INSTITUTE FOR DEFENSE ANALYSES

Weapons Systems Evaluation Division IDA The Pentagon, Washington 25, D.C.

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Mr. John T. Conway
Executive Director
Joint Committee on Atomic Energy
Congress of the United States
Washington, D. C.

Dear Mr. Conway:

Dr. Dunham has sent me a copy of the comments of the special committee he selected to review my report Iodine-131 in Fresh Milk and Human Thyroids Following a Single Deposition of Nuclear Test Fallout. These comments are to be included as part of the report, to be published as AEC TID-19266. I have no objections to this procedure, since the comments help emphasize the uncertainties in present understanding of the transport and biological uptake of radioactive iodine in fresh, tropospheric fallout. There are, however, some features of the comments which must appear puzzling to the reader unfamiliar with the reason the report was prepared, and uninformed as to the "potential significance" of the report alluded to in the first paragraph of the review committee's comments. Let me clarify these points, and comment on the comments.

On June 15, 1960, Chairman John A. McCone of the Atomic Energy Commission transmitted to Senator Clinton P. Anderson, Chairman of the Joint Committee on Atomic Energy, a report entitled The Contribution of Hot Spots and Short Lived Activities to Radiation Exposure in the United States from Nuclear Test Fallout. This report was prepared by me in considerable haste in response to a request from the Joint Committee growing out of a controversy between the Commission's General Advisory Committee and Dr. Ralph Lapp (see pages 2260-2267 of the Fallout Hearings held by the Joint Committee in 1959). It was a matter of some concern to the Commission at the time the Hot Spot report was sent to the Joint Committee that radiation doses from internally deposited I-131 as high as 2.5 rads could have been delivered to the thyroids of some infants in the St. Louis area for the two-year period ending in April 1959. These doses were not excessive in terms of the annual permissible thyroid dose set for adult radiation workers (30 rads), but they were large in terms of any nuclear test fallout doses from internal emitters then known or suspected. They were, in fact, of the same magnitude as the whole body, external gamma doses delivered to some 20,000 persons living within two-hundred miles of the Nevada Test Site (Hot Spot report, page (v)).

In transmitting the Hot Spot report to the Joint Committee, Chairman McCone noted:

> "The maximum estimated doses to individuals in the hot spot areas are of the order of several rads delivered over a period of weeks or months.

The average doses are in the range referred to by the Federal Radiation Council in its recent memorandum for the President as well below the level where biological damage has been observed in humans. Since this does not mean that no effects will occur, a large part of the Commission's biomedical research program is directed towards developing more precise information on the possible chronic effects of such levels of radiation, ie., accumulated doses of 10 rads or less at low dose rates."

The St. Louis infant thyroid doses estimated in the Hot Spot report were calculated from the levels of I-131 measured in raw milk from a milkshed supplying the St. Louis area. It was noted in the summary of the report that "no information is available on the I-131 content of milk in the Nevada Test Site area for the periods of active testing" (1951-1958).

After the Hot Spot report was transmitted to the Joint Committee, an attempt was made to go through each section critically to see if any changes were needed prior to reproducing the report as an AEC Technical Information Document. This effort was never completed, partly because of interruptions for other more pressing problems, but mostly because the review of the specific sections led to such extensive revisions as to require the preparation of a separate report on that section. Thus, the section on strontium 90 led to TID 13945, The Effect of Deposition Rate and Cumulative Soil Level on the Concentration of Strontium 90 in U.S. Milk and Food Supplies, and the section on external gamma doses led to TID 16457, Gamma Ray Exposure to Non-Urban Populations from the Surface Deposition of Nuclear Test Fallout. Further, it became evident as the effort to complete the comprehensive evaluation continued that many of the ideas about the relative importance of the radioactive nuclides present in fallout as given in the report transmitted to the Joint Committee would have to be substantially revised. Strontium 90 began to seem less important and external gamma radiation more important. The significance of Cesium 137 as an internal and external emitter required reconsideration. It appeared that lung doses from plutonium inhalation and doses to the gastrointestinal tract from gross fission product would have to be considered. Even basic physical data on individual fission product yields and gamma energy radiation rates was undergoing significant revision. All of these factors introduced changes in previous estimates of external radiation doses to the whole body and internal radiation doses to specific organs, and in the average dose to large populations and the above-average doses to special segments of the population.

