UNITED STATES ATOMIC ENERGY COMMISSION

IN THE MATTER OF:

TOLEDO EDISON COMPANY
and
THE CLEVELAND ELECTRIC ILLUMINATING COMPANY

(Davis-Besse Nuclear Power Station, Unit No. 1)

Docket No. 50-346

Place - Port Clinton, Ohio

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UNITED STATES OF AMERICA

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(Davis-Besse Nuclear Power Station, Unit No. 1)	

Trinity Methodist Church
Conference Room
Adams and Second Street
Port Clinton, Ohio

Tuesday, 3 February 1971

The above-entitled matter came on for further hearing, pursuant to notice, at 9:00 a.m.:

BEFORE:

WALTER SKALLERUP, JR., esq., Chairman,
Atomic Safety and Licensing Board.

DR. CHARLES E. WINTERS, Member.

DR. WALTER H. JORDAN, Member.

APPEARANCES:

(As heretofore noted.)

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PROCEEDINGS

CHAIRMAN SKALLERUP: The hearing will please come to order. We notice that Mrs. Bleicher is not present. Miss Evans do you have any comment to make?

MISS EVANS: Mrs. Bleicher is not able to attend today, but she will attend tomorrow on our behalf.

CHAIRMAN SKALLERUP: At ten o'clock last night I had a phone call from Mr. Lau who said that it would be convenient for him and his witnesses he thought to meet Wednesday evening. Mr. Lau expects to see his doctor this morning and thought he would be able to drop by the hearing this morning to confirm this arrangement.

At the present time efforts are being made to find a suitable room to hold the evening meeting, inasmuch as this room will be occupied and we will have to break tomorrow at about 4:15 in the afternoon so preparations can be made for the evening affair here. We will be able to meet here the following day at this point in time.

I understand you have a communication from Mr. Baron.

MR. ENGLEHARDT: Yes, sir, Mr. Chairman.

Last evening Mr. Russell Baron, counsel representing the Coalition telephoned me to find out what the status of the hearing was. He informed me that he was committed this morning to some professional matters and would

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be unable to be here this morning, but he would make every effort to be here this afternoon. I indicated to him that it was likely that the hearing would reconvene at 2:00 p.m. in accordance with what has appeared to be the customary procedure. He indicated he would make every effort to be here this afternoon.

CHAIRMAN SKALLERUP: Do you care to make any comment or statement, Mr. Charnoff?

MR. CHARNOFF: Only that I did understand yesterday from Mrs. Bleicher that LIFE would be prepared to proceed with cross examination of our witnesses on rebuttal this afternoon. And I assume that is still the case in the person of Vicki Evans if not in the person of Mrs. Bleicher. And as I recall our phone conversation yesterday at noontime with Mr. Baron, he was also planning to be prepared to cross examine us this afternoon.

CHAIRMAN SKALLERUP: Is that your understanding?

MISS EVANS: Yes, it is. And I will try to cross examine as far as I can go. And Mrs. Bleicher will resume tomorrow.

CHAIRMAN SKALLERUP: Dr. Jordan informs me that he has a question he would like to ask the witness.

MR. ENGLEHARDT: We are also prepared to present a clarifying statement with regard to the record of yesterday. So we can begin again with Mr. Rogers.

Whereupon,

IESTER ROGERS

resumed the stand as a witness on behalf of the Regulatory Staff and, having been previously duly sworn, was examined and testified further as follows:

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Dr. Jordan, did you want to raise your question or shall we proceed to clarify?

DR. JORDAN: My question has to do with the factor of 700. Is your clarifying statement with respect to that? You testified yesterday afternoon that in the case of iodine-131 and particulates there would be a factor of 700 applied. Indeed the power plant effluents would be held to a factor of 700 lower in the case of those isotopes than is shown in table 2 of 10 CFR 20.

Now my question is: What isotopes do the particulates include; namely, such things as cesium and strontium? And, secondly, how does the factor of 700 get applied? Is it in the tech specs or how?

WITNESS ROGERS: The factor of 700 is applied to particulate radioactivity with a half-life greater than 8 days. Now this would include any releases of cesium, strontium-90, and practically all other radionuclides other than noble gases would be included.

The way this is actually factored into deriving the tech spec limits is that the air concentration at the boundary based on the Appendix B part20 values for unrestricted areas, those concentrations or air concentrations are in fact reduced by a factor of 700 in deriving the release rate for the iodines and particulates with a half-life greater than 8 days which is applied at the stack.

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DR. JORDAN: Yes, I understand. Therefore, from the known meteorological conditions at the stack you calculate how much iodine and the other particulates can be released from the stack in order to have a concentration at the boundary thatis a factor of 700 underneath those given in table 2. And this appears then as a technical specification for the plant; is this correct?

WITNESS ROGERS: Within the release rate, it is factored into the rate. You will not find a factor of 700 stated as such in the tech spec, but it is factored into the release rate that is derived for the iodines and the particulates with a half life.

DR. JORDAN: Okay.

MR. ENGELHARDT: May the record show that Mr. Howe of the Division of Reactor Licensing will now respond.

MR. HOWE: The factor of 700 is shown as a technical basis for the technical specifications and described in the manner in which the computations are made and how the factor of 700 is entered into this computation.

The factor of 700 only appears in the basis as part of the description for the mathematical technique used to derive the values that are set forth in the actual technical specifications.

DR. JORDAN: But the values that are set forth, do they give a release rate in curies per day, say, of

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iodine?

MR. HOWE: It gives it in the form of curies per second.

MR. ENGELHARDT: At page 1755 of the transcript which was at the close of yesterday's session there was an exchange between Mr. Rogers and the chairman of the Board appearing on lines 17 through 22 with respect to the practice with regard to averaging out the releases.

Mr. Rogers would like to clarify his response in connection with the Chairman's statement or question at line 20 on transcript page 1755.

WITNESS ROGERS: The provisions of Part 2106 generally permit concentrations for radioactive material released to unrestricted areas to be averaged over a period not greater than one year. As a practical matter licensed nuclear facilities are designed and operated in such a way that releases of effluents to unrestricted areas and exposures offsite are spread reasonably uniformly over the year.

The general provisions of the Part 20 regulation apply to broad and varied categories of licensing activities. However, in the application of the provisions of Part 20 to limiting releases of radioactivity and effluents in nuclear power reactors, the technical specifications which control the operation of the reactor and are included as

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part of the operating license further restrict the concentrations or quantities of radioactivity that are permitted to be released over a short period of time. For example, technical specifications in operating licenses have provided that for gaseous effluents the maximum release rate over any period of 15 minutes shall not exceed 10 times the average release limit.

Current practice is to generally limit maximum concentration or release rates at any time to the annual average release limits. These provisions make it unlikely that an individual near the site boundary would receive more than a very small fraction of the annual limit of 500 millirem in a short period of time.

Technical specifications also require that releases of radioactivity and effluents be kept as low as pratical. Implementation of this provision will provide reasonable assurance that actual releases will generally be small percentages of the tech spec release limits.

CHAIRMAN SKALLERUP The Board will go off the record for a moment.

(Discussion off the record.)

CHAIRMAN SKALLERUP: Back on the record.

DR. JORDAN: Mr. Rogers, I understand that in applying 10 CFR 20 there really are several provisions of 10 CFR 20 and you don't always pick the same provision.

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Sometimes you say well it is a concentration limit. Sometimes you say it is going to be 500 millirems to the man at theborder. Sometimes you say it is the 170 millirem to the population at large, or a representative population near the boundary. And other times we say well there is a factor of 700 going to apply to certain isotopes. Presumably this is done because we are meeting some other part of 10 CFR 20, so I feel that 10 CFR 20 is not just a single, simple specification. And I think it would be helpful to the Board if you would take time, not try to do it right now, but prepare a statement for the record as to how you know which part of Part 20 to apply and when. And I believe it would clear things up considerably if you would try to do that this afternoon.

WITNESS ROGERS: I will be glad to do that.

DR. JORDAN: Do you understand the question?

WITNESS ROGERS: Yes, sir.

DR. JORDAN: Okay.

MR. ENGELHARDT: Mr. Chairman, I believe that completes for the moment Mr. Rogers' testimony. We have some plans to present approximately six additional witnesses, none of whom have been previously here to offer testimony. I think it might facilitate matters if I were to call upon all of those witnesses to appear now to be sworn by the Chairman so that we can then call them as necessary and don't

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have to worry about it later. So at this time I would like to ask Dr. Tompkins, who is here to my right and Dr. Kahn, Mrs. Tompkins, Dr. Daniel Nelson, Dr. A. K. Davis and Dr. William Bibb to come forward.

We have one other witness who is not here yet, but we will hear him when he arrives.

CHAIRMAN SKALLERUP: Dr. Nelson and Dr. Bibb have been sworn.

MR. ENGELHARDT: They were interrogators. THey were not sworn at that time.

I believe we have everyone here now with the one exception. And this gentleman will be with us a little later and we will handle him separately.

Thereupon,

PAUL TOMPKINS,

DANIEL NELSON,

WILLIAM BIBB,

A. K. DAVIS,

EDYTHALENA TOMPKINS, and

BERND KAHN

were called as witnesses on behalf of the Regulatory Staff and, having been first duly sworn were examined and testified as follows:

MR. ENGELHARDT: I would like to call Dr. Paul Tompkins as the first witness.

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CHAIRMAN SKALLERUP: Would it be a convenience to you to have the witnesses up at the table?

MR. ENGELHARDT: We will, as we develop this case. Dr. Tompkins and some of the witnesses have a particular area that they will speak to. And when we begin the cross examination then we will bring all the witnesses back. The program as we envision it with regard to the presentation of these follow-on witnesses is to have the witnesses present their testimony first and then to have them available as a group for cross examination by members of the Board and the parties.

MISS EVANS: I just have a comment. Yesterday upon receipt of the testimony from Mr. Engelhardt I did not receive Mr. Tompkins testimony.

MR. ENGELHARDT: That is correct. I think I mentioned to Mrs. Bleicher that we had only the three pieces of testimony that we were able to give her in prepared form. I think I see some of the material in front of you now.

MISS EVANS: Do you have Dr. Tompkins outline?

MR. ENGELHARDT: No, we do not.

CHAIRMAN SKALLERUP: I believe in colloquy I had with Mrs. Bleicher the understanding was that she would be given sufficient time to prepare cross examination on these witnesses.

MISS EVANS: Thank you.

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DIRECT EXAMINATION

BY MR. ENGELHARDT:

Q Dr. Tompkins, would you please state your full name and address?

A My name is Paul C. Tompkins. Address: 6808 Melwood Road, Bethesda, Maryland.

Q Would you please state your present position and give a summary of your educational and professional qualifications?

A My present position is that of acting director, Division of Criteria and Standards, Radiation Office, EPA. As to education and qualifications I have a Bachelors in chemistry from Whitman College, Walla Walla, Washington, 1935, and graduate work at the University of Chicago and the University of California. Ph.D from the University of California in Biochemistry in 1941.

As regards my work in this field, as a graduate student I helped some of my colleagues in the preparation of radioactive materials for the cyclatron at Berkeley where they were doing distribution experiments on such things as phosphorous burning tubes, potassium and so forth for physiological measurements.

When the Manhattan District was created, the recruiting procedure was that those who were brought into the project wrote to their friends that they thought had the

background which would help the project. I was so approached and I joined the Manhattan District in 1942.

The particular functions we were given were in the biology division of the metallurgical laboratory, University of Chicago, for the specific purpose of separating and isolating high specific activity fission products for toxicity measurements.

The reason for going after high specific activity materials is that the function of that program was to derive the kind of data that would be needed to establish the working practices and standards for the plutonium separation project at Hanford.

My work during the war was in the separation of strontium isotopes, barium and lithium isotopes, phosphorous burning tubes, radium, plutonium, columbium, yttrium and a few others that I forget right at the moment that were then used for animal experimentation, the purpose of which was to determine those conditions and quantities that would be lethal as a result of the internal deposition of the nuclides in animals.

As a result of this dealing with a material that was very high in radioactivity I became quite interested in safe handling procedures and techniques. And a lot of my experience during the war was in the development of laboratory designs and laboratory equipment ranging in activities from

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the range of microcurie up to several hundred curies.

During the way as a result of this interest I was moved to Oak Ridge where I continued this same process. And I might say my interest in the safe handling techniques and so forth was strictly a matter of self-defense. At Oak Ridge I joined the biology division because of my interest in biological effects.

At the end of the war we were given our choice of remaining with the project or returning to our point of origin. I elected to return to San Francisco, from where I came, and join the staff of the Navy Radiological Defense Laboratory with the assignment of development of defenses for the military against the radiation effects of nuclear weapons with specific reference to fallout and fallout contamination.

From 1952 to 1960 I was the scientific director of that laboratory. And the experience and the functions and missions naturally dealt with the origin, distribution and environmental behavior and derivation of subsequent hazards from environmental radioactivity.

In 1960 I was approached by the Public Health Service to join them to do the same kind of thing but with the emphasis on civilian protection instead of military defense. I became the chief of the circuit branch of the Division of Radiological Health. It became apparent very

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early that the Public Health Service was a little premature, that they were not equipped and not prepared to support a program of the type envisioned.

So I transferred to the Atomic Energy Commission as the Deputy Director of Radiation Protection Standards. My first assignment there as a result of my experience with the Navy was as the AEC member of the working group of the Federal Radiation Council.

In 1963 when it was decided that the FRC would set up a staff independent of any of the agencies I was asked to become the executive director. It was in that capacity that I served from March of 1963 until December 2 of 1970 at which time the FRC was abolished and its functions were transferred to the Environmental Protection Agency.

Q Dr. Tompkins, would you please discuss the history of the development of the radiological protection guidelines which underlie 10 CFR Part 20.

A I would be happy to. I think the basic factor to be understood is that it has been known for some time that ionizing radiation under appropriate circumstances and in sufficient quantities is capable of causing damage and even death to the persons exposed. The experience of the early radiologists prompted the Second International Congress of Radiology in 1928 to examine the hazards being

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experienced by practitioners of that profession.

This, in turn, led to the establishment that same year of the International Commission on Radiological Protection, more commonly known as ICRP. The ICRP was established to develop recommendations for protection of radiologists from exposure to X-rays and gamma radiation from radium and its products.

The interests of the ICRP and associated national organizations expanded thereafter to protection from all occupational sources of exposure. As an outgrowth of the founding of the ICRP, the National Committee on Radiation Protection, known as NCRP, was formed in the United States in 1929 under the sponsorship of the United States National Bureau of Standards for the purpose of coordinating the views of the various societies and other organizations with an interest in radiation protection problems.

In 1964 Congress, pursuant to Public Law 88-376, chartered the NCRP as an independent advisory body.

The series of events which led to the formation of the Manhattan District during World War II included the recognition by responsible scientists that the development of nuclear energy contemplated would be associated with quantities of radiation and radioactive materials many orders of magnitude greater than man had ever encountered.

