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LOS ALAMOS SCIENTIFIC LABORATORY
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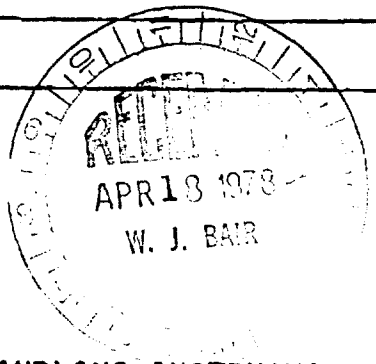
TO	AT	FAX NO.	CONFIRM. NO.
Dr. William J. Bair	Biology Department Battelle Pacific Northwest Laboratories Richland, WA	946-2718	



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COLLECTION Marshall Islands

BOX No. 5685

FOLDER Enewetak April 1978

To: W. J. Bair

From: J. W. Healy

Subject: ~~Enewetak~~ Dose Assessment
ENIWETOK

I find this a difficult document to review because the data leading to the basic assumptions are not presented and dose calculation techniques and parameters are not included. I became particularly suspicious upon our review of the coconut data at the meeting and noting from Table 4 that the coconut data provides over half of the terrestrial food-path dose. I would feel that, at a minimum, the original data should be found and a good analysis be done.

The GI uptake factors should be redone using experimental data rather than reported factors from Larsen and Bloom and Martin. At the moment the paper reminds me of the two handed scientist who says that it is safe, but on the other hand it is not safe. I still intend to get a detailed review of these factors out shortly.

The inhalation pathway is not much better. At the minimum, the inhalation should be considered separately as ambient air (with and without disturbance) and as local resuspension with time periods attached. The latter value could, perhaps, be estimated by resuspension factors.

I cannot check the dose calculations because I do not know their basic assumptions.

I would also add that it is disturbing to come down to the wire and find this type of document and uncertainty. Perhaps we should consider recommending a change in the overall management and funding of projects related to the islands so that studies to obtain needed data are expedited.

More detailed comments follow:

1. P. 2, line 9. The statement that the transuranics are "...readily available...to man..." is wrong. Generally, there are a number of discriminations against them.

2. P. 3, line 6. The value of 2 liters of drinking water per day seems high as compared to the ICRP reference man. Is there evidence to support it?

3. P. 3, lines 13-14. Here a plutonium to americium ratio of 2 to 1 is assumed. Surely with the number of measurements that have been made, a better value could be chosen from the data. The ratio chosen appears to give much more americium than I would have expected.

4. P. 3, lines 16-18. Is there a basis for the assumption that one-half of the surface transuranic concentrations will be

in the root zone? I would guess that in undisturbed areas it would be less and in disturbed areas it could be greater. The potential impact of plowing should be considered.

5. P. 5, line 4. A reference should be given to the work of Stuart. The coefficient listed appears high to me, but I will have a review available in the next week or so.

6. P. 5, lines 16-17. It should also be noted that the EPA made no attempt to justify their numbers and they appear to be assumed. In particular, there seems to be no justification for using a higher uptake for ^{238}Pu, except, possibly, with $^{238}PuO_2$ particles. In fact, Weeks, et al. in 1956 reported on uptake from nitrate solution over a range of 0.019 to 140 µg intake with no difference in uptake. Plutonium-238 was used to obtain the low mass feedings.

7. P. 6, lines 1 and 2. The three orders of magnitude should not be taken as a result of experimental work by Larsen. I suspect that it will be lower but must finish the review.

8. P. 6, lines 13-14. Justification should be given for the 10^{-3} uptake by americium. While data are scarce, what we have indicates a somewhat lower value. Again, this will be in the review.

9. The discussion on the uptake is unsatisfactory in that the liver is not included and many of the values quoted included the urine component so that they are not strictly comparable.

10. P. 6, line 21. It sould be helpful to provide a better derivation for the plant uptake factors in Table II including the actual data used. This would enable the reader to bettor assess the validity of the values. Was americium assumed to have the same plant concentration ratio as plutonium?

11. P. 7, par. 1. It would be useful to the reader if the data for the birds and bird eggs were included. In particular, the concentration ratios that were used in the calculations should be included.

12. In the dose calculations throughout it would be useful if the exact parameters (bone weight, energy of alpha, etc.) along with the calculation methods were given. It is not clear, for example, whether the decay of ^{241}Am and ^{238}Pu over the 70-year period is included.

13. P. 12, lines 6-7. The mass loading of 100 µg/m^3 needs greater justification. For example, the time period of sampling and the activities in progress for the 80 µg/m^3 mentioned later

should be described. It should be remembered that our interest is on the yearly average including periods of eating and sleeping as well as time spent on the water. The AMAD of 0.5 μm seems small, particularly when the main source appears to be mechanical disturbance.

14. P. 12, lines 19-20. The statement that 20% of that on the filter is usually regarded as respirable raises the question of how this was included in the calculations. The definition of the AMAD of 0.5 μm implies fractions deposited and, therefore, the "respirable" fraction.

15. P. 19, par. 2. It should be noted that such a program is in progress at Battelle and that numbers from their studies are not as extreme as those picked from reviews or articles for another purpose (i.e. Larsen). It would be well to draw such conclusions from the published experiments rather than from an interpretation.