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Santa Fe, New Mexico

February 1, 1957

Colonel Dent L. Lay, Chief
Weapons Effects Division,
Armed Forces Special Weapons Project,
Washington 25, D. C.

Dear Dent :

With further reference to our telephone conversation of this date, I am enclosing for your information a copy of a letter to General Luedecke. This covers some broad comments on the chapter on "World-wide Fallout". In addition I would like to send you some detailed comments which you may wish to collate with those of your own and of the members of your group.

The material as offered is very poorly orgaized and repetitive, but that is not a serious obstacle. If other things were satisfactory, we could readily take care of reorganizing the material, and this would, incidentally, cut its length considerably.

General comments : The title should more appropriately be "World-wide Fallout from Weapons Tests." Much of the material is already in the ENW. In some cases an attempt is made to refer back to what we have written and in other cases this is ignored. The separation between "nontechnical" and "technical" parts, with the same stuff repeated over again, is poorly done.

Page 2. Lines 1 and 2. By the time the particles are blowⁿ by the wind through leaves (sic !), surely the particles are so near the ground that they will soon settle anyhow. This is not comparable with removal by rain from hundreds or thousands of feet up.

Page 7. Table 0.1 is frightening and very dangerous to put out. The smallest value given is five times the maximum permissible concentration. Somebody will pick this up, out of context, and the results may be disastrous.

Somewhere here there should be explained the correlation between microcuries in the body and the Sr-90/Ca ratio. It seems to me that the latter^s is the important quantity. Thus, although 1 microcurie would be permissible in an adult, it would be very serious in a baby. The Sr-90/Ca ratio, however, would seem to be quite general. This point needs clarification.

Page 8. Lines 10 and 11. "Cesium is not listed ... standard man." Isn't the same true for strontium? This should be clarified. The impression is left that strontium in the body is normally appreciable, which is not true.

Page 9. Paragraph 0.19, sentence before last seems to put the "cart before the horse". Isn't the data in para. 0.7 based on studies of geographical distribution of fallout? I don't understand what is meant here.

Page 9. Last line and elsewhere. A distinction is made between total and available calcium, but the same distinction is not applied to strontium-90. Surely there must be such a difference. If it isn't important, then this should be stated.

Page 10. Paragraph 0.22, last sentence. "Invariably" does not agree with Table 0.6 on page 30.

Page 12. Paragraph 0.27. The last sentence is very unsatisfactory. If there are later data, then why not give them. Alternatively, say that a smaller value is possible, but the larger value is adopted for safety. However, there is something later which bears on this point. I will refer to it in connection with page 33.

Page 12. Paragraph 0.28. Of all places to put this at the end of the section -- it should go in at the beginning. I don't understand sentence 4. "The amounts... agree... with statements made in this chapter." Isn't the reverse true-- the statements in this chapter are based on the results? If not, then I'm lost! I think pinpointing the U. S. Midwest on the last line of the page is very unwise.

Page 18. Paragraph 0.35 and Fig. 0.2. I cannot help but feel that this lengthy and detailed quotation from a U. K. report, which is not even identified, is quite out of place. The results of the U. S. studies are relegated to a footnote! Surely this determination of the residence half time for the tropospheric fallout isn't so important that a curve and detailed quotation ~~is~~ necessary. If so, then what about the stratospheric fallout -- shouldn't that receive equal treatment?

I wonder whether Fig. 0.1 serves any useful purpose that isn't covered elsewhere, e.g., Figs. 0.7 and 0.8.

Page 19. I very much dislike the long footnote on this page. Does every small point have to be documented in such detail? If so, we should have an encyclopedia!

Page 20. Paragraph 0.39. I don't see the point of this calculation of dis./min./gal., based on the very particular case of 1 millicurie of Sr-90 per sq. mile dissolved in 1 inch of rainfall. It isn't used anywhere. And what does per square mile refer to? The atmosphere through which the rain is falling? Surely, that doesn't make sense. I just don't understand what is intended.

Page 24. Table 0.4. To what date do columns 3 and 4 refer? Is it 1953? And how were the 1955 figures in the 1st column obtained?

Page 25. Paragraph 0.46. Line 2. "The collection pot is an excellent differential method." I don't understand what this means.

Page 25. Paragraph 0.48. Line 4. "Assume (1) no further detonations... calculations. This is just fantastic and ridiculous to make such an assumption. Calculations based on it are valueless. We would just get laughed at for doing that sort of thing.

Page 26. I don't see how equation (0.3) was derived, neither do I understand its purpose here. Three lines below the equation, we are told "air should have contained in October 1956..." But did it? In any case, is this bad or what?

Page 26. Equation (0.4), at bottom. How are the numerical factors derived? I suppose I could work it out if I took the time, but most of us have other things to do. The purpose of a book is to save people time and effort of digging things out for themselves.

Page 27. Equations (0.5) and (0.6) should explain why the $e^{-t/40}$ is not included in the integral. Presumably because the Sr-90 decays irrespective of where it is.

Page 27. First line. We now introduce the date October 1956 and combine it with an expression previously derived for May 1954 (bottom of page 25). I guess the reason is given by the statements at the top of page 28, but I'm not sure. In any case the whole thing is very confusing.

Page 27. Under equation (0.6). "This equation assumes, ..., valid." Since further tests have been made by the U.S.S.R., the equations are already invalid !

Page 27. Line 3. I don't see how an area on Fig. 0.7 can give a value in milli-curies per square mile.

Figs. 0.7 and 0.8. The significance of the numbers of "megatons of fission" is not clear, especially as the arrows have been omitted from Fig. 0.7. I guess that the 0.8 refers to the area under the heavy curve, and 0.4 for that under the horizontal dashed line. (Incidentally, a rough measurement of the areas does not seem to agree with this.) The curve of Fig. 0.7 is repeated in Fig. 0.8, and here the arrows are included. Is it necessary to repeat the ~~figure~~ curve ?