It was also recognized that from the experience

with radium poisoning and the deaths from cancer that had ensued there would be a spectrum of unfamiliar radionuclides as well as a spectrum of unfamiliar types and energies of emitted radiation which would also be encountered in large quantity.

Accordingly, General Leslie Groves, head of the Manhattan District, set up medical, biological research and what is now known as health physics organizations to establish safety practices and associated standards to guide in their development. Members of the NCRP were brought in as principal advisors to the health and safety personnel with responsibilities in the Manhattan District.

In the early days of the Manhattan District they were faced with three choices about which I can assure there was a good deal of debate. The first choice was to set the standards so that one would avoid any acute toxicity and hope there would not be too severe a hazard from the expected long-term latent hazards such as delayed cancer.

The second possibility was to try to compromise between these two and do a benefit-risk approach.

And the third was to insert, as a matter of policy, very stringent control standards with the hope of not only avoiding acute radiation injury but also avoiding

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the more latent longer-term developing side effects.

As a matter of policy, General Groves selected the third option, and severe and stringent controls were applied. And this has influenced the concept of radiation protection developed subsequently through activities of the Manhattan District during the war.

Radiation protection standards until the end of World War II were expressed in terms of a "tolerance dose." Also, since these were occupational standards, the population at risk was considered to be adults. The radiation protection standard in force during the Manhattan District period was 0.1 R per day. For purposes of reference I will equate for purposes of this discussion, one R with one rem which is a tissue dose. This would permit a presumed annual dose of 35 rem per year.

In 1946 the NCRP was reorganized to accommodate the vast increase in responsibilities imposed by the development of the atomic energy program. And the Committee set up a number of subcommittees to reexamine NCRP standards.

On the basis of the experience during the war, the NCRP almost immediately decided to lower the permissible dose for radiation workers from the then current level of 0.1 rem per day to 0.3 rem per week. This was done primarily in recognition of the fact that under peacetime conditions

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the potential exposure of radiation workers would be related to a much larger work force than was true during the war. The action was based then on prudence, demonstration of industrial capability to operate at the lower level and in recognition of a growing work force.

The NCRP members participated in a series of tripartite conferences among the United States, Great Britain and Canada in 1949, 1950 and 1953. These tripartite conferences went into detail on the lessons learned in the wartime development. And all of the radiation protection standards were re-examined. The other nations agreed with the NCRP and the United States that experience with radiation workers in nuclear institutions has shown that it is practical to operate such installations at a lower value than 0.1 rem per day for annual exposure. Therefore, the value of 0.3 rem per week previously proposed by the NCRP was adopted. This recommendation was also adopted by the ICRP in 1950.

In 1954 the NCRP Handbook 59 was issued containing that body's further recommendations respecting exposure to radiation. A distillation of all available knowledge concerned with possible effects of ionizing radiation on human tissue at the time of the report lay behind the NCRP recommendations. In the Handbook they discussed biological variability, latent period, recovery and repair, radiosensitivity, relative biological effectiveness,

differential variations, that is, two distinct biological entities with different radiation sensitivities being exposed to two or more radiations of different specific ionizations. It also included examination of the whole body radiation, genetic effects and reduction in lifespan.

The philosophy behind the basic recommendations for radion protection is expressed in Section 4.1 of Handbook 59 and is as follows:

"As a matter of principle it is sound to avoid all unnecessary exposure to ionizing radiation, because it is desirable not to depart from the natural conditions under which man has developed by evolutionary processes.

"However, man has always lived in a field of ionizing radiation due to the presence of radioactive material in the earth and cosmic rays. Whether exposure to this level of radiation is beneficial or deleterious to man and the race is a matter of speculation.

"The obvious fact is that it cannot be avoided and it is therefore normal for man to live in this environment. We have a lower limit of continuous exposure to radiation that is unavoidably tolerated by man.

"There is, on the other hand, a much higher level

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of exposure that is definitely known to be harmful. Between these two extremes there is a level of exposure, in the neighborhood of 0.1 rems per day, that experience to date shows to be safe for the individual concerned. However, the time of observation of large numbers of people exposed at this rate under controlled conditions is too short to permit a categorical assertion to this effect.

"It should be noted in this connection that lowering the level of exposure by a factor of two or even ten, does not materially alter the situation insofar as making a positive statement of absolute safety is concerned. The only statement that can be made at the present time about the lifetime exposure of persons to penetrating radiation at a permissible level considerably higher than the background radiation level, but within the range of radiological experience, is that appreciable injury manifestible in the lifetime of the individual is extremely unlikely.

"It is therefore necessary to assume that any practical limit of exposure that may be set up today will involve some risk of possible harm. The problem then is to make this risk so small that it is readily acceptable to the average individual; that is, to make the risk essentially the same as is present in ordinary

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occupations not involving exposure to radiation."

End quote. Public controversy over the effects on the environment of radioactive materials developed in the early 1950's is the result of fallout from atmospheric testing of nuclear weapons. By 1954 the controversy was acute enough to prompt the Rockefeller Foundation to give a grant to the National Academy of Sciences, known as the NAS, to make a comprehensive review, independent of the government concerning knowledge related to the biological effects of atomic radiation.

The NAS, in turn, organized the Committee on Biological Effects of Atomic Radiation commonly known as the BEAR Committee. The first summary reports of the BEAR Committee were published in 1956. Based upon its consideration of the effects of radiation on reproductive material and the quantity of radiation which was judged at that time would double the natural mutation rate in man, and considering the fallout would affect the population of the whole world, the BEAR Committee recommended that, for the general population, an average per capita gonodal dose accumulated during the first 30 years of life should not exceed 10 rem of man-made radiation and should be kept as far below this value as is practicable.

The average per capita dose is by its definition the per capita dose that would give -- per capita dose

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multiplied by the size of the population at risk -- that would give the same number of rems as the exposure actually received.

This was the first numerical recommendation for a limitation of cumulative radiation exposure due to all kinds of sources of man-made radiation based on genetic considerations alone. The NCRP reviewed the BEAR Committee reports and and, for practical reasons, divided this numerical value between radiation exposure as associated with medical practice and radiation exposure associated with all other practices. It divided this 10 rems recommended by the BEAR Committee into two , 5 rems for medical practice and 5 rems for everything else.

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Meanwhile, the United Nations, following the implications of fallout from weapons testing, established in December 1955, a United Nations Scientific Committee on the Effects of Atomic Radiation. This is known as UNSCEAR. This committee examined every aspect of what was know at that time about ionizing radiation, both naturally occurring and man-made.

The first report issued in 1958 contained five main subjects:

A. Genetics.

B. Effects of radiation by internally absorbed isotopes, and the effects of external radiation.

C. Natural radiation levels.

D. Exposure during medical procedures and occupational exposure.

E. Environmental contamination.

Item E quite naturally emphasized everything which was known at that time about radioactive fallout from atmospheric testing, the movement of this material in the environment, and its implications with regard to effects on health involving the whole world population. The available information on the effects of ionizing radiation at the level of individual molecules, cells, tissues, and more complex organisms, such as the whole body of animals. The types and quantities of radiation absorbed by tissue were

examined for the whole spectrum of functional organs. These included separate sections on evidence for radiation injury affecting the blood-forming organs, skin, gastrointestinal tract, nervous system, bone, gonads, vascular system, eyes, lungs, endrocrine organs, and embryonic development.

The 1962 U.N. report was also comprehensive.

I would like to insert for the record starting with the 1962 report -- I have served as a member of the U.S. delegation on UNSCEAR and still serve in that capacity -- which covered the physical and biological aspects of the interaction of ionizing radiation with matter -- somatic effects, hereditary effects, sources of irradiation and comparison of doses and estimates of risk.

In its 1958 report the U.N. Committee estimated absolute risk, that is, it calculated the frequency or number of effects which would result or be expected to appear in a stated population subjected to a stated radiation dose.

In its 1962 report, however, the committee decided that the assumptions they had to make about biological behavior under stress were so far reaching that the results were highly theoretical and that the hypothesis on which they rested were rather flimsy, and so the committee abandoned this approach on the basis that such absolute risk could not be estimated reliably, and shifted to estimating what

it called comparative risks, or the dose commitment.

By this procedure the estimated dose resulting from a particular source such as fallout from atmospheric testing of nuclear weapons is compared with the dose that would be delivered by a different source such as that arising from exposure to natural background radiation.

There is no attempt made to calculate the number and frequency of adverse effects that might be associated with either of the doses being compared.

After preparation of the 1962 comprehensive report, the U.N. Committee felt that future reports should concentrate on selected areas and that an updated evaluation would be useful.

The 1964 report was accordingly confined to an evaluation of radioactive contamination of the environment by nuclear tests and radiation carcinogenesis in man.

The 1966 report up dated information concerned with radiation from natural sources and environmental contamination by manmade radiation.

The genetic risks of ionizing radiation were also updated and reevaluated.

The 1969 report reexamined radioactive contamination of the environment by nuclear tests, effects of ionizing radiation on the nervous system and radiation in chromosome aberrations in human cells.

The memberships of the various delegations that

assisted in preparing these various reports were selected on the basis of their established professional competence and expertise in the particular areas being reviewed.

In 1957 the NCRP issued a preliminary revision to its recommendation for maximum permissible exposure which was designed to control the accumulation rate. They adopted the basic formula that occupational exposure should be so controlled that the accumulated radiation dose would not exceed $5 \times N^{-18}$ where "N" is the age in years. It also repeated its earlier recommendation that permissible levels from radioisotopes taken into the body would be accomplished by control of the average concentration of radioactive materials in the air, water or food taken into the body.

In discussing dose to persons outside of the control areas, NCRP recommended radiation or radioactive material outside of the controlled area and attributable to normal operations within the controlled area shall be such that it is improbable that any individual will receive a dose of more than 0.5 rem in any one year from such radiation. It also observed that the maximum permissible average body burden of radionuclides in persons outside of the controlled area and attributable to the operations within the controlled area will normally entail control of the average concentration in air or water at the point of intake. And that the body burdens and concentrations of radionuclides so estimated may be averaged over periods up

to one year.

During the same period of time, that is, between 1957 and 1960, the NCRP set up an ad hoc committee specifically to consider the scientific and philosophical base of radiation protection standards affecting the general population. This was again an outgrowth of the concern over weapons test fallout.

The important contribution of this study was the view that the range of exposure conditions in the naturally occurring environment was a logical point of departure for considering permissible exposures of the general public.

At about during the same period there was a great public debate over the applicability of the NCRP standards to the interpretation and control of fallout from weapons testing. It suddenly became aware to the government that the NCRP was not a government body, was not a government entity.

So the President asked the Director of the Bureau of the Budget in concert with the Secretary of Health, Education, and Welfare, and the Chairman of the Atomic Energy Commission to review completely teh posture of the United States Government in its ability to handle official standards for the government.

The result of this review which was transmitted to President Eisenhower was that there was no single agency with the scope of mission and assignment that could view

the problems of the Federal Government as a whole in concert. It was therefore recommended to him that he be advised by an interagency advisory group called the Federal Radiation Council.

Accordingly, the Federal Radiation Council was established by President Eisenhower in Executive Order No. 10831 dated August 14, 1959 and was subsequently made a statutory body by an enactment of Section 274(h) of the Atomic Energy Act of 1954 on September 23, 1959.

Section 274 of the Atomic Energy Act as amended provides as follows and I quote: "There is hereby established a Federal Radiation Council consisting of the Secretary of Health, Education, and Welfare, the Chairman of the Atomic Energy Commission, the Secretary of Defense, the Secretary of Commerce, and the Secretary of Labor or their designees and such other members as shall be appointed by the President. The Council shall consult qualified scientists and experts in radiation matters, including the President of the National Academy of Sciences, the Chairman of the National Committee on Radiation Protection and Measurements, and qualified experts in the field of biology and medicine and in the field of health physics.

"The special assistant to the President for science and technology or his designee is authorized to attend meetings, participate in the deliberations of and to advise

the Council.

"The Chairman of the Council shall be designated by the President from time to time from among the members of the Council.

"The Council shall advise the President with respect to radiation matters directly or indirectly affecting health, including guidance for all federal agencies in the formulation of radiation standards and in the establishment and execution of programs of cooperation with states.

"The Council shall also perform such other functions as the President may assign to it by Executive Order."

The President added the Secretary of Agriculture to the Council on August 16, 1962 and subsequently added the Secretary of the Interior on January 16, 1968.

The Secretary of HEW was the first chairman appointed by the President and by convention the Secretary of the Department of Health, Education, and Welfare continued effectively as the chairman of the FRC from the time of its abolishment.

Following the creation the FRC undertook a completely independent review of what was then known about ionizing radiation, with particular refarance to its effects on man. The first report of the FRC was submitted as a memorandum to the President in 1960.

In its report number 1, the FRC stated "Although

ionizing radiation can induce genetic and somatic effects, that is, effects on the individual during his lifetime other than genetic effects, the evidence at the present time is insufficient to justify precise conclusions on the nature of the dose effect relationship at low doses and dose rates.

"Moreover, the evidence is insufficient to prove either the hypothesis of a damage threshold, that is, a point below which no damage occurs, or the hypothesis of a no threshold in man at low doses."

It also said "There are insufficient data to provide a firm basis for evaluating radiation effects for all types and levels of irradiation."

"There is particular uncertainty with respect to the biological effects of very low doses and low dose rates. It is not prudent, therefore, to assume that there is a leveling of radiation exposure below which there is absolute certainty no effect may occur."

This consideration, in addition to the adoption of the conservative hypothesis of a linear relation between biological effect and the amount of dose determines our basic approach to the formulation of radiation protection standards.

The FRC report number 1 also accepted the view that setting radiation standards inevitably involves a judgment on a balance between the benefits from the activities

associated with the cause of the exposure and risks resulting from the exposure.

FRC report number 1 established an annual radiation protection guide known as RPG of 0.5 rem for individuals in the population and coupled that as an operational technique when the individual exposure could not be estimated, one-third of 0.5 would be applied to the average per capita dose of a suitable sample of the exposed population.

The recommendations contained in FRC report number 1 were approved by the President for guidance of federal agencies on May 13, 1960. It is implicit in the definition of the RPG which states that the radiation protection guide is the dose that should not be exceeded without careful consideration of the reason for doing so, and that every effort should be taken to maintain actual exposures far below these guides as is practicable; that the benefit-risk balance made by the Council at that time was that provided these recommendations were met; that the aggregate benefits from all activities would exceed by far the aggregate risks coming from the resultant potential cumulative exposure.

On September 13, 1961, the FRC reviewed report number 2, or issued report number 2, in the form of a memorandum for the President.

In summary, report number 2 contained recommendations for the guidance of federal agencies and activities designed to

limit exposure of members of the population groups to radiation from adioactive materials deposited in the body as a result of their occurrence in the environment from normal peacetime operations.

Subsenquently normal peacetime operations had to be redefined and the actual definition is that the RPG applies to industrial activities, where the controls are primarily placed at the source.