Special population groups may be characterized by age, diet, geographical location, or some combination of these factors. One cannot say, a priori, whether a given test or series of tests might pose a greater public health hazard in terms of low doses to large numbers of persons, or high doses to relatively few persons. Both possibilities act as constraints on what may be safely tested, and where, and how, and when.

It was with regard to the above-average dose to a selected segment of the population that a rather startling discovery was made during the process of revising the Hot Spot report. To remedy an omission in the original version, an attempt was made to estimate the radiation doses to the thyroids of infants and young children living within several hundred miles of the Nevada Test Site. Calculations of thyroid doses to the children in St. Louis had been based on measured I-131 levels in milk. Yet, while the St. Louis milk levels seemed high (the maximum was 640 μμc/liter, and the average for the year 1958 was 258 μμc/liter), it was noted that the external gamma dose in St. Louis from Nevada fallout amounted to not more than a few hundredths of a rad. Measurements showed the external gamma doses in many communities around the Nevada Test Site (where no measurements of I-131 in milk had been made) were in the range of several rads. Since the external gamma dose and dose rate which occurs after a deposition of fresh fallout constitute a measure of the quantity of fission products deposited on the ground, an obvious thing to do is to determine a relation between the external dose or dose rate and the resulting level of I-131 in fresh milk. The milk level thus calculated from the measured gamma level could be used, as before, to estimate the dose to the thyroid of a child consuming the milk.

The quantitative relation between the external gamma dose rate from fresh fallout and the subsequent maximum level of I-131 in fresh milk is the subject of the report which has just been released by the Commission. The relation derived is supported by the experiments of R. J. Garner, by the measurements of I-131 in milk and on herbage following the Windscale reactor accident, and by direct observations following the SMALL BOY shot at Nevada in the summer of 1962. It was concluded that the maximum level of I-131 in fresh milk, $I_{max}$, following a deposition of fallout on pasturage giving rise to an external gamma dose rate of $r_o$ mr/hr at 24 hours following detonation, usually lies in the range given by

$$I_{max} = (26{,}000\ r_o) \text{ to } (96{,}000\ r_o) \quad \mu\mu c \text{ I-131/liter.}$$

The "potential significance" of this finding may be appreciated by an example. The relatively heavy fallout in the St. George, Utah area on May 19,1953 resulted in a measured $r_o$ of 27 mr/hr. If the relation were valid for this fallout, it would mean that milk levels reached values of from 700,000 to 2,600,000 μμc I-131/liter, and that the associated doses to the thyroid of an infant which drank 1 liter of this milk per day for three weeks following the test would lie in the range of 120 to 440 rads.

In retrospect, the possibility of such high levels of I-131 in Nevada milk following the early tests is not surprising. The Windscale reactor accident in England, in the fall of 1957, released an amount of I-131 equal to that generated in a fission explosion with one-sixth of a kiloton energy release, and led to milk levels as high as 1,300,000 μμc I-131/liter. In Nevada, about 1000 kilotons of fission energy were released before there were reliable measurements for I-131 in milk in the off-site communities and ranches. The circumstances of the Nevada releases were such that only a small fraction of the I-131

created may be presumed to have been deposited in the area within a few hundred miles of the test site. Nonetheless, the fallout patterns observed from past tests indicated that depositions of I-131 per unit area at many inhabited ranches and communities must, on several occasions, have exceeded the maximum I-131 concentrations on pasturage found after the Windscale accident. As explained in the report, it would not necessarily follow from this that milk levels reached or exceeded those noted after Windscale, since the chemical and physical form of the fallout particles were different. But all things considered, it seems to me more difficult to conclude that levels of I-131 in milk comparable to those measured following Windscale did not occur in many places following several of the early tests than it is to conclude that they did occur. In any event, the original purpose of the report just released by the commission was to provide a method for estimating the doses which may have been delivered to the thyroids of children who consumed fresh milk from cows grazing on pastures which experienced relatively high levels of fallout from past nuclear tests.

