COMMENTS ON THE MARCH 1978 DRAFT OF "ASSESSMENT OF POTENTIAL DOSES TO POPULATION FROM THE TRANSURANIC RADIONUCLIDES AT ENIWETOK ATOLL"

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The following are specific comments from several members of the Advisory Group on Cleanup of Eniwetok Atoll. The redundancy in these comments serves to indicate areas of special concern to the reviewers.

The report is well done and the authors are to be complimented. The comments are directed more toward the inadequacies of the data available to do a dose assessment than on the methods used by the authors. Apparently in the interest of keeping the size of the report down, much of the details of the dose assessments were excluded. These details would have been very helpful to the Advisory Group in reviewing the report. The only other major criticisms are directed to: the report's selection of assumptions and data that maximize the dose estimates rather than the selection of assumptions and data that would yield most probable dose estimates; the lack of sufficient statements about the uncertainties of the data base for the doses; and the lack of attention given subgroups within the population, (e.g., infants, children) and the maximum dose individual.

The authors should be invited to direct questions regarding the comments to the Advisory Group.

p.2, line 7. It is not clear what the quantity and quality of the more recent data, i.e., what new data is now available that emphasizes coconuts as the limiting route for transuranics.

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Need for guidance on use of 3×10^{-5} or 10^{-3} as GT coefficient for $^{239,240}Pu$.

4(a), line 1. Why is it assumed that plowing will reduce the surface soil concs by a factor of 2. If the Pu is on the surface only, than the reduction for inhalation purposes will be much more than 2.

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.

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?

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.

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.

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.

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.

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.

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.

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.

p.6, line 21. It should 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 better assess the validity of the values. Was americium assumed to have the same plant concentration ratio as plutonium?

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.

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.

p.12, lines 6-7. The mass loading of 100 $\mu g/m^3$ needs greater justification. For example, the time period of sampling and the activities in progress for the 80 $\mu g/m^3$ 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.

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.

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.

The concentration ratios of vegetables seem low; probably should be closer to 10^{-1} and 10^{-2}. Thus, actual observations are needed and not an estimate from papaya and bananas.

More data are needed for CR values as well as more fish sampled to validate the last sampling.

The estimates of Pu in cistern water is made using a K_d value (soil to water in ml/g) of 5.9×10^5. This is a very high K_d value for Pu (VI) thus it probably represents the K_d for Pu(IV) and not the K_d for Pu(VI), which will dominate when the water is chlorinated. A more realistic K_d would probably be 10^2 to 10^3. Also what is the concentration of Pu in soil particles that could be deposited on the roofs? As one can observe a K_d of 10^3 instead of 10^5 will make a great difference in the dose rate by drinking chlorinated cistern water. I think a serious error is being made here by using the assumptions stated in the report.

(Some recent work at Oak Ridge has shown that a gastrointestinal tract coefficient of 6×10^{-3} is necessary to account for an observed lifetime body burden of Th indicating that the 5×10^{-3} coefficient, as stated in their report, may be more representative than the 3×10^{-5} coefficient for Pu.)

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average of all the raw soil data as a whole collected on the island? It is suggested that the authors either define the word average or delete it.

In the last line of Table 9, the datum 1.11×10^{-2} is incorrect and should apparently be 1.11×10^{-1}. Also, in Table 4, it appears that the datum 0.159 in the row for 20 g/day should be 0.149. The tables should be carefully proofread since there may be other errors.

It would be helpful to the reader if the dose estimates for at least one of the tables (perhaps Table 12) were plotted on graph paper (% time versus dose for each hypothetical soil concentration). This would make clear the simple multiplicative relationships between the dose estimates in the table.

The $^{241}Am/^{239-240}Pu$ ratio data in fish muscle mentioned on page 9 (last paragraph) should be presented, especially since the data are described by the authors as being "insufficient" to arrive at "meaningful averages."

To what extent, if any, is the assumed diet realistic or conservative? After what period of time is it anticipated that this diet will, in fact, be available as the primary, if not sole, source of food? For example, are the people now to some extent dependent upon imported food, and would this continue?

How do LLL soil surface (0-3 cm) measurements compare with EPA recommentations (0-1 cm)? (Perhaps information related to this could be obtained from the Rockwell comparative soil sampling program at Rocky Flats.)

How reliable and consistent is the Pu:Am ratio of 2:1? Is it justifiable to assume a 2:1 ratio for both the surface soil (0-3 cm) and the root zone (0-30 cm)?

How realistic are the occupancy factors stated? Are these valid also for women and children? For example, children might be expected to spend more time on a village or picnic island, but would their estimated dose be decreased because of avoidance of agricultural islands, increased because they might be expected to play in the dirt, sand or coral, or would the dose be essentially the same as for an adult?

A gut transfer factor of 3.0×10^{-5} may not be conservative. EPA recommends 10^{-4} for Pu-239, 240 oxide, 10^{-3} for oxides and non-oxides of other isotopes of Pu, Am and Cm, and 5×10^{-3} for biologically incorporated material. Use of 10^{-3} for Am is okay, but Pu-239, to say nothing of Pu-238, absorption factors may have been underestimated.

This subject is one in which numbers are given in the report, but little is said about the experimental conditions or the applicability of the numbers to the Eniwetok dose assessments:

a) Pu in chlorinated water may not remain as +6 in physiological milieu.

b) Reference to Stuart is not given.

c) How significant is Pu-238 dose from marine pathway if transfer factor of 10^{-3} is used.

d) Concentration factors (ratios?) appear very important for coconut meat and milk. To base such an important parameter upon 5 coconuts (some of which are lower values than "LT" values) raises questions as to their suitability and accuracy. (It is incredible that the Bikini soil and coconuts have not yet been analyzed; also, presumably nothing is known regarding biological incorporation of Pu in coconut meat/milk!).

e) Is there no information on leaf vs. fruit concentrations?