It is envisaged that this would apply to such things as power reactors, research installations, experimental arrangements, hospitals, and so forth.

The recommendations in report 2 contained radiation protection guides for certain individuals in the general population, as well as averages to be applied to the suitable sample of exposed groups. It also included guidance on general principles of control applicable to all radionuclides occurring in the environment, and specific guides in connection with exposure of population groups with radium 226, iodine 131, strontium 90, and strontium 89.

Included in this guidance was the concept of a graded scale of accidents. One-tenth of an average daily intake is the basis under which intake guides are established, one-tenth of the average daily intake --

Excuse me. I will go back.

The average daily intake taken each day over a

year which would result in a tissue dose or dose rate equal to the RPG for the organ or tissue in question. This was the base number. One-tenth of that, or 10 percent of the RPG was considered to be sufficiently low that he only requirement was that surveillance be capable of insuring that the environmental contamination levels were indeed less than 10 percent of the RPG. This was called range 1.

Within range 2, which is in the area between 10 percent and the RPG, the instruction was to shift the quantitative surveillance, measure or estimate the actual exposures, and if a growing trend was noted, to take such additional engineering restrictions as would be required to prevent the annual exposures going as high as the RPG.

It also established a range 3 which was 10 times the RPG, and there the stipulation was that some kind of definite corrective action to bring the exposures back down was indicated.

These FRC recommendations were approved by the President on September 20, 1961.

Now as to the way the FRC works, I have already indicated in Section 274(h), the composition in terms of the agencies involved.

Procedurally, each member of the FRC appointed, a senior member of his senior staff, the criterion being scientific competence in radiation matters, and the purpose

was to serve on what was known as the working group of the Federal Radiation Council.

The working group brought to the FRC problems of immediate interest to federal agencies. When the FRC was engaged in a specific project, the work was conducted by means of task groups of technical people in government, and when appropriate, consultants from the scientific community, representatives of state agencies, industry, and labor.

The work of these task groups was then reviewed by the working group and the relevant information compiled in a background staff report. The staff report included a discussion of the essential scientific considerations and technical considerations in a way which will be relevant to policy decisions that might be involved in the particular problem and the policy decisions then are made by the members of the Council themselves.

The FRC Executive Director and the working group approved each staff report but the Council members themselves approved the memorandum for the President which made the official recommendations.

The recommendations to the President involved both technical and policy considerations..

Upon approval by the President and publication in the Federal Register, FRC recommendations become official guides for federal agencies.

There have been nine FRC reports to the President during the period of 1960 to 1969.

The situation at the present time is that on December 4, 1970, Mr. Ruckelshaus put into the Federal Register a notice which will be found in Federal Register Volume 35, No. 235, page 18486. This is entitled "Continuation of Functions" and the instruction was that those functions in being at the time of the transfer or the creation fo the Environmental Protection Agency would continue as the EPA -- they were simply transferred and would be adopted by the EPA.

Accordingly the Water Quality Standards which were formerly in the Department of Interior became EPA Standards in the Water Quality Office of the Environmental Protection Administration.

The air quality criteria and standards formerly in the Department of Health, Education, and Welfare became the EPA Standards for the Air Pollution Control Office in EPA. And the FRC Standards became the EPA Standards for the Radiation Office in EPA.

I think there are a few other things I should mention in terms of the general history.

The ICRP adopted the general philosophy of attempting to measure the safety in the nuclear industry against safety in other industries that had quite different

hazards to cope with.

Consequently they felt that they had a policy requirement to identify or estimate to the best of their ability the risk of injury, particularly of cancer, at the doses and dose rates specified in their maximum permissible dose recommendations.

End #1

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Now this is quite a departure from the scientific decision of the United Nations that said this couldn't be done. But feeling that they had to make the best estimate they could anyway, they have had two task forces examining the matter of estimating risks as they might be judged at levels and conditions comparable to the MPDs.

During the middle sixties they put out two rather important documents, one known as ICRP Task Report No. 8 that made estimates of risk and they cataloged them in what they termed the orders of risk. By definition the order -- well, a 6th order risk, for example, would indicate that one would expect between 1 and 10 cases per million persons exposed, the 6 coming from the exponent 6.

If it is 1 to 10 per 100,000 persons exposed, it would be called a 5th order risk. The ICRP 14, recognizing the rather widely diverse dose distributions in different tissues instituted a study on the relative sensitivity as a function of space distribution and tissue sensitivity. This was called ICRP 14. And the philosophy of ICRP is that they are concerned with what you might call the actual number or absolute number of adverse effects that would be predicted.

The possibility that radiation risk could be examined equally in terms of a percentage change in the underlying natural risk was pointed out by the Committee in the early 1960s. But the rationale of ICRP is that a small

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percentage change in a particular disease that has a high natural incidence would result in more deaths than a large percentage change in a disease which had a low natural incidence and since they were concerned with absolute safety, they wanted number of cases, rather than percentages.

And that is the official position of ICRP at the present time.

In November of 1969 Drs. Gofman and Tamplin from the University of California at Livermore opted to go for the percentage change explanation. And they made a presentation to Senator Muskie, in which they claimed that the actual effects to be anticipated from or permitted under the guidelines of any of the radiation protection bodies would result in a much larger number of adverse effects than such bodies had contemplated.

Senator Muskie sent this testimony to the federal agencies, it was decided the agencies would reply separately, and they did. In the reply from the Department of Health, Education and Welfare it was pointed out that the HEW, as indeed was true of all of the federal agencies, did not accept the underlying premise, assumptions, and so forth that were necessary for Tamplin and Gofman to make in arriving at their numbers.

But in view of the fact that there are three different ways to express risk, and that recognized

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international scientifically competent bodies had opted for alternates, Secretary Finch quite properly in my opinion felt it was time for the Federal Radiation Council to completely review the scientific basis of its own guidance. We made this recommendation to the Council and as a consequence of that, a major review was initiated by the FRC early in 1970.

The review covers essentially four parts. The reexamination of the scientific basis for estimating risk was established by contract between the FRC, or between the Department of Health, Education and Welfare and on behalf of the FRC and the National Academy of Sciences. That review will be not only comprehensive, but it is hoped it will be quite critical and exacting. It is of a scope and magnitude equivalent to the Bear Committee's reports of a decade or so ago and the estimate of the Academy is to do a scholarly and reliable job of reviewing all of the evidence, and also all of the competing interpretations and it will take approximately two years.

We have with the NCRP a contract to evaluate the models relating to contamination of the environment by strontium and cesium isotopes, the intake in the body, and subsequent tissue dose resulting from an effort to look at the distinction between a continuous long-level low intake, as is characteristic of fallout, and a more acute contaminating event as might occur from an accident, where you had a short

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burst. The risk considerations become quite different here: The NCRP is examining the best way to draw these relationships in order to permit reasonable decisions for protection and safety to be made in the face of a variety of circumstances.

A third part of the review is being conducted by a temporary staff assigned to the FRC, one from the Atomic Energy Commission, one from the Department of Defense, and one from the Department of Health, Education and Welfare. The purpose of this temporary staff is to utilize the full resources of all of their agencies to quantitate what we can say at this point in time about the dose commitments associated with differing activities, what we can say about the populations at risk, what we can say about changes in occupational exposure that have occurred in the past decade, are they going up or going down, are the control practices keeping pace with the changes in application.

Recognizing that if one is going to talk about radiation risk as such, one is not entitled to talk about only that part of it which is being subjected to radiation. Therefore, we are going to see to what extent we can quantitate the dose commitments from the natural backgrounds, the variations in populations at risk that might be associated with variations in natural backgrounds, the dose commitments to the traveling public as a result of cosmic ray exposure from jet travel, projections of the future exposure that might

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be anticipated as a result of the growth of nuclear power. And in effect as complete a catalog of the current state of knowledge regarding what is now affecting the population of the country as a whole as we can derive.

Now in order to establish a rational basis for a benefit-risk balance, it will also be necessary to project major changes or innovations of growth that may occur in the next 10 to 20 years. Consequently, we will try to get some kind of official judgment as to what reasonable can be expected in connection with the SST development, in connection with the not currently approved but sought after commercial application of peaceful nuclear explosive, particularly for stimulation of gas and any other major newer growth in old applications that we can find.

Implicit in this is there is no distinction for risk purposes drawn between medical and nonmedical exposure in contrast to the distinction drawn on standards. And I believe -- this whole program was transferred to EPA and starting with fiscal year 1972, it will be conducted under EPA sponsorship and EPA budget. In January of this year the National Council on Radiation Protection and Measurements issued a new Report No. 39 which in effect updated and upgraded its so-called basic standards that were previously found in the so-called Handbook 59.

This covers the same territory essentially covered

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by FRC Report No. 1. They have made some changes which they consider to be minimal and certainly not very drastic. One change was the thyroid dose criterion for occupational workers which in the previous report and in the FRC report was 30 rems per year, this has now been changed to 15.

I think this is in recognition of the fact that the current evidence would suggest that instead of being somewhat less sensitive than other tissues to injury, it is now felt that the thyroid is at least as sensitive as other tissues and, therefore, should be treated similarly in terms of dose limits.

The occupational skin dose criterion for an unlimited area of the body was changed from 30 rem per year to 15 rem per year. The forearm dose criterion for occupational workers was changed from 75 rem per year to 30 rem per year. The feet and ankle dose criterion for occupational workers was changed from 75 rems per year to 15 rems per year. A limitation of 0.5 rem to the fetus during the entire gestation period has been proposed for application to pregnant women in the occupational worker category.

Women of reproductive capacity in the occupational work category and exposed to stated radiation conditions should be limited to a rate of two to three rems per year instead of the five rems which is the normal occupational and it is known that when a woman becomes pregnant the

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exposure of the fetus should be limited in both dose and dose rate. This change essentially would establish a new radiation worker category.

They have a recommendation for students under age 18 who are involved in educational activities for which the limitation should be 0.1 rem per year. The educational activities in question do not necessarily refer to the use of radionuclides, but are intended to serve as a basis for establishing appropriate procedures and safeguards in high school and college physics laboratories using cathode rays and similar radiation producing types of devices.

In the nonoccupational exposure category, certain organ dose criteria to the individual and the public were changed from 1.5 rems per year to 0.5 rem per year. These particular organs include skin, GI tract, lung, bone, thyroid, kidney, spleen, pancreas, prostrate, muscle tissue, or fatty tissue. In its explanation of its report, the members of the NCRP made it clear that the fundamental approach in making these changes had two objectives.

One was to examine and reexamine what levels could be considered practicable for the activities to which the standards apply and the changes in occupational categories were predicated on the belief that these lower limits were indeed practical and, therefore, should be utilized. The second was that in having a difference between exposure of

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the whole body and the exposure of certain individual organs, a difference by a factor of three, made life unduly complicated and where one is dealing with the general public it is by definition the risk must be kept exceedingly low so that biological variability becomes an important factor. They saw no biological justification for trying to drawn distinctions between various organs and tissues, and, therefore, they just arbitrarily established the same dose limit of .5 rems per year regardless of how, where, who, what the exposure conditions might be.

They were quite emphatic that none of these changes were related to -- well, there is only one change that was in fact related to a change in opinion on sensitivity and that was the thyroid. They do not feel that the restriction they put in on the fetus or to protect the fetus implied any change in their previous judgments regarding sensitivity of embryonic and fetal developments, but the recommendation was made to insure that the dose rate, which is important in potential hazards to developing embryos, was kept quite low, as wellas the total accumulated dose.

And I would say in closing that there is one other principle that has been followed by ICRP, NCRP, and by the FRC, and that is what we call the consensus principle. Namely, we try to get a consensus of opinion on all of these recommendations and changes from anybody who would be directly affected

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by one or the other. The medical profession was consulted at great length on the hand and finger limitations because of the use of radium applicators and so forth in medical practice.

Any group that might be affected is invited ir to participate in the task forces. We make a broad-scale effort to incorporate everybody who has a direct stake in the outcome. And I think that that is the general viewpoint of all people who are concerned with standards.

We make no pretense of trying to consult everybody, but we make an issue out of consulting all legitimate interests that have a stake in the outcome of whatever standards may be up for development.

Now, the last comment I will make is that FRC standards and guidelines apply to all activities, not just to a few. They are not designed specifically for the atomic energy industry, but the atomic energy industry is expected to live within them. They apply equally well to the regulations put out by the Department of Health, Education and Welfare relative to permitted emissions from color TV sets, and the whole works.

There is some belief that in EPA one should perhaps look at the implications of establishing enough planning standards such as FRC has done. But perhaps relook at the types of activities for which standards would be appropriate.

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No action has been taken on this, but the subject is certainly being looked at in terms of the advantages and disadvantages.

DR. JORDAN: The last statement you had made had to do with apportioning between the various industries?

DR. TOMPKINS: That is what some people call it. See, when you put in a limit and then talk about apportioning, the first assumption one is making is that exposure to that limit is quite acceptable. I don't buy that. That is the maximum.

The exposure should be kept as far below as is practical. So what it comes down to is determining what is practical for each of the different classes of activities. And I call that separate standards for separate activities, but I would not concede to a true apportionment because of the concept.

That is just a philosophical thought.

MR. ENGELHARDT: Mr. Chairman, I believe that completes Dr. Tompkins' testimony. In line with our proposed approach, we would now plan to proceed with the presentation of our other witnesses, and Dr. Tompkins would be available for such examination by the Board and parties as may be desired when we complete the full presentation.

CHAIRMAN SKALLERUP: Thank you, Dr. Tompkins.

We will take a 10-minute break.

end 2

(Recess.)

DB3 lnl 1 CHAIRMAN SKALLERUP: Will the hearing please come to order.

We are fortunate to be able to announce that arrangements have been made for tomorrow night's session and they will be held at the St. John's Lutheran Church, which is across the street from here, on the southeast corner of this intersection of Adams and Second Street.

It is our plan to adjourn tomorrow afternoon at 4:15 and to resume the evening session at the St. John's Lutheran Church at 7:00 p.m. We will make this announcement again tomorrow morning at the opening of the session.

MR. CHAPNOFF: Mr. Chairman, before Mr. Engelhardt resumes with his rebuttal, I would just like to indicate that this morning we have sent over a copy of yesterday's transcript to Mr. Lau's home, so that that would be available to him for his preparation of any cross-examination he might have of us.

Secondly, after noting the good right arm of Miss Evans writing away while Dr. Tompkins was speaking, we have made arrangements with the Reporter to bind up copies of this morning's transcript and we will lend to LIFE and to Mrs. Stebbins, as soon as it is available from the Reporter, a copy, we will lend a copy to LIFE and a copy to Mrs. Stebbins of our copies of this morning's transcript, so that the rebuttal testimony by the AEC offered this morning and not

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available in written form yesterday will be available to both of those parties as soon as it is available to all of us in writing.

CHAIRMAN SKALLERUP: Thank you, Mr. Charnoff.

Mr. Engelhardt.

MR. ENGELHARDT: Mr. Chairman, I would like to call as our next witness Dr. Daniel Nelson.

