the dose estimates using average soil concentrations computed in two ways: (1) using the MDA (minimum detectable activity) for each less-than number, and (2) using the actual concentration reported whether it be negative, positive or zero. This would indicate how much dose estimates might differ depending on how these "less-than" data are treated.

Also, the soil data in Appendix D are inadequate to allow us to double check their dose calculations. Data units are not identified, there are too many significant digits, and the error terms are not defined.

3) Lack of any error analysis on final dose estimates. Robison indicates this will be forthcoming at a later time, but I include it here to emphasize how important it is that this type of analysis be conducted. In fact, I believe the whole approach used in the paper should be directed toward evaluating the probability that a given individual will exceed guidelines. Admittedly, this approach is fraught with difficulties. Nevertheless, I recommend that the authors begin thinking in those terms so that some future version of the paper can reflect the probability approach.

4) Tables 1, 2 and 3 (giving initial dose rates, concentration ratios and diets) are deficient in that absolutely no attempt is made to quantify or even mention the range of errors in the data or to indicate the number of samples involved. I have consistently raised this objection to past LLL dose assessments, but to no avail. We should insist that future LLL reports follow established scientific principles and indicate whenever possible the limits of error on these types of data.

5) Others in the Advisory Group are more qualified than I to evaluate whether or not the specific models used to obtain doses are satisfactory for the Enewetak situation. However, I think the authors should indicate those instances (if there are any) where these models have in any sense been validated. For example, has Bennett's bone model been tried on data other than that for which it was initially developed? If so, how did it perform? Bennett's (1973) paper is quite sketchy as to methodology. For example, how were the parameters c, g, and λ estimated? A more crucial question is whether LLL used Bennett's estimates of c, g and λ in the Enewetak calculations. They could be severely criticized if that is the case since Bennett's estimates are based on New York and San Francisco data. The applicability of the parameter estimates used should be established in the paper.

6) Whose idea was it to use the phrase "dial-a-dose" on page 18? Need I say more?

Specific Comments

1.	p. 1	Is "reassessment" or resettlement" the first word in the title? The cover letter used the former. I don't like either title. How about "Post Cleanup Dose Assessment for the Enewetak Atoll."
2.	p. 3,4	The introduction should outline the contents of the paper, or else a summary and/or abstract should precede the introduction.
3.	p. 3,4	No mention is made of the extensive soil and IMP sampling undertaken immediately preceding and during the cleanup operation. A brief description or mention of this effort should be made to tie together a more complete history of the sampling effort.
4.	p. 4, 7 lines from botton	The 10-15 cm increment should be included.
5.	p. 4, last line.	Description of profile sampling insufficient for me to follow completely.
6.	p. 5	Were quality control (QC) soil samples analyzed? The manuscript doesn't say one way or another. If QC samples were analyzed, these data should be summarized and presented. Documentation must be available to indicate the reliability of the data. This is a very important point in my mind.
7.	p. 5 Lines 11,12	What laboratory did the gamma spectroscopy for ^{137}Cs?
8.	p. 5, Lines 6,7 from bottom	Where and why was the 25 meter grid used? Why was a 50 meter grid used? We need more details on the sampling design and underlying rationale.

9.	p. 5 Lines 10-25	Additional details on Analytical Procedures and External Gamma Measurements should be given even though other other references cover these topics in detail.
10.	p. 5 Last 2 lines	Who specifically is preparing the report and when will it be available?
11.	p. 6, Line 13	What does "we have <u>developed</u> concentration ratios" mean? I prefer "estimated" or "approximated" rather than "developed".
12.	p. 6 Lines 19,20	We are not told whether equal weight is given to each 5 cm increment down to 40 cm to estimate concentration ratios? This type of detailed information is needed for this Advisory Group to evaluate LLL's work. It could be provided in the text or in an appendix.
13.	p. 6 and Table 1 (p. 23)	Are these average doses? If so, what average, the mean or median? How many readings were taken per island? How precise are these averages? What is the standard error about each number? Are they justified in reporting 3 significant figures? The average of the 4 quadrants on Engebi doesn't equal the island average. Should they? How were the results for the southern islands determined?
14.	p. 6 Lines 1-3	No ^{241}Am data are given. Why not?
15.	p. 6 Lines 12-20, Table 2	Table 2 is incomplete. They should at least indicate the number of hard data used to estimate each ratio and provide an estimate of each ratios standard error even though it may only be approximate.
16.	p. 6 Line 6 from bottom	The assumption is made that ratios do not depend on soil concentration level, island, or time. This should be explicitly stated and justified.
17.	p. 7 Line 4 from bottom	Bennett's model is <u>not</u> used to <u>correlate</u> Sr-90 in diet with that in bone. That is, Enewetak's data (observations) are no available on both variables for which a correlation coefficie can be computed. Bennett's model is a way of approximating

or estimating Sr-90 in bone from that in diet. The statistical concept of correlation is not involved. I suggest the authors do not use the word "correlate" since bone data are not availab

