UNITED STATES GOVERNMENT
MEMORANDUM

TO : N. H. Woodruff, Director
Division of Operational Safety

DATE: June 14, 1963

FROM : Gordon M. Dunning, Deputy Director
Division of Operational Safety

SUBJECT: COMMENTS ON "IODINE-131 IN FRESH MILK AND HUMAN THYROIDS FOLLOWING A SINGLE DEPOSITION OF NUCLEAR TEST FALLOUT," BY DR. HAROLD KNAPP

OS:DIR:GMD

This is in reply to Dr. Dunham's request for our comments on "Iodine-131 in Fresh Milk and Human Thyroids Following a Single Deposition of Nuclear Test Fallout."

The paper is written in better form than previous drafts.

(NOTE: COMMENTS ON THESE COMMENTS HAVE BEEN INDENTED.)

The report transmitted is not a draft. It is a formal, complete document, ready for Commission action. Three weeks of review and discussion were spent with Mr. Joshua Z. Holland, Chief, Fallout Studies Branch, to place the report in its final form. Other review procedures are described in the acknowledgement section (Report, pages 67-68) and in the letter of transmittal to Dr. English dated June 6, 1963. Mr. Holland was the last person to review the mats before they were sent for reproduction. I am, of course, pleased to correct or change any portions of the report relating to form or substance provided there are valid substantive scientific or editorial reasons for so doing.

It still retains, however, the same basic technical uncertainties. Therefore, it has the same objections. Since these points have been discussed in great detail before

there is no useful purpose in repeating all of them here. But to insure that it is clear what is meant, two examples will be mentioned:

What technical uncertainties?

Whose objections?

Where are they discussed in great detail?

Firstly, one of the principal bases for the author's conclusions is attempting to relate one set of measurements (gamma dose rate measurements) to an entirely different parameter (iodine-131 in milk). Such attempted relationship is quite unreliable for many reasons but in addition he develops this relationship on only two points (Alamo and Caliente).

All of science involves relating one set of measurements to an entirely different set of measurements. All of the difficulties, conceptual and practical, of relating the fallout contamination level in terms of the external gamma dose rate at H+24 hours to the resulting level of I-131 in milk are discussed in the report in detail (pages 1-10, 51-56). Ironically, the credit for this approach goes to Dr. Dunning, who has used it both in material for the public (1959 JCAE Hearings on Nuclear War, page 443 et. seq.) and in estimates of the biological and environmental effects of thermonuclear war, prepared for the President of the United States. From Dr. Dunning's memorandum of July 25, 1962, to Ink, Bloch, Woodruff, Dunham, Western, Clark, Betts and Kelly, subject Recent Public Health Service Data on I-131 in Milk, one may infer that it simply never occurred to him to apply the same relations he had derived to evaluate the I-131 hazard of a nuclear war to

the fallout from Nevada tests. In the second paragraph of the July 25 memorandum, Dr. Dunning states:

> "The high value at Caliente, Nevada (6900 μμc/liter on July 19) probably was from SMALL BOY.1/ This latter value seems high, especially considering the low external dose rate measured (in the low mr/hr range). But then the Salt Lake City values are also high since there was essentially little or no rise in external radiation levels there. These data illustrate our need to know much more about the iodine problem"

Dr. Dunning's JCAE testimony noted above states 1 μc/I-121 meter$^2$ → 100,000 μμc I-131/liter milk. The Caliente milk gamma relations suggest 1 μc I-131/meter$^2$ → 96,000 I-131/liter milk.

Thr reason that the report concentrated on the milk-gamma relations at Alamo and Caliente, Nevada, was that these were the two locations for which the best data were available, and the Special Review Committee suggested to me that I should concentrate on the best data.

Further, the author estimates that the range of ratios (gamma readings to iodine in milk) for just these two points is greater than a factor of 10 (23,000-290,000).

For both the Windscale and the Alamo-Caliente fallout data, the estimated <u>limits</u> of the milk/gamma

1/ See report, Figure 9, page 26. The value 6900 μμc/liter was noted on July 19, 1963, and the next measured value was 3500 μμc/liter on July 23. When the Off-Site Radiological Safety Organization issued its Interim Off-Site Report of the SMALL BOY Event, November 8, 1962, it forgot to include the 6900 μμc/liter figure.

ratio differ by a factor of somewhat more than 10, with the best estimate varying by a factor of 4. This large variation is due partly to uncertainties in measurement, and partly to the large variety of factors which can influence the level of I-131 in milk following deposition on pasture of a given amount of this nuclide in fallout. It's admittedly too bad not to know the relation more precisely, but on the other hand, before the paper was written, we didn't know it at all -- except for Dr. Dunning's theoretical calculations based on the Windscale data; yet now there is some experimental verification for his earlier calculations, he insists they are not true.