DIRECT EXAMINATION

BY MR. ENGELHARDT:

Q Would you please state your name and address?

A Daniel J. Nelson. I live at 116 East Morningside Drive, Oak Ridge, Tennessee.

Q Would you please state for the record your present position and provide us a summary of your educational and professional qualifications?

A Presently I am Assistant Director of the Ecological Sciences Division, at Oak Ridge National Laboratory, Oak Ridge, Tennessee.

My educational qualifications include a bachelor of science degree in 1947 from Iowa State University, in 1947, a master of science degree in fish and game management from Oregon State University in 1949 and a Ph.D. in zoology with major in ecology from the University of Georgia in 1957.

I have worked for the Georgia Game and Fish Commission from 1949 to 1953 and I was an assistant professor

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of biology at West Virginia University from 1957 to 1959. Since 1959 I have been at Oak Ridge National Laboratory as an ecologist, where I work with radioactivity in the environment. We study food chain movements, radionuclide cycle and the effects of ionizing radiation on natural populations of organisms.

I am a member of a number of professional societies, such as Ecological Society of America, American Society of Limnology and Oceanography, Health Physics. I am a fellow of the American Association for the Advancement of Science. I review papers editorially for the Journal of Science, Ecology, Limnology, and Health Physics, among several others.

Q Dr. Nelson, are you familiar with the testimony of Dr. Tamplin which was given in this hearing?

A Yes.

Q Would you give your evaluation of Dr. Tamplin's views with respect to the doses of radioactivity which may be anticipated because of food chain process.

A Both the testimony of Dr. Tamplin in the Transcript Pages 1499 to 1510 and answers in the cross-examination in Transcript Pages 1523 to 1560 question the adequacy of 10 CFR 20 to protect people living in the vicinity of nuclear facilities from excessive exposure to radiation.

Dr. Tamplin contends that the maximum permissible

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concentation values have very little relevance in the real world. Dr. Tamplin further contends that radionuclides are concentrated in food chain processes and because of this man will experience excessive radiation exposures from drinking milk from cows which are grazing on pastures exposed to MPC levels of cesium-137.

Similarly people drinking water with MPC values would receive excessive exposure, as would people eating one pounds of fish per week from contaminated water.

Dr. Tamplin's testimony is based on a mathematical description of the movements of cesium-137 in air to pasture forage which cows eat and in turn yield milk contaminated with cesium-137.

According to Dr. Tamplin's calculations, the consumption of one liter of milk by a 150-pound man, a 75-pound child or a 100-pound pregnant woman would result in exposures greater than the limits set forth in 10 CFR 20. Similar conclusions were reached for drinking water and eating fish.

Details of the methods used by Dr. Tamplin to calculate the effects on man of radioactivity in the environment are contained in a series of reports identified by Dr. Tamplin on the Transcript Page 1524.

An important assumption of Dr. Tamplin's mathematical description is that the food chain will receive the maximum amount of radioactivity immediately upon exposure from a

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source. This is biologically unsound because we know that cesium-137 moves in the food chain from grass to the cow to the milk and then to man and there are time-dependent variables in this process.

Another assumption is the fact that he assumed that the availability of the radionuclide in food was always 100 percent. We know -- by availability, we mean the capability of the organisms, digestive processes, to utilize and assimilate this particular nutrient element. And we know that availability varies significantly depending upon its chemical form and associated materials which are swallowed along with the grass.

With cesium, for instance, if any dirt or soil particles are ingested with the grass, the amount of cesium which is available will be reduced. Dr. Tamplin does not take into account varied practices.

For instance, supplemental feed is an important part of the diet of dairy animals and cows give more milk when they are penned and fed than when they are allowed to graze at will on the range. In fact, Dr. Tamplin admitted on page 1554 of the Transcript that it was absurd to assume that cattle would stand at the boundary fence and eat all day.

With respect to the assumption that man drinks 2,200 milliliters of water each day, Dr. Tamplin has similarly misinterpreted the application of concentration

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limits with respect to calculations used with average man. Actually the average man is assumed to drink 1,200 milliliters of water, and the other 1,000 milliliters of water in his average daily diet comes from the food.

CHAIRMAN SKALLERUP: Could you convert that into common language of pints?

THE WITNESS: Well, a liter is roughly a quart, slightly more than a quart, one and six-hundredths quart. So what we are saying is that a man drinks probably about a quart and a pint of water each day, five pints of water, someplace in there. And then there is another quart of water that you get with your mashed potatos or beef steak, whatever you happen to be eating, or gravy.

The total for the average individual being then slightly over two quarts. With fish exposed to one day's maximum permissible concentration of cesium in water, Dr. Tamplin also assumed that these fish reached their maximum concentration instantaneously.

Again this is biologically unrealistic, because we know fish ahve to digest their food and this is a function of temperature and it can go from maybe eight to twelve hours on up to three or four days. Dr. Tamplin has used these mathematical descriptions to calculate estimated doses.

On page 1551 of the Transcript, he says that there is little discrepancy between the results of his methods and

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results obtained by other methods. Actually his results yield doses that are high by a factor of 50 to 100 or even more for adults. For a 75-pound child, they are in even greater error, because he simply doubles the dose that a child would get which is not correct.

He neglects important biological factors with respect to children. They have a higher metabolic rate, the volume of air they breath is less, and also the volume of water consumed is less. But actually the quantity of food they eat, I guess most of you know, is considered equal to that consumed by the average man.

We are talking about a 75-pound child in this particular case. Because of Dr. TAmplin's unrealistic or erroneous assumptions with respect to the biological and ecological processes, his dose estimates are not valid.

BY MR. ENGELHARDT:

Q Dr. Nelson, in your testimony you indicated that Dr. Tamplin has used mathematical descriptions to calculate estimated doses. Then you say that his results yield doses that are high by factors of 50 to 100 or more for adults and similar highs for others.

Would you explain a little more specifically what the basis is for that particular statement?

A Well, we have calculated doses using similar mathematical descriptions of the processes which occur in the

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environment and in these descriptions we calculated doses as did Dr. Tamplin that were one percent to two percent of those that he observed. And I think this is the basis for our comments there.

MR. ENGELHARDT: Mr. Chairman, that completes Dr. Nelson's direct testimony.

DR. JORDAN: Dr. Nelson, have these calculations that you have done been published somewhere?

THE WITNESS: No, they have not. We do have calculations available if you wish to look at them in tabular form for direct comparison of Dr. Tamplin's doses and the doses we calculated.

MR. ENGELHARDT: Mr. Chairman, Dr. Nelson in conjunction with some of his associates at Oak Ridge National Laboratory has prepared a document which is called "Comparison of Doses Estimated by Dr. Tamplin's Methods and the Oak Ridge National Laboratory From Releases of Maximum Permissible Concentrations of Cesium-137 in Air and Water."

It may be helpful to offer this document for identification and then offer it in evidence. I would, however, like to distribute this proposed exhibit and have Dr. Nelson explain this proposed exhibit and then we can consider further the offer of this exhibit as a staff exhibit for this proceeding.

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BY MR. ENGELHARDT:

Q Dr. Nelson, would you explain the significance -- first of all let me ask you, did you prepare this for the purposes of our identification?

MR. ENGELHARDT: Mr. Chairman, I would like to have this chart which has just been distributed identified as Staff Exhibit 7.

(The document referred to was marked Staff Exhibit No. 7 for identification.)

BY MR. ENGELHARDT:

Q Dr. Nelson, would you tell me whether you have prepared this document which we have identified as Staff Exhibit 7?

A Yes, I prepared this document.

Q Would you identify the document by again reading the title?

A This is a comparison of doses estimated by Tamplin's method and the Oak Ridge National Laboratory from releases of maximum permissible concentrations of cesium-137 to air and water.

Q Can you tell us how this chart or this exhibit was prepared?

A Well, people in the Ecological Sciences Division at Oak Ridge National Laboratory have been developing

mathematical descriptions of ecological systems for a long period of time. The general idea behind these approaches is to permit us to estimate exposures of populations, both wild animal populations and human populations, to radionuclides in the environment.

The particular mathematical description that I used here in conjunction with two of my colleagues at Oak Ridge National Laboratory, Dr. S. V. Kaye and Ray Booth has grown out of a need to predict doses or to estimate doses in connection with nuclear events. This is the same reason that Dr. Tamplin has developed his mathematical descriptions of ecological systems.

end 3

Q Dr. Nelson, could you now explain this document which was identified as Staff Exhibit 7 so that the Board and the parties may understand what this chart is intended to convey?

A Well, Dr. Tamplin used three sources, milk, water and fish, which we have up there. And he has also put in certain assumptions with respect to these sources.

Incidentally there is an error in the transcript on page 1549, line 5, in connection with Dr. Tamplin's testimony. It says 82 microcuries per square meter and it really means 0.82 microcuries per square meter. This is in the milk calculation, the deposition of cesium 137.

DR. JORDAN: How do you know that that is an error on Dr. Tamplin's part?

THE WITNESS: Because his paper on the deposition of manmade radioactivity in the environment, which he quoted from at the hearings, contains the 0.82. It is the regulation of manmade radiation in the biosphere by Arthur R. Tamplin.

DR. JORDAN: Was that put into evidence?

THE WITNESS: No, it was not.

DR. JORDAN: And it might be the one that is in error.

THE WITNESS: No, I don't think so, because at the same time we found one other error in this transcript

where he had used one-thousandth for a concentration factor for fish and it appeared in the transcript as 1,000.

MISS EVANS: I might add we noted the same mistake about the concentration of cesium in fish flesh.

MR. ENGELHARDT: Mr. Chairman, I think it should be noted that there has been no opportunity for witnesses to offer any corrections to the transcript as yet and it may very well be that as Dr. Nelson has indicated in these two areas that there has been an error in the transcription presented in the record.

CHAIRMAN SKALLERUP: The Board will go off the record.

(Discussion off the record.)

CHAIRMAN SKALLERUP: Back on the record.

Mr. Engelhardt, we had a discussion regarding this AEC Staff Exhibit No. 7 and Dr. Winters would like to make a comment with respect to it, and then after that Dr. Jordan has a number of observations that he would like to make with respect to it.

DR. WINTERS: I would like to clarify the origin of this document and the representations that it represents the Oak Ridge National Laboratory.

Does it indeed represent the Oak Ridge National Laboratory's position? Or are these positions of individuals of that organization?

THE WITNESS: This work will appear in official Oak Ridge National Laboratory documents. The work on which this is based will appear in the documents.

DR. WINTERS: That is different.

What is this purporting to set forth then?

THE WITNESS: Well, what this is is another mathematical description which we feel is more realistic. It represents a dynamic approach to the study of radionuclide movements through the food chain. Whereas Dr. Tamplin's position is one where you obtain instantaneous contamination at the source, followed by transients through the food chain.

The net result being that in his model you have peak contamination, or his mathematical description, you have peak contamination initially, with a gradual decline in radioactivity through time.

In the model developed by Drs. Kaye and Booth, there is a gradual build up and when you integrate the areas under the curves, you see large differences in the dose to organisms and to man.

DR. JORDAN: Dr. Winters concern however was that this particular calculation was represented as an ORNL document and he thinks it would probably be fairer to represent this as a Nelson-Kaye-Booth document.

Would you be willing to so change the description?

THE WITNESS: We could, yes.

MR. ENGELHARDT: I think, Dr. Jordan, that in our description on the record earlier of this document that it was clear that Dr. Nelson and the other two had done the work with regard to this chart. We can easily delete from this identified exhibit the term "Oak Ridge National Laboratory" and identify it as Dr. Nelson's, as he participated in developing this model.

DR. JORDAN: All right.

Now then --

CHAIRMAN SKALLERUP: Well, let's do that.

What do you strike and what do you insert?

MR. ENGELHARDT: Mr. Chairman, I think the matter can be resolved by just striking out the words "And the Oak Ridge National Laboratory" and then we have a comparison of doses estimated by Tamplin's method from releases of maximum permissible concentrations. And the comparison would be with the wrk of Dr. Nelson and his associates.

I was not proposing to insert anything, but to have the witness sponsor this exhibit as his own work.

CHAIRMAN SKALLERUP: I am having difficulty with the word "comparison" if there is nothing to compare it with.

DR. WINTERS: And lines 12 and 15.

CHAIRMAN SKALLERUP: Where it says "ORNL."

MR. ENGELHARDT: Mr. Chairman, we may revise it to a comparison of doses estimated by Tamplin's method with the Nelson model.

CHAIRMAN SKALLERUP: And likewise on lines 12 and 15?

MR. ENGELHARDT: Likewise on 12 and 15, we would strike "ORNL" and insert "Witness Nelson."

DR. JORDAN: I know I am anticipating a little bit what is coming, but on the other hand under the assumptions here, it is one day exposure?

THE WITNESS: Tamplin specified the one day exposure, that is right.

DR. JORDAN: So therefore --

THE WITNESS: But he also multiplied by 365 to get an annual exposure. Just directly.

DR. JORDAN: I see. And you claim that the --

THE WITNESS: I say that this is wrong.

DR. JORDAN: Okay.

Now then this means that we will have to understand how you get from one step to the next and I am a little doubtful that we will be able to do this on just hearing you testify.

I am wondering if therefore a submitted sample calculation wouldn't be more effective.

THE WITNESS: Or supporting documentation.

DR. JORDAN: Or supporting documentation wouldn't

be more effective than just testimony as to how you get from one place to the next.

MR. ENGELHARDT: Mr. Chairman, to expedite this matter, could we defer on further consideration of this exhibit to permit Dr. Nelson to work out this explanation in a form that will be usable and understandable for the record?

Meanwhile this Staff Exhibit 7 has been offered for identification purposes only, not as an exhibit, and it will remain in that status, that limbo status, until we have an opportunity to present some further back-up material with respect to its contents, at which time we will offer that and further discuss that matter.

CHAIRMAN SKALLERUP: Very good.

MR. ENGELHARDT: Dr. Nelson then has completed hiw rebuttal at this stage. I think we are ready to call our next witness.

I would like to call as the next Staff witness Mrs. Edythalena Tompkins.

XXXX

DIRECT EXAMINATION

BY MR. ENGELHARDT:

Q Mrs. Tompkins, would you state your name and address?

A Edythalena Tompkins, Bethesda, Maryland.

Q Would you please state your present position and give a summary of your educational and professional qualifi-

cations?

A Well, as of last week I was transferred to the Envornmental Protection Agency where I am the senior radiation epidemiologist in the Division of Research, Radiation Office.

I received an A.B. degree in biochemistry in 1939 at Stanford University, California. I then did a year's graduate work in organic chemistry, followed by two years as a research assistant in the Department of Biochemistry.

For the next 10 years I was occupied full time as a mother.

Then I returned to graduate school and had two years work at the Department of Statistics at Stanford University in biostatistics, and since that time I have been working in radiation epidemiology.

The first five years were spent at the Stanford University Medical School, where I was involved in a study of children who had died of acute leukemia, as compared to children who had not, in which we were trying to determine what factors in the history of the children with leukemia might be associated with the disease.