In Fig. 0.8, I guess that 2.2 megatons refers to the areas between the curves, i.e., the extra fallout between the two dates. ~~But~~ On second thoughts, that may be wrong, it may be the area between the horizontal lines -- I don't know. And what does the 1.8 megatons refer to ? Wouldn't it be better to restrict Fig. 0.8 to one curve only, that for Feb. 1955. It would be easier to see what the numbers of megatons meant.

Page 29. The soil-to-plant transfer is a mess. Table 0.5 gives data indicating that K is mostly less than 0.5. Then three lines below the table, it says that a value of 0.7 is more likely, and finally on page 30 (Table 0.6), the data clearly show that K is greater than unity. An attempt is made to pass this off, at the beginning of paragraph 0.56, by talking about an "apparent discrepancy." It isn't apparent it is real ! Surely, one can't pass off data from such widespread locations as the U. S. and New Zealand. If plants absorb nutrients from the leaf surface, as I well know they do, then they may very well have a higher Sr-90/Ca ratio than the soil. Let ~~is not~~ put out data that invite obvious adverse criticism. The value of 0.7 is used later for K, and there is absolutely no justification for it.

Page 30. Table 0.6. It is regrettable that this does not conform to Table 0.5 in so far as the latter gives K values, but the former gives data from which K can be calculated. Perhaps the solution will be to omit them ~~both~~, especially as the value of K used later has no relationship to the values in either table. How futile can you get ?

Page 31. Paragraph 0.57, lines 6 and 7. "...current observations...varies from 1/10 to 1/3." Both of these fractions are larger than 1/12, yet ~~neither of these~~ ~~the~~ the smaller fraction is used.

Page 33. Lines 6 and 7 "...the discrimination factors...young children." This implies that if conditions remain much the same the Sr-90/Ca ratio in the bones of children born in 1956, say, will not increase during the rest of their lives. If this is so, it is important. Otherwise, one might draw the conclusion that a child born in 1956 will increase its Sr-90/Ca ratio throughout life, even if the rate of weapons ~~testing~~ remains constant. There is a point here which the public is entitled to know about, *and it should be elaborated.*

Page 33. The soil to human bone factor is worked out to be 1/24. I wonder if this is the "smaller number" referred to on page 12. There is something discrepant here, anyhow. To give 1/12 on one page and work out 1/24 on another is not good.

Page 33. Paragraph 0.62. Line 4. Should one use an average value here ? I don't think this is justified. The value used should be for the youngest children only.

Fig 0.7 + 0.8 shows also
give latitude of USSR
left - extra fallout!

Page 34. First line. Here we have the discrimination factor $1/12$ again, with a comment that it may well be $1/50$. The evidence presented does not bear this out. In the first place, the soil to plant factor 0.7 may be too small, and on page 31 it is stated that the soil to milk factor may be larger than $1/12$. There is nothing to show that any of the factors are too large. This sort of thing is sure to bring a lot of criticism in such a sensitive area.

Figs. 0.9 and 0.10. I can't believe that both of these figures are necessary, especially as the data are in general agreement. I presume Fig. 0.9 comes from U. K. data and Fig. 0.10 from U.S., but this is not clear. However, I can't see any justification for both. It is true that by distorting the abscissa in Fig. 0.9, which I very much dislike, it has been extended to 70 years, but the data beyond about 30 can be left out. Incidentally, in these two figures the ordinates are given as MMC, which is wrong -- they should be μC . ~~Incidentally,~~ ^{Further,} the change of units from $\mu\text{C Sr-90/kg Ca}$ earlier (e.g., Table 0.1) to $\mu\text{C Sr-90/gram Ca}$ by no means contributes to the clarity of the presentation. Let us make up our minds about the units we are going to use and stick to them!

Page 34. Paragraph 0.63. Lines 4 et seq. The conclusion about the amount of Sr-90 in children's bones should be elaborated. The bare statement will not convey much to the uninitiated. What is needed is a general conclusion to be drawn from the results presented, and--very important-- some sort of estimate as to what might be expected in the bones of these children as they grow up, making some sort of reasonable assumption as regards future tests. I would say that it should be assumed that tests will continue at the same rate as in the past -- that is the only safe assumption. It is the great weakness of this chapter ~~is~~ that it really doesn't tell the reader anything that he can hang on to. In all the other chapters we have tried to leave the reader with something tangible -- what a certain number of calories, roentgens, etc., means in terms of an effect on the human being. There is nothing like that here -- just a number 3.3 micromicrocuries per gram! Sure, I can work it out from there, but we must think of the people we are writing for.

Page 34. Paragraph 0.63, last two lines. The cosmic ray doses at sea level and 5000 feet are given here as 37 and 60 mr/year , respectively. In Table 9.42 we give 35 and 50. The difference is not great, but I like consistency!

Fig. 4. The extrapolation beyond June 1956 is terrifying! I wonder if it is correct, and if so how wise it would be to put out anything of this kind. This requires careful consideration.

There must be other things which I haven't seen in this rather quick reading, but I have found enough to make me very unhappy. If the difficulties noted above were clarified, I could reorganize the material into something readable and consistent. If we do have to include it in the book, then what about the treatment on Sr-90 we already have in Chapter IX? We won't need both. I hope it will be possible to persuade the AEC to accept one of the compromises I suggested to General Luedecke. The last resort would be an appendix, separate -- at the end of the main book. But I just don't know whether any compromise will be accepted.

I recall that this started off as a letter, so I will conclude by sending best personal regards from Violette and myself.

Sincerely yours,


Samuel Glasstone