Let me now comment on the major points made by the special review committee, whose remarks are to be published with the report. The report, as noted above, assumes a relation between the external gamma dose rate at 24 hours post detonation ($r_o$) and the maximum I-131 concentration ($I_{max}$) in the milk of cows grazing in the area of the form

$$I_{max} = k\, r_o ,$$

and concludes that the value of the coefficient k generally lies in the range of 26,000 to 96,000. This conclusion may be questioned in two ways:

(1) Is such a linear relation correct for the fallout in offsite communities 50 miles or more from the test site?

(2) Is the range of values which has been estimated for the variable constant k the correct one?

As far as I can tell from their comments, the Committee does not suggest that the proposed mathematical relation, as qualified in the report, is not an appropriate one, but only that the range of values given for the coefficient k may not be correct for want of a sufficiently sound experimental basis for determining k.

The most unfavorable opinion held by a Committee member is that "the errors in gamma ray field measurements as used by the author are considerably larger than estimated--so large, in fact, that there is no justification for attempting to base a quantitative relationship on the existing data," and that "measurements for gamma radiation as conducted in the field are adequate for the intended purpose of delineating areas of relative dose rate and overall evaluation of external gamma radiation exposure, but are not adequate to provide specific values for the author's model."

I do not think it a meaningful scientific criticism to simply assert that the gamma measurements in Alamo and Caliente

are good for one thing but not for another. It would make sense to assert that they are not reliable to within a factor of two (if that be true), or that they are an upper limit, or a lower limit, or just that they are wholly useless and should not be bothered with. But the fact remains they were made by trained monitors, and they are the only measurements we have, and when one uses them, the range of values of k which result is remarkably consistent with the range developed from the Windscale data, and with the measurements made in Northeast Utah. All this agreement may be fortuitous, but right now it's the best estimate we can make. Considering that the committee member who is "completely negative" with regard to the nature and utility of the report happens also to be the person in charge of the Commission's Off-Site Radiological Safety Organization, and thereby responsible both for an accurate appraisal of gamma levels and the levels of I-131 in milk, I would say that to a considerable extent, the burden of proof is on him to either produce a better estimate of the range of values for k, better supported than the present report, or to admit that he has no knowledge of and no way of estimating the levels of I-131 which may have resulted from past tests. He might also give a precise, quantitative, statement of what we know and do not know about the measurements of external gamma levels which have been and are being made in the off-site area. In preparing the report, I went to Alamo and Caliente with the person in direct charge of the field monitors to assure myself that the readings reported in the paper were interpreted as accurately and carefully as possible. If there is more information available than is given in the report, I should like to know about it.

I take slight exception to the Committee's comment concerning the highest milk levels in Northeast Utah following SMALL BOY. It is not correct to say that <u>two</u>--or strictly speaking <u>any</u>--of the methods of reasoning used to deduce that milk levels reached 100,000 μμc I-131/liter are based on an $r_o$ of 4 mr/hr. One of the methods--that of applying the Alamo-Caliente results to the aerial survey measurements of gamma levels, requires that the aerial results be correct in an absolute sense. An $r_o$ of 4 mr/hr then gives milk levels in the range of 104,000 μμc I-131/liter to 384,000 μμc I-131/liter, provided there were cows on pasture in the area where the highest gamma levels occurred. Another method only requires that the gamma levels at some pastures in the hot spot be ten times greater than they were at Snyderville. Such differences were observed in both the aerial and surface gamma surveys. This would mean an $r_o$ of 1.5 mr/hr, provided the estimated gamma levels at Oakly, Kansas, and Snyderville are correct. However, it is a less stringent requirement on either the Army's surface monitoring, or the aerial survey, that the <u>relative</u> readings be correct than it is that the absolute gamma levels be correct. The third method has, to be sure, all the uncertainties of back-extrapolations in time. The extrapolation used, however, is the best one that can be made from the data, and it has an equal probability of underestimating, as well as overestimating the levels of I-131 in milk at earlier times.