This is scarcely adequate ground for such an overall generalized conclusion that he applied to other situations!

What isn't adequate for what generalized conclusion applied to what?

(It is noted that measurements for additional locations used in a previous draft have now been downgraded to a position of "corroborating the relationships observed at Alamo and Caliente" for SMALL BOY and that similar uncertain measurements for Sedan and Des Moines have been eliminated. Will another evaluation eliminate the final two?)

As noted above, the concentration on the SMALL BOY data was done at the suggestion of the Special Committee because it was the best data. Also, the discussion in the March 1, 1963 report concerning the I-131 levels in milk following SEDAN and

DES MOINES are quite embarrassing to the Commission, and specifically to the Division of Operational Safety. The discussion of the I-131 measurements at the Blue Eagle Ranch (pages 49-66 of the March 1, 1963 report) following DES MOINES should cause Dr. Dunning the gravest concern. If he would like this material reintroduced into the present, report, I would be very pleased to put it back.

The measurements were taken in a low radiation field--some even less than one milliroentgen per hour - where instrument response is least accurate. These data were than extrapolated in time by a relationship of $t^{-1.2}$ - a procedure that can give rough estimates of decay of a radiation field but not to a degree of accuracy to allow establishing a quantitative relationship as was attempted in the paper.

The burden of proof is on Dr. Dunning to back up this last sentence. The paper contains a discussion of the accuracy of the procedures used on pages 5 and 6, and on pages 22 through 29.

For the two locations (Alamo and Caliente) the author indicates that there are inherent errors in the assumptions used since, among other things, the two different sets of measurements (gamma readings and iodine in milk) were not at the same place. Although the places may not be widely separated, even relatively small distances can give substantially different gamma readings, especially when they are on the gradient of a fallout pattern and where the levels were as low as these were. I have measured variations of factors of 5-10 within distances of a few hundred yards in nearby fallout area.

If Dr. Dunning has some referenceable data bearing on these variations as they may reasonably apply to

Alamo and Caliente, let him produce it. This problem is explicitly recognized in the report (page 25, paragraph 3), and everything was done in the preparation of the paper to check with the most competent and responsible monitors, e.g., Mr. John S. Coogan, Chief, AEC Special Projects Section, Public Health Service Off-Site Radiological Safety Program, to determine the most reasonable interpretation of the existing data.

Measurements taken along a highway, as were many of those used in the paper, are especially variable, as contrasted with a hundred yards or so off the highway. Other ground measurements were taken within a community or near populated buildings since these were the points of primary interest to the personnel monitors. Extrapolation of these readings to a pasture down the road are quite uncertain.

I specifically went to Alamo and Caliente, right to the places where the readings were made, talked with the monitors, and did everything I could to assure myself that the readings, as used in the paper, were interpreted as accurately and carefully as possible. The pastures in question border the road where the readings were taken.

For four of the five other localities given in Table III the extrapolation is from aerial survey data, thus introducing an even greater unreliability. It is not possible to fly at the prescribed 500 feet altitude in that type of terrain and uncertainties are introduced both by different distances from a contaminated surface and by variation of the instrument response by changing barometric pressure with altitude. Also, it has been found that the aerial readings have been due to gases

and finely suspended particles in low-lying terrain, and and when ground monitors entered the area they did not find that the surface deposition recorded these levels. Aerial surveys under the conditions of SMALL BOY can yield useful information as to general outlines of fall-out but their use with such precision as attempted in the paper, constitute an unwarranted extrapolation. For the fifth location, Kamas, Utah, the ground survey was conducted four days after SMALL BOY (which is 0.02 to 0.03 mr/hr in these areas) of only 0.04 to 0.07 mr/hr within five miles of Kamas. The extrapolation of such low levels back to H + 24 hours at a pasture somewhere around Kamas is most uncertain.