We then moved to Washington and for the past nine years I have been with the Bureau of Radiological Health.

My continuing primary responsibility has been as the project director of a large scale study of persons who

were treated for hyperthyroidism with the radioisotope 131-I. We have 22,000 of them under study and we are comparing the long-term effects in these patients with some 14,000 patients who were treated by other means for the same disease.

In addition for the past two years I have been chief of the Genetic Study Section in the Epdemiology Branch of the Bureau.

Q Mrs. Tompkins, at page 1234 of the transcript this Board took official notice of Appendix 2, titled "Evaluation of the Possible Causal Relationship between Fallout Deposition of Strontium 90 on Infant and Fetal Mortality Trends," in Volume 2 of the Joint Committee on Atomic Energy hearings on the effects of producing electric power.

Are you the author of this report identified in Appendix 2?

A Yes, I am, in asscciation with Morton Brown, a colleague of mine.

Q Mrs. Tompkins, I would like to show you a copy of a document which is published by the United States Department of Health, Education, and Welfare, Public Health Service, Consumer Protection and Environmental Health Service. It is identified as document DBE-69-2 and is entitled "Evaluation of a Possible Causal Relationship between Fallout Deposition of Strontium 90 and Infant and Fetal Mortality

Trends."

Is this document which I am showing you now identical to that which is contained in Appendix 2 of the Joint Committee hearings?

A Yes, it is.

Q And you are the senior author, as you described, of this document also?

A Yes.

Q Mrs. Tompkins, could you tell the Board why this particular report was prepared?

A In the summer of 1969 Dr. Sternglass, Dr. Ernest Sternglass, sent to the Secretary of HEW, Secretary Finch, a copy of all of his papers and presentations which he had made up to that time on infant mortality trends.

As is usual when such a thing comes into a Secretary, it is referred down to the program level for evaluation. In this case a task force was formed consisting of people from the Bureau of Radiological Health, the National Cancer Institute, the National Office of Health Statistics, to prepare a report to return to the Secretary evaluating these documents.

I was assigned the primary responsibility of preparing the report, doing the analyses, and Mr. Brown, my assiciate, prepared certain of the analyses, and then when the report was in draft phase, it went back to all of the

members of the task force for review and when it was in its final form it was then returned to the Secretary.

Q Would you please summarize the content of this report?

A I would like to refer to the report at certain points in the summary. I think the Board members --

MR. ENGELHARDT: As a point of information for the Board, the Staff will at the conclsuion of this summary offer this document as a Staff exhibit. But we would like to have the witness summarize the content for the benefit of those who may be attending this hearing primarily. The report we are summarizing is identical to the report distributed to the Board and parties at yesterday's session of the hearing.

THE WITNESS: When we looked at the documents which Dr. Sternglass had submitted we found that in each of them he stated that his research had proven a causal relationship between the deposition of 90 strontium from fallout due to atomic weapons testing, and the decrease in the rate of downward trend of infant and fetal mortality. He also stated that his hypothesis was that the slower rate of decline was caused by an excess of deaths that resulted from genetic damage due to the incorporation of 90 strontium into the genetic material of the parents before the child was conceived.

Now the only data given in any of these documents

was in the form of graphs. Sf f necessity we were limited to analysis of these graphs. And we found that in one paper, the evidence for low level radiation effects on the human embryo and fetus which was printed in the proceedings of the Hanford Symposium on the Radiation Biology of the Fetal and Juvenile Mammal, May 5-8, 1969, contained a graphic presentation which was included in the other papers.

We, therefore, limited our analyses to this paper which seemed to contain everything that was in the other documents.

Now, the evidence which he presented was divided into three areas, leukemia, childhood leukemia, I should say, fetal mortality, and infant mortality.

I will discuss these in these three areas, the evidence, and simply give examples of the kinds of problems we felt were inherent in the data.

All of the leukemia data is based on numbers of cases. The primary data is various plots of distribution by time, by age, and so forth, of 55 cases of acute childhood leukemia, I should say 55 deaths of acute childhood leukemia in the years '52 to '62 in children age zero through 15 in the Troy-Albany area of New York. These children were alleged to have died as a result of the exposure to the rain fallout debris in April of '53 in this area. And it was stated that there was an increase in the rate of leukemia following the rain fallout.

Now all of this data was presented independent of the underlying population at risk. And it is almost impossible to look at cases over a time period and determine that there is an increase in rate of death.

For example, if you observe seven cases per hundred thousand in 1950 and --

Excuse me, I gave it to you as a rate.

If you observe seven cases in 1950 and if you observe 14 cases in 1960, the rate has not doubled if the population from which these children came has doubled. Seven per 100,000 children is exactly the same rate as 14 children, deaths, per 200,000 children. So that looking at cases alone is very misleading, adn very difficult to interpret.

In addition, when you talk about 55 cases divided over 10 years, you are talking about an average of 5.5 cases per year.

With small rates of this kind, with small numbers, wehad very large errors, statistical errors, on what the true rate really is.

Consequently, to make any statement about changes from year to year is virtually impossible.

To get around this problem of the small numbers, Dr. Sternglass then went on and looked at all of the deaths from acute childhood leukemia in the State of New York. Of course with a much larger population he had many more cases. I would like to refer to figure 5 on page 8 --

End #4

#5 DB-1

MR. ENGELHARDT: Mr. Chairman, at this juncture since reference is being made to this report specifically, we would like to have this report previously identified to be identified now as Staff exhibit 8, just for identification.

CHAIRMAN SKALLERUP: It is so ordered.

(The document referred to was marked Staff Exhibit 8 for identification.)

xxxxxx

THE WITNESS: Dr. Sternglass went on and said the fall-out in New York which of course was from other than the Troy rain-out, had caused an increase in rish of leukemia in the whole state of New York.

In this plot which is somewhat confusing you will notice there are three different distributions, all on the same scale, I should say, and all again by numbers of leukemia cases, not by rates.

The top group is the total number of leukemias in the age group zero through 14, with the base line at approximately 153 which he said was the average number of deaths in the years 1950 to 1954 in New York, stating that this was the time before the effect of the fall- out could be seen in leukemia.

The next distribution down on the graph is a break-out of the children 5 to 14, through 14 years of age, and the

DB-2

bottom group are the zero to 1 year olds.

He also has entered the dates of various testing programs, both at the Nevada test site, the USSR, US and UK large tests, and if you notice there is an off-set from the time of the tests going over five years, approximately five years, in which he states it takes five years from the time of the fall-out for its effect to be seen in leukemia.

I think in this plot the problems with working with cases alone is quite evident. For example, the maximum number of cases which he shows in any year is 209 cases in the State of New York. And yet the population at risk between 1950 and 1960 -- that is children zero through 15 years of age -- has increased in New York State in that period of time by 38 percent.

So if you take a simple 38 percent of his average of 153, you would expect to see 206 cases of leukemia, which is about what they did see, 209 cases.

So there is no evidence here for any increase in leukemia in the New York area in this period of time.

I would like to just briefly read one paragraph which was our summary conclusion on the leukemia data.

"The dangers in postulating the existence of an association based on comparisons of number of cases observed over the period of time is well recognized by epidemiologists. Unless the cases can be related to a base population, from

DB-3

which the cases were derived, and comparisons then made of the observed rates, or unless the base population has remained static, no trends of change over time can even be assumed.

"Even if such a trend could be demonstrated, an increase in an exposed population" -- and I say exposed in quotes -- "without a different pattern in a 'no-exposed' population fall short of even demonstrating an association let alone a causal relationship."

Now the fetal and infant mortality data. We used one methodolgy in all of the analyses. We divided our evaluation of this data into two parts, one the methodology itself, and two, ignoring our criticisms of the methodology and accepting his methodology, we looked at the completeness and the consistency of the data in support of his hypothesis.

Now the methodology which he used is the following: He assumes that a logarithmic transformation of the infant or fetal mortality observed rates from 1935 to 1950 is linear. And if you extend this line this is what we would have expected to see for the next 16 or 17 years if fall-out had not been around.

We have plotted in Figure 6 on page 9 the logarithmic transformation of the infant mortality rates and the fetal mortality rates for the United States from 1922 through 1966. Excuse me, the fetal mortality through 1966, and infant

importantly these reporting requirements havechanged within states over the period of time which are used.

For example, you will see on this plot of Figure 6 that there is a break, there are two lines for fetal mortality. The top one is all gestations, meaning all periods which were reported. That is not even reported after 1960. In 1945 the National Office of Vital Statistics changed their requirement and they now report fetal mortality of gestations of 20 weeks or more.

As far as we can tell Dr. Sternglass used a cross-over from the line of all gestations to get the data that he needed for the lower gestation periods. These are two different reporting bases.

With these problems with fetal mortality, the base data, which incidentally are reported in every publication of the fetal mortality ratios by the National Office of Vital Statistics, all of the conclusions are somewhat suspect, shall we say.

But I would like to look at one figure as an example of the kinds of things we did in looking for completeness and consistency of the presentation, independent of the methodology.

DR 6
rms 1

For example, I would like to refer you to figure nine on page 13. That is a reproduction of the figure presented by Dr. Sternglass in his paper, and we used the galleys to prepare these graphs. This plot shows fetal death rate in California versus New York State and is stated to show that the New York trend began to change in '50 to '52 due to fallout from the Nevada test site, whereas California did not change until '56 to '57, because it did not get fallout except after the start-up of Pacific testing.

I am sorry it is on the next page, but if you look on the next page, page 14, Figure 10, you will see the plot from the same data which we prepared. We did a least squares fit line, whereas Dr. Sternglass put an eyeball fit line on.

And in addition, we included all of the data for New York State. If you look on the first plot, Dr. Sternglass' plot, for some reason he left out all of the points between 1938 and 1945.

If you look on ours, we include this data. And to state that any line from these figures from 1935 to 1950 could project what could happen in the future, of course, is inane. There is just no way of doing it.

The California data, however, was complete, and with a fitted line it appears the change in California started

in the very early 50's, not in '56 to '57 as you would look at the plot of Dr. Sternglass with his eyeball line. The infant mortality data is quite complete and does not have the problems inherent in the fetal mortality data. All of the data which Dr. Sternglass presented are plots comparing his calculated excess deaths. And to avoid having to say this every time, whenever I say excess deaths, this is Dr. Sternglass' calculation of what if the mortality trends had continued on the same line they had been going on, then how many more children would be alive, is actually the way you would say it.

But those are not excess deaths, these are excesses over his projected line. I don't want to have to qualify this "excess" every time I use it. All of his data is a comparison of these excess deaths versus various distributions of strontium-90 in teeth, in milk, deposited and so forth.

Now the first data, figure 17, page 20, in which he is comparing the excess deaths for the State of Missouri with the data of -- data against the data of strontium-90 in teech. These are in decidious second molars of children, as collected by Dr. Rosenthal at St. Louis starting in 1947.

If you look at this plot, it does appear to show there is a close correlation between the rise in his

rms 3

excess infant mortality and the strontium-90 in teech. However, I would like to point out that the 1951 point of excess seems to have been ignored in the line of his trend line of the excess.

And unfortunately Dr. Sternglass also seems to have made a fairly simple arithmatic error in that he changed his baseline, he calculated, he got the slope of his projected line by extending from 50. But then he wanted to set it back to 47, and instead of moving the slope of the line and recalculating all of his excesses, he simply subtracted the percentage of 1947 from his percentages of 1950 and subsequent years. And his percentages are a ratio. If they are based on different denominators, you cannot subtract or add them and have any meaningful term.

We redid this data intending to correct the arithmatic error, but we also found that St. Louis infant mortality over these years was not the same as the State of Missouri. As all of these teeth had been collected in the St. Louis metropolitan area, we felt that the St. Louis data, if his hypothesis was true, should fit the strontium-90 even better than the Missouri data.

So we did the infant mortality excesses, using his model and his methods, for the St. Louis City-County area, and we plotted this against the strontium-90 in teeth.

rms 4

Now, as the infant mortality data was only available from 1942 on, we also did the Missouri data using 1942 to 1950 as the baseline rather than 1935 to 1950.

If the hypothesis is correct, that past predicts future, it should not matter what years you use for this baseline. And in fact the closer the better. So that the '42 to '50 should not change the projection.

I told you this was a very sensitive indicator. If you will look at figure 18 on page 21, the line with the circles in it is the Missouri excess deaths calculated by Dr. Sternglass based on the years '42 to '50 projected line. And you will see that it is very, very different from that on a '35 to '50 base.

There seems to be little explanation for the St. Louis City-County mortality as compared to the strontium-90 data. You will notice as the strontium is increasing, his excess deaths go down below zero. And then they increase very rapidly and then they plateau out again independent of the even, smooth increase of Strontium.

Perhaps the plot which appears in every paper which Dr. Sternglass perhaps gives, it is the one plot which appeared in all of the papers which Dr. Sternglass presented to the Secretary, the maps of the United States, two maps, one in 1946 and one in 1959. This plot is figure 24 on page 28. Dr. Sternglass states, and I quote, "The

rms 5

three to five-year delay in the peak of infant mortality which suggests the genetic rather than a direct somatic effect is best shown in the changes of the rates of mortality in the Southern, downwind states following the first atomic weapons test in Alamogordo, New Mexico in July, 1954."

Now to explain this plot for a moment, Dr. Sternglass calculated his excess, and because he was looking for the effect of Alamogordo testing, he used the baseline 1935 to 1945, which of course is perfectly proper.

He stopped his projections at 1945, but he only used 1940 to 1945 as his projected line. He then calculated the excess or deficit in deaths, in infant mortality, for each state and he has plotted 1946 for all of the states and 1950 for all of the states.

The shaded states are those which show an excess of more than five percent. Now in his 1946 plot he shows that Montana and North Dakota have an excess of five percent -- I shouldn't say that -- more than a 5 percent excess of deaths.

In the 1950 plot Montana and North Dakota still have this excess. But Texas, Arkansas, Louisiana, Mississippi, Alabama, Georgia, South Carolina and North Carolina show the same, more than 5 percent excess. He states that this is due to the fact that the fallout from Alamogordo went across the southern part of the United States and the gradual build up in the genetic material of the parents resulted five

rms 6

years later in this excess in infant mortality across the southern states.

If this hypothesis is true, you would expect to see a very gradual build-up of excess infant mortality in these lower states, with nothing showing in the first years and then gradually building up to the plot which he has shown in 1950.

We calculated these excesses for all of the 48 states plus the District of Columbia for each year from 1946 through 1951. We found some interesting things. For example, in 1947 Arkansas and Louisiana already showed an excess of five percent or greater. But Montana no longer did.

In 1948, Texas, Mississippi and Alabama joined the five percent or greater group, but so did Wyoming and South Dakota. In 1949 Georgia, South Carolina, North Carolina passed the five percent level, as did Virginia and Vermont while South Dakota was no longer at 5 percent any more.