Considerable effort was made by myself, with the critical and constructive assistance of Mr. Joshua Holland, Chief of the Fallout Studies Branch, Division of Biology and Medicine, U.S. Atomic Energy Commission, to state the probable and possible maximum levels of I-131 in milk in the area around Altonah and Fruitland with as great care as possible. Perhaps the report should have stated that milk levels of 100,000 μμc I-131/liter could have been reached. But we felt it justified to state (page 64) that "milk from some farms in the general vicinity of Altonah rose to 100,000 μμc I-131 on or about July 19, 1962," that "milk levels probably reached 200,000 μμc I-131/liter at some locations," and that "one cannot rule out the possibility that milk levels as high as 400,000 μμc I-131/liter could have occurred in the 20-square mile area near Fruitland, Utah, where the aerial survey indicates the H+24 hour external gamma dose rate to have been 4 mr/hr.". I would not wish to change this result unless there is further evidence to indicate these conclusions are misleading. To be sure there are uncertainties, but one has to be willing to let the evidence lead where it leads, no matter how disturbing the result. It is not that I object to the Committee's emphasis of the uncertainties. The careful reader has the benefit of both the report and the Committee's caution to lead him to his own conclusion, but the question of the highest credible level of I-131 in milk in Northeast Utah following SMALL BOY is a legitimate one, and the only criteria used in the report was that of the best deduction from the available evidence.

No attempt was made to treat the estimates of $I_o$ and $I_{max}$ at Alamo and Caliente (Figures 8 and 9) because the estimated variations in these values (± 20%) is considerably smaller than the estimated variations in the estimate of $r_o$ (a factor of two in either direction--see Table I). Treatment of the errors in the estimates of $I_{max}$ and $I_o$ would be useful, however, in indicating the range of uncertainty of the final estimate of the milk level resulting from a given gamma level. The upper limit would be increased, and the lower limit decreased. I would be inclined to let the best present estimate of the ratio $\frac{I_{max}}{r_o}$ stay in the general range of 26,000 to 96,000 μμc/liter per mr/hr at 24 hours post detonation.

The Committee's observation concerning the first conclusion on page 45 of the report that the data "show that in about 50 percent of the cases, the calculation overestimates the observed thyroid burden by more than a factor of 2, and that in about 35 percent of the cases the calculation overestimates the observed thyroid burden by more than a factor of 3 (Figure 19)" is a good one. I would not think it necessarily true, however, that the somewhat better comparison based on the assumption of 1 liter intake per day "merely reflects the operation of compensating errors." It may only mean that some people who estimate that they drink 3 liters of milk per day really drink nearer 1 liter, or that part of their milk was from a source lower in I-131 content than the fresh milk from their family farm. I don't object to assuming a different uptake of iodine by the thyroid gland than the 30 percent used by the Federal Radiation Council and the ICRP. It is not clear, however,

whether the Committee meant that the calculations should have been altered. If 20 percent I-131 uptake by the thyroid is more correct for infants and young children, then the appropriate references should be cited, the calculations should be altered to reflect this new value, and the Federal Radiation Council should announce a change in the basis for its computations.

Finally, it is true as the Committee observes, that the author, because of lack of adequate data, is "forced into the unfortunate position of having to develop the two parts of the report's objective from different sets of data each of which is derived largely from assumptions and/or extrapolations." But unless someone can either produce valid measurements of I-131 in milk in the path of fresh fallout from the early Nevada tests, or develop a better model than the one proposed, or give a valid reason why no attempt should be made at this time to estimate the levels of I-131 which may have occurred in fresh milk in the off-site communities, this is also the unfortunate position of theAtomic Energy Commission.

Thank you for your invitation to comment on the problem of radioactive iodine from Nevada tests.

Sincerely yours,

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This letter is reprinted on pages 1078-1082, of the Hearings before the Subcommittee on Research, Development, and Radiation of the Joint Committee on Atomic Energy, Congress of the United States, on August 20, 21, 22, and 27, 1963 (Part 2). The Hearings also contain the whole report, and the comments of the Committee established by the Atomic Energy Commission to review the report.