Is it Dr. Dunning's contention that the aerial survey measurements and the ground measurements are too high, or too low, and how much too high or too low? Why, when the Division of Operational Safety had a hot spot it didn't expect, didn't it find out what the gamma levels were? All one can say is that some measurements were made and the results were reported and used in a straightforward fashion, and all the rough pieces of data fit together very well and consistently to give exactly the levels of I-131 in milk that Dr. Dunning would have estimated on the basis of his own calculations (JCAE hearings on the Biological and Environmental Effects of Nuclear War, page 443 et. seq.). It looks mightily silly to suddenly become so doubtful just because when the theory is applied, we suddenly realize we have all been missing the obvious in a haystack for over 10 years, and that the public relations impact of this oversight are painful to ponder.

Secondly, most of the key data on milk intake are derived from a few "grab" milk samples, even a single "grab" sample followed by brashly extrapolating the data.

Is one supposed to believe that the key milk data on page 24 (Figure 8, I-131 levels in milk from Alamo, Nevada) and page 26 (Figure 9, I-131 levels in milk from Caliente, Nevada) constitute a few grab samples? And if the milk from the Altonah area of Northeast Utah constitutes a few grab samples, how do we know we grabbed the highest samples? Why is a 5-day half-life a "brash" extrapolation of the data?

Whether or not the conclusions of the paper are true may be debatable - this paper does not provide the scientific basis for a decision one way or another.

I would be pleased to let the scientific community be the judge of this assertion. The only comment I have seen so far from the Special Review Committee is that sent to Dr. Dunham by Dr. Julian Nielson, Manager, Radiological Chemistry, Hanford Laboratories, who stated:

> "The report is objective and states the facts clearly. I was very impressed by the comparison of the Windscale incident and SMALL BOY test data.
>
> This report should stimulate much interest and investigation because it so clearly points out areas requiring further study. It seems to me to be well worthy of publication."

Although it was agreed extrapolation to past tests would not be done, it is noted that on pages 55,

65 and 66 just enough is said to raise the issue and whet the reader's appetite. It is as effective as if a more complete discussion were included.

The I-131 doses to the thyroids of infants and children as the result of testing in Nevada since 1951 constitute a very legitimate problem which the Commission has to be prepared to face. I can appreciate Dr. Dunning's concern for the past, but the Atomic Energy Commission simply has no choice but to be honest and candid as to what is known and not known about what may have happened. This entire report on radio-iodine arose when I was trying to put the finishing touches on a report on Radiation Exposure in the United States from Nuclear Test Fallout and discovered at the last minute that the most important problems concerning the most important nuclide hadn't been though about.

It is also to be noted, for example, that the report states " - - - one cannot rule out the possibility thatmmilk levels as high as 400,000 μμ c I-131/liter could have occurred - - -." Aside from the unsound technical basis for this conclusion, we may expect someone to point out that a single quart of milk contained a 10 year's allowance of iodine-1-131. (FRC's 36,500 per year times 10 years.)

That we may.

There are two basic problems involved, one, the questionable technical validity (which has been discussed at great length in previous meetings), and two, a policy question of the Commission publishing the

paper. In response to a question asked as to the motive for publishing the paper, a member (not the author) of the Commission's staff said, "The Commission has been telling the world for years that it has been conducting its operations safely, now it appears that this may not be so". If a member of the staff says this about the paper, what reaction may we expect from the press and the public?

I expect somebody (not the author) might want to hang Gordon Dunning from a sour apple tree.

Just last week at the Congressional Hearings, Senator Bartlett essentially accused the Commission of irresponsibility in conducting its operations in Nevada. A reply to refute this allegation is being prepared for the JCAE.

Presumably, by the Division of Operational Safety. I trust they will include the earlier safety standards as given on pages 213 and 233 of the 1957 JCAE Hearings on The Nature of Radioactive Fallout and its Effects on Man. One standard in those days was a 30 r whole body, external gamma dose before evacuation. Another, by implication, was a level of I-131 in milk for safe, continuous consumption of $1.8 \times 10^5$ $\mu\mu$c/liter, or 2,000 rads/year to the thyroid of a 1 year old child consuming 1 liter of milk/day. (This follows from the safety criteria of 5 $\mu$c/liter at H+3 days for safe, continuous consumption of fission products in water. See also Enclosure (2) to this memo.)

Through meetings and exchange of letters between the author and others outside the Commission, the paper

probably is well known. The author spent several days traveling and talking with many people around the Nevada Test Site, followed by exchange of letters with them. This has just goaded them on. We have spent years of hard, patient effort to establish good and calm relations with the public around NTS. Such action as the author's has been harmful.