Little was said about analytical methods and deviations.

All derivations progress from food, water and air concentrations to dose. It might be informative to understand inhalation/ingestion body/organ content dose.

The marine pathway raises a number of questions as to the '72 survey and the '76 survey which probably can only be resolved by additional data. The conflicts between the two sets of data are not resolved, and the reasons given for accepting the '76 values (e.g., the data match global values) are not convincing, especially when the '72 samples were conducted by 3 labs and the '76 data is given only by one. (Is it to be expected that the Eniwetok marine life Pu values should match those in the North Atlantic or the Irish Sea? It would be a bit surprising to expect similar values.)

Other issues re marine food paths and derivations include:

a) How representative is a single fish, the mullet, of either the islanders' diet or of the fish and seafood population? I would think that other fish and the coconut crab should need to be sampled before stating that the dose via marine life is insignificant.

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b) How valid are the statements (made at the meeting) that the mullet does not migrate, presumably either between islands or across ocean/lagoon barriers? If it is not a migrating fish, were the fish obtained in those areas most likely to be fished by the islanders?

c) What is the basis for the assumption that the mullet is the most direct and representative link between marine contamination levels and dose to man?

d) It is stated that there is some uncertainty about what fish tissue the Marshallese actually ingest. This sounds difficult to believe considering that we have had 30 years--more or less--to observe/study their diet. If nothing else, why don't we ask them? Unreal! If it is true that we really don't know, why are muscle and skin assumed?

e) If there is a difference of a factor of 8 in the $^{238}Pu/^{239}Pu$ ratios in fish (mullet?) muscle in different parts of the atoll, why are mean concentrations used and why is 3×10^{-5} used as the gut transport factor for ^{238}Pu?

f) On page 8 it is stated that use of 10^{-3} instead of 3×10^{-5} for Pu-239, -240 would increase the dose rate from 3.2 mrad/yr (Table 5) to 9.9 mrad/yr. Does this also include 238 Pu? What if the ^{238}Pu component value is 10^{-3} and $^{239,240}Pu$ is 10^{-4} or 3×10^{-5}?

g) What this all reduces to is that we don't really know anything more about the marine pathway than we do the terrestrial or, for that matter, the inhalation one.

If there may be a Pu problem at Bikini with surface soil concentrations of 10 pCi/g (page 11), how can we consider settlement at Eniwetok with levels of 10-40 pCi/g?

The uncertainties of the inhalation dose calculations have already

difficult: should anyone need to accept a higher risk than the "average"? Considering all of the uncertainties, it is felt that averages are acceptable as long as maximums similarly are defined (e.g., a residence island might have an average of 6-8 pCi/gm with no area of the island to exceed, say, 30 pCi/gm).

The above becomes tied into the applicability of the EPA Guidance to the Eniwetok return. This has literally forced OES to consider dose projections from transuranics, something that heretofore had been either not considered or considered to be insignificant. Obviously both "considereds" were in error. It is felt that the EPA Guidance should be considered to be what it is -- guidance. The closer we can get to or below it, the better off DOE and the Eniwetok people will be. However, it is doubtful that EPA will insist on the use of their Guidance as an upper exposure level and have indicated that if it can be met we should by all means do so, but if it cannot be met the reasons are understandable because of the uniqueness of the situation and because the benefits, while intangible, no doubt exceed the additional risk. Furthermore, EPA stated that their Guidance was intended for use in land deeds, development and use, and that these concepts undoubtedly do not apply to the Eniwetok culture. In addition, it was stated that the Guidance was intended for U.S. public/private land use, and was not directed toward sites of atmospheric nuclear weapons tests (i.e., NTS, N.M., Bikini, Eniwetok). Consequently we should make every reasonable effort to assure that the Guidance is complied with, but it is not necessarily a prerequisite for resettlement--at least from EPA's perspective.

How all this will help OES within 2 weeks is a mystery to me. Any suggestions or help in determining clean-up levels for residence, agriculture, and visiting islands would be most gratefully appreciated.

While the bone dose exceeds EPA Guidance to a much greater extent than does the lung dose, the largest single contributor to the bone dose is translocation from the lung. If the inhalation assumptions are conservative by up to a factor of 10, the lung dose becomes quite acceptable and the bone

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dose is reduced almost by 1/3. Possible conservatisms in ingestion parameters (e.g., concentration ratios) diet estimates may lower the bone dose still further; on the other hand, raising the GI absorption factor will increase it. The unknowns and uncertainties in the terrestrial and marine ingestion pathways almost preclude any realistic estimates via this exposure route.

More recent data appear to show 25 fold less Pu in fish from Northern Islands. This alone could balance the factor of 33 between the suggested GTC and ICRP GTC. Reported effect of clorination on GTC needs to be verified. Note that reference is not published.

Use Pu values for fruit rather than leaves if the latter are not eaten. Can not introduce conservative factors for each parameter.

Doubt if Pu VI would remain as such in human GI tract. Probably would become PU IV.

Perhaps water catchment systems could be replaced on lagoon side of houses since suspected Pu contamination may be from ocean side.

What fraction of population expected to live 70 years?

Data on air concentrations of Pu?

Disturbing that importance of coconut in food chain still not resolved since is major contributor to dose.

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