In 1951, the year after the plot he shows, Virginia was again at the 5 percent level. Thus, only the year 1950 shows the distribution of states with percent of excess mortality of 5 percent which Dr. Sternglass used as the demonstration of a three to five-year delay from the arrival of fallout to a five percent excess infant mortality.

rms 7

No explanation other than radiation from fallout is suggested by Dr. Sternglass for these excesses, including even the '46 excesses in Montana and North Dakota. In order for the Sternglass hypothesis of the three to five-year delay effect to stand unchallenged, some sources of radioactive strontium in Montana and North Dakota must be identified in the years 1941 to 1943. The Hanford Washington reactor did not go into operation until 1944. The first atomic weapons test in 1945 could not explain the changes in Arkansas and Louisiana as early as 1947 and still other sources would be needed to explain the changes in Vermont, South Dakota, Wyoming and Virginia.

We are unaware of any such sources of strontium at that period in time in these areas. In epidemiology we make hypotheses and we say that all of the data must be consistent with the hypothesis or we must change our hypothesis to fit all of the data.

We were also interested as to why Dr. Sternglass change his line from '35 to '45 to '40 to '45 in this particular plot. Being curious epidemiologists we did it. So we did the same thing exactly using '35 to '45 for all of the states for the periods of time and the results of this analysis are shown in figure 25 on page 30.

end 6

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The sensitivity of the base line I think is well demonstrated by this plot. For those of you who do not have the plot, I would like to state that in 1946 there were five states with an excess of five percent: Washington, Idaho, Montana, North Dakota, and Texas. And in 1950 the states were Utah, Idaho, Montana, North Dakota, Minnesota, and Wisconsin.

If you use the '35 to '45 base line, there is no excess across the southern part of the United States at all.

The last piece of data which was presented was milk data and the comparison of excesses with milk.

Now the milk data which Dr. Sternglass used was gathered by our Bureau in the so-called milk network. There was a raw milk network which started in '57 and '58 with ten stations and then in 1961 and 1962 it converted over to a pasteurized milk network and went to 62 stations.

This milk network was to monitor the amount of radioactivity in the milk. The raw milk network was based on sampling the milk at the producer. The pasteurized milk network was based on monitoring the milk at the consumer level, that is, it was taken from shelves in grocery stores.

Now there were nine stations which had been continuously in the network since it started in 1957. However, analyses reported in the Bureau Rad Health data reports, which incidentally Dr. Sternglass referenced as his way of putting

DB-2

together the raw milk network and the pasteurized milk networks, specifically state that two states, Missouri and Georgia, can not have the data put together. The milk sheds are so different that the data is not comparable. Where, therefore, Dr. Sternglass got his data for those early years in Georgia and Missouri I don't know. But it is presented.

He used four-year moving averages. He took all of the measured activity in the state for a four-year period and then moved the average along. He did this to try to get an average dose showing to the parent before the infant mortality to allow for this three to five-year delay in the effect.

Now if you look at the plots on page 31, figure 26, this is the data presented by Dr. Sternglass and indeed some of these states show a very close relationship with the moving infant mortality, excess infant mortality and the moving four-year average in milk.

Again, being curious, we wondered about the three states in the network which were not shown. And unfortunately Dr. Sternglass made his same arithmatic error of subtracting percentages. So we went back and for those states in which it was valid to put together the raw milk network and the pasteurized milk network, we plotted all of the data. This is shown in figure 27.

Now as there are some differences with the correction of the arithmatic error in the four states. Utah, Illinois and New

DB-3

York and Texas, which are valid, there are still some reasonably close relationships. However, the three states which he did not show, which are Ohio, California and Washington, seem to be quite different.

For example, Washington has one of the highest concentrations of strontium 90 in the milk of any of the states which we have continuous records on. And yet it has the lowest excess infant mortality of any of the states used in this comparison.

On the other hand, California has the lowest strontium 90 of any of these states in the milk, and it has one of the highest rates of excess infant mortality.

In summary and in the discussion we made the statement: "Although all of the evidence which Dr. Sternglass has presented to support an association between 90 strontium deposition and a decrease in the rate of decline of infant and fetal mortality in the United States has failed to stand up under careful scrutiny, the important implications of such an association, if true, warrant some further investigation."

We felt that although Dr. Sternglass' data could be knocked down, that it was incumbent upon us to do anything that we could do to try to see if this association was indeed a valid one. We made the hypothesis that if the changes in the infant and fetal mortality, the infant mortality,

DB-4

I should say, which we will limit ourselves to, were due to a common factor, that is, fall-out of strontium 90 in particular, then you would expect to see certain things occurring simultaneously.

For example, you would expect to see the neo-natal and the post-neo-natal mortality rates changing at the same time. In those states which have large enough non-white populations to be reported separately, you could also hypothosize that the non-whites and the whites should change at the same time.

In other words, what we were doing was trying to cut the populations within states into different groups to see if they were behaving commonly.

However, we realized that the main entry of strontium 90 into the human is via milk, and we felt that it was perfectly possible that the socio-economic status of the non-whites could mean that they got a lower level of milk and therefore they might change at a later period of time.

These analyses were primarily done by Mr. Brown, my co-author, but the table and the results of these analyses are shown on page 35.

Looking at the neo-natal and post-neo-natal data, those entries with dashes in them indicate that there was either no change in the period from 1935 through 1967 in the trend of infant mortality deaths, or that there were multiple changes.

DB-5

DR. JORDAN: Could you explain to me neo-natal and post-neo-natal?

THE WITNESS: Sure. Infant mortality are the number of deaths which occur within the first year. Neo-natal is within the first 28 days; post-neo-natal is from 29 days through 365 days.

Now although some of the states did change then at the same time neo-natal, post-neo-natal, some of them changed at quite different times, and in two states there was as much as four years difference when maximum change in trend occurred. These two states were Michigan and South Carolina, in which there was a four-year difference between the neo-natal and the post-neo-natal change.

It is interesting that they are just the opposite, one was 49 in '53, the other is 53 in '49.

We also looked at the white versus the non-white population. Contrary to our hypothesis that the non-whites might be later because of their lower consumption of milk, the non-whites -- I should say there are 26 states which have a 10 percent non-white population and therefore their statistics are reported separately. Of these 26, in 14 states the non-whites preceded the whites and in one state by as much as five years. That state is Oklahoma in which the change in trend occurred in 1946 in the non-white population and did not occur until 1951 in the white population.

DB-6

As a final effort to check out this hypothesis, we did one more thing. It was stated that complete deposition of strontium 90 records going back from the early fallout days are available for 13 states.

We hypothosized that the accumulation of excess of deaths, if you accumulated these excesses by years, should correlate with the accummulation of strontium.

For the 13 states we did the accumulated excess deaths, as calculated by Dr. Sternglass' method, and the strontium 90. The correlation coefficient is minus zero point zero 15.

I would like to say that this is about as close to a no-relationship as you will ever get in a correlation coefficient. Zero indicates none.

In summary, our summary paragraph for this paper which was presented to the Secretary says:

"The rate of decline of infant mortality in the United States did change around 1950.

"This lowering of the rate of decline has been a concern of many people working in the public health field. Many careful studies have been made and no single factor nor group of factors have been identified which explain this change.

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"While the hypothesis that this change is a result of 90 strontium deposition from fallout is an interesting one, the data presented do not appear to indicate any relationship between the change in rate of decline of infant mortality and the deposition of fallout in the United States."

MR. ENGELHARDT: Mr. Chairman, I would like to ask that the report which has been identified as Staff Exhibit 8 be admitted as Staff Exhibit 8 in this proceeding.

CHAIRMAN SKALLERUP: Would you please read back what Mr. Engelhardt said?

(The Reporter read the record as requested.)

CHAIRMAN SKALLERUP: As evidence?

MR. ENGELHARDT: As evidence.

CHAIRMAN SKALLERUP: It is so ordered.

(The document referred to, heretofore marked Staff Exhibit No. 8 for identification, was received in evidence.)

XXXXX

BY MR. ENGELHARDT:

Q On page 1296 of the Transcript, Dr. Sternglass has stated that he was the only one engaged in certain types of epidemiologic studies. Do you agree with Dr. Sternglass' statement?

A No. In addition to the studies which we have made there are two other studies which have already been published

ln2

on the relationship of strontium-90 and infant mortality. One is 90 strontium in infant mortality by Patricia Lindlop and J. Rotblat of the Medical College of St. Bartholomew's Hospital, London, which was published in "Nature," Volume 224, December 27, 1969.

Another one, 90 strontium in infant mortality in Canada, by R. F. Shaw and A. P. Smith, Department of Preventive Medicine and Pediatrics, Dolhauser University, Halifax, Nova Scotia, which appeared in "Nature," Volume 228, November 14, 1970.

These studies are based directly on the types of analyses and studies which Dr. Sternglass has done. In addition, there are a great many other definitive epidemiology studies going on and have been going on for some year in some cases in an effort to determine the long-term effects of internally deposited isotopes in the human body.

Q Mrs. Tompkins, one last question with regard to your response. Do the reports that you specifically identified relate to the type of studies that Dr. Sternglass has performed, or do they reach any similar conclusions as does Dr. Sternglass in his studies?

A I can read the final summary paragraph of the Lindlop-Rotblat paper. I should say the final sentence. "In summary, none of the evidence given by Sternglass stands up to objective analysis and we must conclude that there is

ln3

no justification for linking the break in the downward trend of the infant mortality curve with strontium fallout."

Now, the Canadian paper used Dr. Sternglass' method and compared it with the findings in infant mortality in Canada. And their conclusion, "The strontium-90 in the mortality rates in the various states," which we call provinces "do not rise together. If anything, they show a slight negative association. The regression coefficient is minus 0.81."

end 8

DB 9/rms1

MR. ENGELHARDT: That completes this witness' testimony.

CHAIRMAN SKALLERUP: Dr. Tompkins, do you recall whether the Canadians made any studies which indicated the -- let me first refer to figure 25. What hits me here is that the Border States --

THE WITNESS: They do it by Provinces.

CHAIRMAN SKALLERUP: Was there any geographical similarity with their southern provinces and our northwestern states, as it were?

THE WITNESS: Review my geography for me.

DR. JORDAN: She wants to know what the provinces are. Alberta --

DR. WINTERS: Saskatchewan, British Columbia.

THE WITNESS: They have quite close depositions of strontium 90 in those three places and that is where their excesses are, interestingly enough, right in the same place, going up 5 to 25 percent, whereas in New Brunswick, for example, which has the highest strontium deposition, their excesses run to minus 50 percent. This is where I assume he put all of his data together for all of the Provinces and then got the correlation coefficient. It is interesting.

CHAIRMAN SKALLERUP: Thank you.

DR. JORDAN: I had a question or two. Is there any reason that you know of for expecting a logarithmic,

rms 2

straight line logarithmic decrease in infant mortality?

THE WITNESS: There is every reason not to expect it. You mean to be linear?

DR. JORDAN: Yes. In that connection then can you say what is the experience in other countries in this respect, Scandinavia, France, anything like that? Have you looked at that data?

THE WITNESS: Yes, I have looked at every place where I could get strontium data around the world. We compared their excesses calculated in this way. It completely depends, of course, on the general development of the country. For example, as our infant mortality -- and in fact all of Europe and so forth has done it, it has flattened out as it gets lower and lower and lower, even on a logarithmic plot.

Japan, on the other hand, which interestingly enough has had probably the highest concentration of strontium-90 anywhere, because of the Russian tests, is going down much steeper than linearity, as the country has developed since World War II. And this is probablywhat you are seeing more thananything else. So that the shapes of these curves will change very dramatically, depending on the development of the country. This reflects also the reporting, of course. It is an oversimplification, even for a short period of time to assume a semi-logarithmic transformation.

end 2 9

DR. JORDAN: Is linear?

THE WITNESS: Is linear, I am sorry.

DR. WINTERS: Mrs. Tompkins, if you go to this figure 6 in this report on page 9, all of the discussion today has been concerned in extrapolating this curve in a forward direction.

Has anyone tried extrapolating this curve in a reverse direction back into the 1800s?

THE WITNESS: Yes, sir.

DR. WINTERS: Because if you take this curve and extrapolate it backwards, it gets to where nearly everybody would die as an infant.

THE WINTESS: That is right, none of us would be here, there is no question about this.

CHAIRMAN SKALLERUP: What conclusions do you draw from this?

DR. WINTERS: There is something wrong with extrapolations in either direction from data.

THE WITNESS: In normal National Office Vital Statistics analyses, for example, they will take trend lines over five-year periods, because the variability within one year, comparing two years, side by side, can be very great. If you have a flu epidemic, for example. So they normally use five-year trend lines as a measurement of how are we doing in public health, you know, is the general trend

down, is the general trend up. But it is a measurement of what is happening today in a currently moving situation and not projecting at all what is going to happen tomorrow or what happened yesterday.

In our own country, for example, New Mexico is continuing to go down very rapidly, but it started out very high.

DR. JORDAN: In addition to the infant mortality studies for United States, Dr. STernglass also reported on infant mortality studies in the neighborhood of Dresden reactor, and I am going to ask you, are you or someone else going to address themselves to that question?

MR. ENGELHARDT: Yes, sir. We will have two witnesses who have prepared detailed records with regard to that material and they will be our next witnesses following Mrs. Tompkins.

DR. JORDAN: Thank you.

CHAIRMAN SKALLERUP: Thank you, Mrs. Tompkins.

We will break for lunch and resume at 2.

(Whereupon, at 12:30 p.m., the hearing was recessed, to reconvene at 2:00 p.m., this same day.)

End #10

DB11 1nl 1

AFTERNOON SESSION

(2:00 p.m.)

CHAIRMAN SKALLERUP: Will the hearing please come to order.

Any further word from Mrs. Bleicher?

MISS EVANS: No, I couldn't reach her.

MR. ENGELHARDT: The Staff's next witness is Dr. Bernd Kahn. Dr. Kahn has been sworn this morning and is now under oath.

DIRECT EXAMINATION

BY MR. ENGELHARDT:

Q Would you please state your name and address?

A Bernd Kahn, Cincinnati, Ohio.

Q Would you please state your present position and give a summary of your educational and professional qualifications?

A I am with the Orange Engineering Laboratory of Radiological Engineers, Office of the ENvironment Protection Agency. I have a Ph.D. in chemistry, I have been involved with studying radioactivity in waste disposal of radioactivity, radioactivity in fallout and radioactive effluents from nuclear power stations since 1951, first at Oak Ridge National Laboratory, then with the Public Health Service, and for the last few weeks with the Environmental Protection Agency.

ln2

Q Dr. Kahn, are you the senior author of a report entitled "Radiological Surveillance Studies at a Boiling Water Nuclear Power Reactor," identified as BN-DER 70-1, a Public Health Service publication, which has been put into evidence by the applicant as Applicant's Exhibit No. 10?

I show you a copy of that document.

A Yes.

Q Is this report used by Dr. Sternglass as the basis for his radiological exposure analysis contained in a paper by Dr. Sternglass entitled, "Infant Mortality and Nuclear Power Generation," dated October 18, 1970, which has been put into evidence by Intervenor LIFE as their Exhibit No. 2?