18. p. 8, 1st paragraph — This paragraph needs to be rewritten. Why is Bennett's model thought to best reflect Sr-90 concentrations in bone at Enewetak? What alternative models are there to choose from? Bennett's model was developed and fit to Sr-90 concentrations in diet and in mineral bone as determined from autopsy samples. However, this paragraph reads as if output from the model calculations somehow involve autopsies. Also, Bennett's model will use estimated not actual diet information for Enewetak application.

19. p. 8 Last 4 lines — The authors should explain more fully why Martin and Bloom's model, which lumps "the upper two long term compartments into one," is acceptable for Enewetak application. Why is the difference between the ICRP and Martin and Bloom's model not significant?

20. p. 8, Line 4 — What information is available on AMAD. When was an AMAD of 0.5 used?

21. p. 9 Lines 11-14 — Why were 15% and 85% chosen as the short and long term intake parameters for the two compartment Cs-137 model? Cs is probably the single most important radioisotope on Enewetak. Yet, the authors give only 2 or 3 lines to describing model parameters!

22. p. 10, Line 5 from botton — What is meant by the phrase "Engebi is the limiting living pattern of those evaluated"?

23. p. 11 — I've emphasized above the description of the diet survey results and the methodology of the diet survey are completely inadequate. The sample questionnaire displayed in Figure 2 leads me to question the validity of the survey results. For example, what was the definition of "normal" and "famine" conditions applied to the Enewetak people? Under

normal conditions what percent of the food is assumed to be important, or was the definition of "normal" left undefined and/or vague. The authors noted that they believed (even before the survey was conducted) that the diet estimates previously used were too high. What precautions were taken to ensure that their preconceived notions did not bias the way questions were asked of the Enewetak people so that biased responses were not obtained? Their survey results may truly reflect actual diet conditions, but they must be able to document that such is the case!

24. p. 12 — What exactly is a maximum annual dose rate? It should be explicitly defined.

25. p. 12 — Throughout the Results section the authors talk in terms of predicted dose rates. I would prefer that they talk instead in terms of projected or estimated doses. Also, nowhere do they indicate how "less-than" soil concentrations were handled in computing average soil concentrations. Concerning the dose tables, 2 rather than 3 significant digits is all that can be justified. Actually, 1 significant digit is probably all we can really feel comfortable with.

26. p. 15, Line 7 — The childs entire diet intake cannot come from Engebi under "normal" conditions since the latter assumes imported foods are available.

27. Tables 19-44 — Condense into only one or two tables or graphs. Too much wasted paper in present format. It's also hard to get a quick idea of the overall picture with the present format.

28. Tables 47-50 — Need "error limits" on doses.

29. p. 16 — Do these maximum annual doses take into account the skewed nature of the soil data, i.e., was lognormal data assumed?

30. p. 17, Lines 6-9 — How was the 10 month figure determined?

Table 49. — 24000 mrem/30 years is obviously in error (typo).

31.	p. 18	The authors should not use "dial-a-dose" terminology for obvious reasons!
32.	Appendix 7, Line 7	The equation for D_τ is mixed up. What are the definitions of τ_{CS} and τ_{CO}? Also, the authors should emphasize more fully that Appendix A relates to external doses.
33.	p. 18 Last line	Is this the definition of maximum annual dose rate used thoughout the paper? I don't think so, but the reader could get confused.
34.	p. 19	It's not clear from the authors discussion of Appendix C whether the dose estimates in the main text contain the contribution from coconut assumption. Are the doses in Appendix C <u>maximum</u> annual doses, and are the doses for males only?
35.	p. 19 Last paragraph	I've indicated above the inadequacy of Appendix D.
36.	p. 21, Line 14	What exactly is the "significant number" of Enewetak people interviewed?
37.	p. 21 Last paragraph	When will these ^{241}Am data be present?