Consider the author's problem. When he went to Dr. Dunning to ask if there were any data at Alamo and Caliente which could be used to see if the Windscale results could be applied to nuclear test fallout, he was told there were no such data. A second inquiry produced the same response. Then it developed that Dr. Dunning's office was the only one which had the reports from the Off-Site Radiological Safety Organizations, from which alo the data on the Alamo-Caliente milk-gamma relations derive. An inquiry about the high Northeast Utah milk levels from Dr. Dunning produced nothing. A great deal of information was given to me for the asking from the University of Utah and the Utah Health Department. Some interesting milk samples sent by the Utah Department of Health to the AEC's Off-Site Radiological Safety Organization got lost (the details are recorded in a letter to me from Dr. Grant S. Winn, Head, Industrial Hygiene Section, Utah Department of Health, attached as Enclosure (1) to these comments). Dr. Winn has told me the Utah Health Department has found it necessary to build duplicate monitoring

facilities beside those of the Atomic Energy Commission's Off-Site Radiological Safety Organization, because data pertaining to the safety of the citizens of Utah was not forthcoming from the AEC.

Last summer and fall when a previous paper by the same author was being readied for publication, some of the staff raised serious questions about its validity.

It turned out that the objections of Dr. Dunning, Dr. Tompkins, and Dr. Dunham were found to be not valid.

The argument again was used that we had to get into print before Dr. Ralph Lapp. Dr. Lapp did publish in Science, 7 September 1962 about the Troy, New York incident. We took this problem on, as we have many before, and by fine cooperation of the U.S. Public Health Service and the New York State Department of Health, a reply was given by Dr. Frederick W. Lengemann of the New York State Health Department in the 7 December 1962 issue of Science.

A reply was given, but no refutation of the fallout level, milk level relation derived by Dr. Lapp was given. Dr. Lapp evidently based his calculations on Dr. Dunning's work. He states that, "A pasture level of 1 $\mu$ c/$m^2$ may be translated into a milk contamination level of 100,000 $\mu\mu$ c liter." This is the conversion factor used by Dr. Dunning in the 1959 UCAC hearings on the Biological and Environmental Effects of Nuclear War (page 445,

line 4). The estimate of the present report is that 1 μc I-131/meter$^2$ → 26,000 to 96,000 μμ c I-131/liter of milk.

But we are in a dilemma. An ad hoc committee reviewed the paper and, although I have not seen a statement of their conclusions, apparently they felt that the paper (with editorial rewriting) could be published. One might argue that the committee was not made fully aware of the pitfalls inherent in the treatment given the field data. But the fact remains, they did not turn thumbs down. Now what we do?

Dunning

I wish I had a brilliant answer to the question. I do not. There is this much to be said. Since this paper, right or wrong, can have a major impact on the Commission, it should be brought before the General Manager and Commissioners.

I agree that the paper should be brought before the General Manager and the Commission, and it is my understanding that this will happen in the near future.

Knapp

Secondly, before the paper is published (if this is decided) sufficient time should be given to prepare a fully documented report as to what measures were and are being taken to conduct a safe operation in Nevada.

Dunning

If the Commission hasn't by this time a fully documented report as to what measures were and are being taken to conduct a safe operation in Nevada, one wonders if it will ever get one. And how does Dr. Dunning propose to show that the operations in, say, St. George, Utah, were safe at the time of the HARRY shot in 1953, since he

Knapp

etc

has no valid measurements of the I-131 levels in milk there. This is a technical report and should be released promptly and without a great attempt to manage the news. There has been ample opportunity for Dr. Dunning since last summer to get ready for a crisis over radio-iodine.

Admittedly, this is not a panacea, since it is difficult to see how the Commission can "admit" to dire deeds and in the next breath say we are going to keep on doing the operations. Thirdly, through the cooperation of the U.S. Public Health Service, steps are being taken similar to those for Troy, New York to see if there are vital statistics for the Nevada-Utah area. This should be ready before publication of the document (if it is published).

Dr. Dunning has had a whole year to look for this data. I will object most strenuously to any further delay on this account.

Since writing the above, there has come to mind one possible "out" to the situation. Let the Commission tell Dr. Knapp in a matter-of-fact and bland manner that the Commission interposes no objection if he, as an individual scientist, wishes to publish his paper.

It wouldn't work, Dr. Knapp might find out about this suggestion and go up in smoke. He is said to be mighty sensitive by now to this type of scheming.

If Dr. Knapp can find a reputable scientific journal to accept the manuscript, we may expect letters and inquiries. We can then treat these in the same professional and unemotional way that we have many others in the past. By doing so, we can make it clear that the Commission has been conducting its operations in

a responsible manner and that the Commission has a highly competent technical staff that has considered Dr. Knapp's ideas.