A Yes.

Q Do you agree with Dr. Sternglass' analysis of your data?

A I have certain differences with Dr. Sternglass' analysis of the data.

Q Would you explain those please?

A Yes. In brief, Dr. Sternglass tries to show a connection between the radioactivity released from the stack of the Dresden nuclear power station and the increase in infant mortality in the entire state of Illinois, an increase in infant mortality data in certain counties near the station, between 1964 and 1966, and in passing mentions I think some

ln3

other things.

He uses data from our measurements and I think tries to show three things with these data. First, and mainly, he refers to the external radiation due to the radioactive gas discharged from the stack. In passing he mentions the inhalation effect of radioactive gases and also in passing mentions internal exposure due to radiation from other radioactive substances, such as the radioactive particles that are the daughters of these radioactive gases.

Now in respect to the effect from these radiations, which Dr. Davis will discuss later, one has to consider that there is a natural radiation background, external radiation background of approximately 90 millirem per year and one has to put any external radiation from the gas in perspective relative to that.

With regard to the inhalation of radioactive gases, one has naturally occurring radon-222, which is always present, and one has to put the radiation from the radioactive gas from Dresden in perspective to that.

Finally, with regard to internal exposure to radiation from radioactivity particles and other radionuclides, other than the radioactive noble gases, one has radionuclides from fallout as a background relative to what comes out of Dresden.

end 11

Q Dr. Kahn, are you the co-author of an article entitled "A Critical Review of Infant Mortality and Nuclear Power Generation," by E. J. Sternglass?

A Yes.

MR. ENGELHARDT: May I have this report identified as Staff Exhibit 9.

(The above-mentioned document was marked Staff Exhibit No. 9 for identification.)

BY MR. ENGELHARDT:

Q Would you summarize the contents of the report with respect to the portion of the report which you authored?

A Yes. The main argument of Dr. Sternglass is that the external radiation exposure is between 114 and 340 MR per year, which of course would be a considerable increase over the natural radiation background. He bases this on measurements which we made over the period of less than an hour several times at the centerline of the plume of the gas or beneath the plume of the gas emitted by Dresden.

We reported these values in terms of micro-roentgen per hour, and Dr. Sternglass multiplies the hour numbers by the number of hours per year of approximately 8800.

This is clearly a mistake, since the wind direction is, of course, variable at Dresden, as in most other places,

and wind does not come sufficiently close to any one point with sufficient frequency during the year to permit one to multiply the instantaneous maximum plume dose by the number of hours of the year. A more appropriate value would be approximately 1/50th of the number Dr. Sternglass uses. This takes into account the fact that an 8 degree wind is in an 8 degree sector at the point of measurement duirng 1968 when we did this study, approximately 3 percent of the time, and also that the station was only operating two-thirds of the time in 1968.

As a result of this, in 1968 our measurements would show that at these locations, within one mile of Dresden, the values were between 2 and 7 milliroentgen per year, and at a distance of approximately 12 miles from Dresden the value was approximately .4 milliroentgen per year rather than his number of approximately 20 milliroentgen per year.

The secondpoint I would like to make, in view of the fact that Dr. Sternglass points out that a large fraction of the population in Illinois is within a 50-mile radius of the station, that most of this population within a 50- mile radius is more than 12 miles distant. Chicago is roughly 50 miles from Dresden, northeast of Dresden.

And at the location near Chicago, where most of the population would be exposed, external radiation would be considerably less than even 4/10ths of a milliroentgen

rms 3

per year, or would have been that in 1968 when we made the study.

I would also like to mentior that with regard to the noble gases, the same thing applies in the Chicago area; namely, by the time the plume would reach Chicago, the concentration of the xenon, radioactive xenons and kryptons, would be considerably less than the 100 to 400 picocuries per cubic meter which is the natural radon-222 level in air near Chicago. I use Argonne National Laboratory Data there. The passing reference to radionuclides other than the noble gases also should be mentioned. This should not be meant to imply that the dose from these radionuclides is significant.

We have made measurements and we have published calculations based on these measurements of the radioactive iodine coming out of the stack, for example, radioactive tritium, some of the cesium-137 and strontium-89 and strontium-90, daughters of these gases, and in every case the radiation is considerably less than the radiation to the people. It is considerably less than the radiation from the gases.

This should not mean that one ought not continue to look at these individual radionuclides coming out of nuclear power stations very carefully. But at least in every case that we have seen so far at Dresden, there has

not been any significant radiation exposure from these other radionuclides. Finally, as has already been mentioned, I believe, I think Dr. Sternglass reversed wind directions, his idea of the direction the wind was blowing at Dresden, in that the county which is actually downwind, if one can use the term, from Dresden -- which I believe to be an erroneous use of the term, really -- Will County has a very small increase in infant mortality rate between 1946 and 1966, 5 percent according to Dr. Sternglass, while Livingston County, which he considered to be downwind, but which is actually upwind in this kind of frame of reference, had I believe 140 percent increase according to Dr. Sternglass.

Now, the reason I believe one should not even use these terms is that, again like many other places, the wind essentially goes in all directions some of the time during the year, and while it may go in the downwind direction twice as frequently as the average, and in an upwind direction half as frequently as the average, let's say, nevertheless every point from Dresden is to some degree downwind.

MR. ENGELHARDT: In connection with Dr. Kahn's testimony, we would also like to have as a witness Dr. A. K. Davis, who was the co-author of this paper.

At this point I would like to ask him some questions, at which time I will offer the exhibit which I

have identified as Staff Exhibit 9 into evidence.

CHAIRMAN SKALLERUP: Including the Sternglass article?

MR. ENGELHARDT: The Sternglass article is already in the record as the Intervenor LIFE's exhibit No. 2, I believe. It might also be noted that the article itself, which is LIFE's Exhibit No. 2, is appended to the copy of the article which has been co-authored by Drs. Davis and Kahn and is included in the -- I believe -- in the material which we have previously, yesterday, given to members of the Board and to the parties.

end

CHAIRMAN SKALLERUP: How do you reconcile this title, or is it completely relevant "The Testimony Presidented by A. K. Davis and E. Howard"?

MR. ENGELHARDT: We are not offering that. This is a copy of material that has previously been offered as testimony in another proceeding. We are offering the content of the document. If you have a document with a cover sheet on it, the cover sheet should be stripped off. We are not dealing with the cover sheet.

CHAIRMAN SKALLERUP: That helps.

MR. ENGELHARDT: I think there were some copies that had no cover sheet and I believe there were some with the cover sheet, depending on where we got our source of supply.

CHAIRMAN SKALLERUP: Then we won't have to duplicate the Sternglass article.

MR. ENGELHARDT: That is correct.

CHAIRMAN SKALLERUP: As part of your exhibit?

MR. ENGELHARDT: That is correct. We are just dealing with the material that has been authored by Drs. Davis and Kahn.

XXXXX

DIRECT EXAMINATION

BY MR. ENGELHARDT:

Q Dr. Davis, would you please state your full name and address?

A A. K. Davis, Great Falls, Virginia.

Q Would you please state your present position and give us a summary of your educational and professional qualifications?

A I am currently chief of the Epidemiology Section, Division of Biological Effects, Bureau of Radiological Health of the Public Health Service.

I hold a B.S. in chemistry from Memphis State University dated 1947. And a Ph.D. in physiology from the University of Tennessee Medical School in Memphis, not in Knoxville, where I worked initially on ion transport of radionuclides, sodium 24 and potassium 42, in animals and in man.

My thesis was on the effect adrenal cortical hormones on ion transport.

In 1952 I went to the Navy Radiological Defense Laboratory, where I was employed in radiobiological research on weapons effects and worked with neutrons, X-rays, beta rays, reactor gamma, and visible light.

And in 1961 I was employed by the Bureau of Radiological Health, where I was director of an experimental laboratory with both in-house and contractor laboratories.

I am currently chief of the Epidemiology Section.

Q Dr. Davis, have you co-authored with Dr. Kahn the document which we have identified as Staff Exhibit No. 9,

which is entitled "A Critical Review of 'Infant Mortality and Nuclear Power Generation'" by E. J. Sternglass?

A Yes.

Q In particular have you revieved in your report the epidemiological findings presented by Dr. Sternglass in that paper?

A Yes, I have.

Q Do you agree with his findings?

A No, there are several places where I have marked reservations and differences.

Q Would you then summarize the content of your paper with respect to that portion of the report which you authored?

A Yes, I will.

If you have a copy, I am going to talk from page 3 which is entitled "Epidemiology."

Sternglass' evidence of serious health effects from the emissions of the Dresden reactor consist of an analysis of the changes in infant mortality and respiratory disease deaths, except pneumonia and influenza for all ages. His initial evidence is that infant mortality in the State of Illinois is greater than that in New York.

Our contention is that New York is not an adequate comparison state for Illinois, as shown by the infant mortality data from 1955 through 1961.

Furthermore, the principal population potentially exposed is metropolitan Chicago, not the State of Illinois. And when the City of Chicago is compared to a control city with similar characteristics, St. Louis, the total infant mortality for both areas follow the same pattern. At least part of the difference between Chicago and New York City is related to the fewer non-white births in New York.

Now if I could develop some of the ideas that are presented here. Instead of looking at New York as a control, we have chosen St. Louis primarily because there is a marked difference between the infant mortality in white and non-white portions of the population. This is probably a socioeconomic difference that may be related to nutrition, it may be related to the availability of the current health benefits, availability of hospitals and so forth. But this difference is of the order of magnitude of a factor of two. That is, the infant mortality in the white population shows values of between 20 to 25 deaths per thousand live births as compared with 40 to 45 for the non-white population.

Consequently, if the ratio between the white and non-white births is changed, this changes the infant mortality. And this is in fact what has occurred in comparing New York and Illinois. It is presented in figure 2 of the appendix if you would like to consult that.

In New York City the percent of non-white to total births risesfrom about 22 to 30, whereas in Illinois, specifically for Chicago, the percentage of non-white births begins at about 40 and rises to about 45 from 1960 to 1968.

Now it is not, would not be fair to say that this is the total picture, because if you examine the infant mortality rate for the non-white population, you will find there is a significant change in 1963, that runs from about 1963 through about 1966. And this is a period in which the Dresden reactor is putting out a significant -- the peak of the gaseous discharge.

Now Sternglass suggests that a relationship exists between the reactor effluent and the difference in infant mortality between New York State and Illinois. And he presents a plot of the excess, that is, the difference between New York State and Illinois, as though if New York State were zero, then the excess would be New York State minus the infant mortality in Illinois.

Now this interpretation is questioned because of two factors. First, the range of values is large; that is, it goes from minus 0.7 to plus 3.4 deaths per 1,000 live births. And this maximum and minimum occur at the single effluent level.

Secondly, a year's lag would be expected if infant

mortality resulting from in utero radiation is reflected in the year following birth, that is, the dose is accumulated over a period when the mother is carrying the infant.

Consequently, you would expect that the majority of the effect would be seen in the year after, but since the information is presented per year, this would prejudice the results somewhat. But still the year following the radiation would be the peak, would be closer to the dose than would be the year in which the radiation actually occurred.

On this basis a comparison of the curves for emission and infant mortality shows that the 1961 emission peak is followed by a fall in in fant mortality in Illinois and the subsequent rise proceeds the peak emission, whereas the fall occurs at the peak discharge. That is, there is no correlation between emission and infant mortality if the in utero irradiation period is the critical period as Sternglass suggests.

In addition to looking at the state, Sternglass has also looked at the counties surrounding the reactor. He analyzes the Illinois counties with the highest potential exposure.

Now here the number of infant deaths are few, and consequently one must consider several years to determine the basic rate for infant mortality. Sternglass uses a single year, 1964 and in some cases this is not representative of

the yearly average.

It might be mentioned that it is also not an irradiated control, since the reactor began emissions in 1960. As a result of this, the increase in infant mortality in Livingston County is an artifact that results from an unusually low 1964 value. In fact the 230 percent increase or so that he suggests for Livingston County can also be presented as a 68 percent decrease, if you use 1963.

Grundy County is somewhat a different situation. Grundy County is a county of only 20,000 people and the number of deaths there in infants, infant mortality is comparatively small. Because of this, the high variability is characteristic of the data.

We have analyzed individual points instead of trying to predict a trend, and we find that the rise in infant mortality is significant at the 5 percent level for Grundy County, if you consider only individual points.

Now whether radiation exposure is the cause of this rise cannot be determined, because infant mortality has many, many different causes, as Mrs. Tompkins emphasized.

The death rate for all ages due to respiratory disease other than pneumonia and influenza is presented as a change in rate. It can perhaps be better appreciated by looking at the absolute numbers, that is, there were 10.9 deaths per 100,000 in Illinois, and 13.6 deaths per 100,000

in New York in 1960, and in 1967 both states had 18.6 and 18.7 respectively.

So two considerations indicate that radiation exposure is unlikely to be the sole cause of this change. First, there are many diseases with various causes included in this category and they are affected by a multiplicity of agents, smoking being one, pollution perhaps, air pollution perhaps being another. But at any rate the rise in death rate from this cause is occurring throughout the U. S.

More important, radiation exposure is reduced by diffusion of the gaseous emissions and the dose to the lungs of the exposed population is considerably less than 0.4 milliroentgen per year.

As the variance in background was measured over the Dresden reactor site, it is 46 to 110 mr, it appears unlikely we could detect such changes regardless of the size of the population. It is highly unlikely that this dose could contribute significantly to respiratory deaths in adults.

In summary, this analysis shows that radiation exposure has been grossly overestimated and in addition, the changes in infant mortality do not correlate with the radioactive emissions from the reactor site.

If I could read on page 16: "This analysis of the epidemiologic data presented by Sternglass does not

support his contention that an association exists between exposure to radioactive emissions from Dresden and infant mortality.

"In contrast," -- and I think this is equally important -- "the data cannot be interpreted to mean that no effects were produced by the radiation exposure. However, if radiation from the Dresden reactor contributes to infant mortality or respiratory deaths in Illinois or Chicago, it has not been demonstrated by this study."

End #13

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MR. ENGELHARDT: Mr. Chairman, I would like to offer in evidence as Staff Exhibit 9 the document we previously identified, "A Critical Review of Infant Mortality and Nuclear Power Generation by E. J. Sternglass, as Authored by A. K. Davis and Bernd Kahn."

CHAIRMAN SKALLERUP: Any objection?

MR. CHARNOFF: No, sir.

CHAIRMAN SKALLERUP: It is so ordered.

(The document referred to, heretofore marked Staff Exhibit No. 9 for identification, was received in evidence.)

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MR. ENGELHARDT: Our next witness is Dr. Marvin Goldman.

Whereupon,

MARVIN GOLDMAN

was called as a witness on behalf of the Atomic Energy Commission and, having been first duly sworn, was examined and testified as follows:

DIRECT EXAMINATION

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BY MR. ENGELHARDT:

Q Dr. Goldman, would you state your full name and address?