A highly competent, thoroughly impartial, technical staff named Gordon M. Dunning.

The above approach will make it clear that the Commission is not trying to suppress the paper.

No comment.

At the same time it does not place the Commission in the untenable position of condemning itself, as it would if it were to sponsor its publication as an AEC TID report.

No comment.

I do not recommend another approach of publishing the paper as an AEC document with a disclaimer by the Commission. This approach would not only sound as though we were talking with a forked-tongue but would add dignity and prestige to the report.

We want to maintain the standards of dignity and prestige set by paragraph 3 of Dr. Dunning's pre-SMALL BOY press release (AEC release No. E-238, July 5, 1962 all three paragraphs of which are quoted below).

"ABOVE GROUND NUCLEAR DETONATION SCHEDULED AT NEVADA TEST SITE

"The Department of Defense and the Atomic Energy Commission announce that a detonation of a low-yield nuclear device a few feet above ground has been scheduled at the Atomic Energy Commission's Nevada Test Site on or after July 7.

> The detonation has been authorized so the Department of Defense can conduct varied experiments to analyze the effects of nuclear explosions.
>
> Most of the radioactive particles produced by the detonation are expected to fall back to earth within a few miles, inside the Las Vegas Bombing and Gunnery Range. Radiological monitoring will be conducted at distances up to 300 miles from the shot point to obtain information on the limits of detectability of the radioactivity." (Emphasis added. Altona, Utah is 400 miles from the location where SMALL BOY was detonated. Milk levels there reached 100,000 to 200,000 μμc I-131/liter.)

There is also the matter of Dr. Dunning's report on Foods and Fallout, Vol. 23, No. 1, January-March 1962 issue of Borden's Review of Nutrition Research, page 13.

Conclusions:

> "Such calculations as have been presented are not precise; neither are the data upon which they are based. Furthermore, any future atmospheric tests would result in additional amounts of radiation exposures. Despite these uncertainties, the data and their evaluations clearly indicate that nuclear weapons testing over a span of ten years has not resulted in radiation exposures approaching the Federal Radiation Council Guides established for normal peacetime operations."

The FRC guides for normal peacetime operations indicate an annual thyroid dose of 0.5 rads to "a suitable sample of the population" and 1.5 rads to individuals. As noted in Conclusion #3 of my March 1, 1963 report -- which has not been changed by the June 1, 1963 version, even though it is not stated,

3. Earlier Nevada test series involving tower or balloon shots of devices in the 5 - 75 KT range frequently gave rise to external gamma fallout levels several times higher than those measured outside the test site area following DES MOINES, SEDAN, and SMALL BOY. It is not known in what way the physical, chemical and radiological properties of the fallout particles from these shots may have differed from those of the surface and underground 1962 tests. In particular, it is not known how differences in the fallout particles may have affected the ratio $\frac{I_{max}}{r_o}$. If the same conversion factors given in 1. above apply to fallout from earlier series, then it would follow that correspondingly higher levels of I 131 would have occurred in milk. For example, subsequent to the firing of the 32 KT HARRY shot on a 300 foot tower on May 19, 1953, an open field dose rate of 16 mr/hr at H+36 hours ($r_o$ = 27 mr/hr) was reported for St. George, Utah, 125 miles from ground zero. If this is assumed to be representative of pastures in the St. George area, and if the milk-external gamma relations derived from the 1962 tests are valid for this case, it would follow that fresh milk in the St. George area reached I-131 values in the range of from 675,000 to 2,700,000 μμc/liter. The associated doses to an infant thyroid would be in the range of 76 to 380 rads assuming the consumption of 1 liter of fresh milk per day and 30% uptake of ingested I-131 by the thyroid gland."

In retrospect, milk levels in the range of 1,000,000 μμc I-131/liter should not be surprising. The Windscale reactor accident -- which caused a national emergency in England in 1957 because of radioiodine in milk -- caused milk levels to reach 1,300,000 μμc I-131/liter when 0.16 kilotons equivalent of I-131 were released. At the Nevada test site, over 1000 kilotons equivalent of I-131 were released before we obtained any

reliable data on the I-131 levels in milk in the Off-Site communities following fallout deposition from specific shots. As far as I can tell, there must have been the equivalent of at least several Windscale-like incidents around the Nevada Test Site which the Commission never knew about.