A My name is Marvin Goldman. I live in Davis, California.

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Q Would you please state your present responsibilities and give a summary of your educational and professional qualifications?

A I am the biophysics group leader at the radiobiology laborator of the University of California at Davis. I am also an adjunct professor of physiology in the School of Medicine there, as well as a lecturer in radiobiology in the Department of Physiological Sciences.

I have a bachelor's degree from Adelphi University, a master's degree in physiology from the University of Maryland, I hold a Ph.D. degree from the University of Rochester, School of Medicine, from the Department of Radiation Biology and Biophysics.

Over the past 20 years I have been engaged in radiation research. I was a biologist with the National Institutes of Health, a physicist with the New York City Department of Hospitals. I held a fellowship and scholarship at the University of Rochester during my graduate training and was later an assistant scientist in their Radiation Toxicology Section.

In 1958 I went to the University of California, where I assumed the responsibility I just mentioned. I have been the author or co-author of over 100 scientific articles and technical reports relating to biological effects of irradiation.

ln3

I am a member of the Radiation Research Society, the Health Physics Society, the New York Academy of Sciences, the Society for Experimental Biology and Medicine, the American Association for the Advancement of Science, and the Sigma Psi Honorary Scientific Society.

I am currently a member of the National Council on Radiation Protection, Committee No. 31, which is evaluating the biological and physical properties of the radionuclides. I am a member of the Advisory Committee on Long-Term Radiation Effects of the United States Public Health Service, Bureau of Radiological Health, now the Environmental Protection Agency.

I am a co-investigator to and consultant in radiation ecology at the University of California, a full investigator with a NASA program investigating the effects of weightlessness on calcium and bone. And I currently have a research grant from the University Cancer Coordinating Committee on the comparative ultrastructure of animal and human tumors.

Q On page 800 of the Transcript, Dr. Sternglass stated, "And the evidence that I simply want to cite is that we have seen again and again in animal studies and otherwise strontium-90 which was believed to reconcentrate only in the bone of animals, actually leading to severe damage to the ova, to the testes, and other organs that had not been anticipated."

ln4

Have you performed any experiments, Dr. Goldman, that relate to the effects of strontium-90 on animals?

A The program with which I am associated at Davis is almost exclusively dedicated to investigating the possible hazards from radiostrontium. And the experimental animal model in which we do our research is the beagle dog.

We are currently studying over 420 animals for their entire lifetime, to document the spectrum of effects which might be expected from strontium-90 fed daily from mid-gestation to maturity, which in the dog is about one and a half years and roughly corresponds to perhaps 20 years in adult humans. The strontium doses that are fed at this constant daily rate to a calcium content in their diet differ by factors of three in concentration from adjacent levels.

The highest amount of strontium-90 that we have been studying is 36 microcuries per day fed to dogs, which delivers a daily dose to the bone of about 18,000 to 20,000 millirads per day. The lowest level that we are studying is approximately 1-1/1000th of this, or perhaps 10 to 20 millirads per day.

The study is now in its tenth year and a clear spectrum of the kind of effects is available. First of all, I should like to point out that no pathologic effects have been seen at doses below about 2,000 millirads per day. At doses of over 2,000 millirads per day, hematologic effects in

the bone marrow are noted, and cause death from myeloproliferative disorders, which include anemias, and myelogenous leukemia. The higher the dose rate, the earlier the effect and the greater the incidence. But these are nonlinear, and in our experience the earliest cases at any of the dose levels in which effects are seen are seen by about one to one and a half years following the initiation of the exposure.

Tumors of the bone tissue are also seen and these include osteosarcomas, and fibrosarcomas. These are solid tumors which are generated in the skeleton. And these are at doses of over 2,000 millirads per day.

And they occur a bit later than the hematological or marrow effects which I just mentioned.

Thus the minimum cumulative total radiation dose in which bone tumors are seen is 800,000 millirads, where the average is over 1 million millirads. In these toxic levels, bone tissue injury is also seen. Marrow cells are affected and a depression in their numbers is seen and is reflected by a lower than normal number of white blood cells circulating in the blood.

And secondly, bone cells themselves are killed and lead to microscopic changes in the distribution of living bone cells within the compact bone, but usually not affecting the gross or radiographic appearance of bones examined by X-ray films. There have been no effects seen in our studies

ln6

indicating that strontium-90 and its daughter product, yttrium-90 has been detrimental to fertility. The number of dogs or young puppies in the litter, their growth rate, their body size or in other organs than the two that I mentioned that are at risk from the strontium deposition.

Our work using the daily feeding intake of strontium-90 has shown that a 1,000 to 10,000 reduction in the strontium-yttrium concentration in soft tissues, including the testes, seminal fluid and ovaries, is seen compared to that which accumulates in the bone.

It is physiologically impossible to get substantial doses to such tissues without first seeing rapid and lethal effects from the bone and bone marrow irradiation.

For example, at the very highest dose level, we have tested, which killed all of the animals in about three years, their bone and bone marrow cells, as I mentioned, were receiving about 20,000 millirads per day, but the genetic tissues, testicles and ovaries, contained almost no strontium and yttrium-90 and at the most received a measured dose of about two millirads per day.

Again we see this factor of 1,000 to 10,000. We were able to breed some of these dogs and their introductive performance was not different than that of the unirradiated control animals.

CHAIRMAN SKALLERUP: Excuse me, at the beginning

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I don't recall whether your question included a reference to the Transcript where Dr. Sternglass made this comment?

MR. ENGELHARDT: Yes, sir, page 800.

CHAIRMAN SKALLERUP: Thank you.

BY MR. ENGELHARDT:

Q Dr. Goldman, what is the relation between the dosage level at which these effects were found and the radiation protection guides found in FRC Report No. 2?

A The lowest level which has caused radiation deaths in the dogs at this time is at doses of about 2,000 millirads per day, or multiplying by the number of days in the year, approximately 700 and 30,000 millirads per year. This is compared to the Radiation Protection Guide in the FRC Report No. 2 limit of one-third of .5 rem per year, or about one-half millirem per day on the average.

These two values, thus, differ by a factor of 4,000. Thus in the dogs it takes about 4,000 times more than strontium-90 related dose to produce the effects that I have described than the limit in the protection guide.

Q Dr. Goldman, on page 800 of the Transcript Dr. Sternglass also states, "And now comes the kind of thing that we must take into account.

"Strontium-90 does not stay strontium-90. When it decays radicactively, it changes it into yttrium-90, which is another chemical substance, and it has different chemical

ln8

reconcentration properties and it tends to seek out the kidney, the liver, the glands, the pituitary, and all of the various chemical factories in the body that produce the hormones.

"And, as a result, growth is affected, especially in the early embryo and the fetus."

Have you done any work on the effects of yttrium-90?

A Yes. It is impossible to work with strontium-90 and not also be working with its daughter product, yttrium-90. The strontium-90 and yttrium-90 ingested is metabolized such that most of the yttrium-90 present in the diet is not assimilated and is excreted in the feces.

A small fraction of ingested strontium-90, which is absorbed and deposited in bone, -- by the way this amounts to about one to two percent of all of the strontium that we feed an animal in this one and a half year period -- decays.

This strontium-90 decays with a half-life of about 28 years to its daughter product, yttrium-90. This new yttrium-90 is thus in the bone and tightly bounds in the mineral structure. It cannot easily be translocated and almost none escapes.

The yttrium-90 available depends upon the parent strontium-90 content and the yttrium-90, I should point out, if it hasn't already been brought out, has only a 64-hour half-life, so that most of it decays very rapidly in the bone in which it is generated.

ln9

Only if the strontium and yttrium is artificially injected into a body cavity can any appreciable amount of yttrium-90 be available to other tissues.

end 15

Q Dr. Goldman, are you familiar with the work of others in this field on the effects of strontium 90 and the yttrium 90 on animals?

A Yes, I am personally familiar with a goodly number of other studies. And the one that I am most familiar with at my laboratory is certainly not the only one. Mice and dogs have been studied for a great number of years at the Argonne National Laboratory. A large experiment on swine, which are fed strontium 90 daily, not only for their lifetime, but for the full lifetime of two subsequent generations are being studied at the Battelle Northwest Laboratory in the State of Washington.

There is a large study on beagles, somewhat similar to our own at Davis being conducted at the University of Utah, in which a host of bone-seeking radionuclides are being intercompared.

In addition to this, rabbits have been studied at the University of Oxford in England, with injection of radionuclides, and about 300 rates also have been studied and reported for strontium 90 effects from the Biophysics Institute in Moscow, the work of Yuri Moskalev.

Q Dr. Goldman, how do the results of these studies which you have identified relate to your work?

A There is a very similar pattern of response. The effects that have been reported in all of these studies are

confined solely to those resulting from the irradiation of cells in the bone and bone marrow. And again in each of these other studies pathologic effects are seen only at very high doses, approximately 5 to 10,000 rads of cumulated radiation exposure. That is 10 million millirads, if that is the unit you are more familiar with.

Now as I mentioned before, if one artificially alters this physiologic route of delivery of the strontium to cells other than the absorption from food through the bloodstream and then into forming bone, different effects can be seen.

For example, in one study in Sweden by Professor Lunning, massive doses of strontium and yttrium were injected into male mice, into their peritoneal cavity, into their abdomen. The peritoneal cavity or abdomen drains into the inguinal canal.

Without getting into an anatomy lesson, it is easy to visualize that this literally results in bathing the testes of these mice with the strontium 90 containing fluid. This does if injected into the bloodstream would have proven lethal to the mice and this lethal dose did cause some effects on the sperm of these mice. But this injection method is not realistic, and it is more like observing the effects on sperm put into a test tube full of strontium 90 contaminated culture media.

Professor Lunning himself has denied that the experimental methodology is at all applicable to the problem of biospheric contamination or the normal physiological method of entry of the radionuclides into the body.

Q Dr. Goldman, how does your work and the work of others in this field relate to man?

A In all of the mammals tested, the metabolism and chemistry of ingested radio strontium is quite similar. This element is what we call an alkaline earth element and it behaves very similarly to calcium and barium and radium. It concentrates only where calcium concentrates and thus its effect is seen only in the cells near bone mineral deposits, that is, the bone and bone marrow.

The radioactive strontium 90 and its yttrium 90 daughter product emit electrons, beta particles. These have a range in tissue which is rather short, perhaps only a few millimeters.

In a mouse with strontium 90 in its bone, this range might include a slightly larger fraction of tissues, but very close to bone than one would see in larger animals, since the atoms behavior is independent of where it finds itself at the time of decay.

In the dog and in man, about 30 percent of the radiation energy and consequently the radiation dose is totally absorbed within the skeleton. That is why we choose the dog. The dog also has a bone and bone marrow that is

similar in anatomy and physiology to man.

An example of this is seen in a parallel study which we are in the midst of with the strontium 90 study in which radium 226 is giver to dogs in a manner quite similar to that which occurred earlier in this century with the radium dial painters, they used to tip the brushes with their lips to get a fine point and consequently ingested small quantities of radium over a short occupational period of time.

The results of this radium deposition in the human is very well documented and the results that we have found in our dogs are almost identical. They are very similar to that which we have seen in man. That is, the radium deposits in bone, the cells of bone are at risk and the consequences are seen in those regions in which the radium is concentrated.

So that our intercomparison then would merely be to take the results of the strontium we see in the dog, compare them to the results of the radium that we see in the dog, we know what the effects of radium are in man, and so it is quite simple and realistic to then project to the possible effects of strontium 90 in man by having this inter-species comparison.

None of the animals tudies about which I am aware have indicated any pathologic effects at levels anywhere near

the human guidelines. These effects have been seen only at doses that are about 1,000-fold higher than these guidelines.

Also the similar results seen in all of the animal species studied give me added confidence in extrapolating from these animal data to possible human situations. These thousands of animals studied over the past 25 years have not shown any radiation hazards at permissible levels or even at rather large multiples of those levels.

MR. ENGELHARDT: That completes Dr. Goldman's testimony.

DR. JORDAN: First of all I am a little confused about the strontium-yttrium relationships to dose. Since the yttrium half-life is of course very short compared to strontium, it should be in radioactive equilibrium and therefore there would be the same number of curies of yttrium as strontium. Is that right?

THE WITNESS: That is correct.

DR. JORDAN: Now, then -- here is where my health physics has lost me -- the half-life of the yttrium is short, and in one way I would think therefore, since there is an yttrium disintegration, every time there is a strontium disintegration, that the dose from the yttrium would be about the same as the dose from the strontium.

On the other hand, if I consider it in terms of biological half-life, then I would say it is very much less

from the yttrium.

Would you straighten me out on that, please?

THE WITNESS: Yes, I will try.

I think a simple way to do that would be to merely state that when strontium 90 decays, the electron it gives off has an average energy of about 0.2 MEV per disintegration of strontium 90. The yttrium on the other hand is a much more energetic electron and has an average energy of approximately 0.9. In equilibrium then the sum of these two would be the dose. So it would be 0.9 plus 0.2 and actually it is 1.13.

Well, the yttrium, therefore, accounts for about 80 percent of the radiation dose for the equilibrium disintegration. The strontium, however, determines where that will occur.

DR. JORDAN: Thank you.

DR. WINTERS: And your dose is calculated on the 1.13?

THE WITNESS: The dose is calculated on the total energy, yes, sir.

DR. JORDAN: You are saying that you have not seen effects for small doses could have two implications. One is there is a possible threshold, or the other is that there is no linearity, or the other is the number of animals you are using is small compared to the population of the U.S. and therefore there would be no chance of seeing any

effect.

Would you like to comment on that?

THE WITNESS: The number of animals that I am personally using is quite small. The number of animals in Russia was quite small and so forth. When you add them all up though the number is considerable. In every instance there were large numbers of animals put at risk at lower levels than the ones I have discussed in which no effects were seen.

If you wish to interpret this as a practical threshold, this seems to me to be reasonable.

Secondly, the lowest doses at which these effects are seen are not constant multiples of the amount of effect. That is to say that as the dose goes up, the effect is not proportional. So that a considerable increase in dose is required. This is what I would call a nonlinear response. It probably would best be called in scientific parlance a sigmoid response or a curvalinear response.

By definition this never reaches zero. There are practical limits. And whether one confines himself to the 10,000 animals or 200 million Americans there is always a difference between the theoretical infinity and the actual population that is studied.

DR. JORDAN: But you feel then that a linear curve response versus dose would not fit the data? It would take a

sigmoid curve?

THE WITNESS: Absolutely. Not only in our laboratory, but in all of the others in which strontium 90 has been studied either by acute injection at one time or continuous feeding, the results are nonlinear.

CHAIRMAN SKALLERUP: Thank you, Dr. Goldman.

MR. ENGELHARDT: Mr. Chairman, before I call the final Staff witness could I ask for a brief recess?

CHAIRMAN SKALLERUP: We will take a 10-minute recess.

End #16

(Recess.)

CHAIRMAN SKALLERUP: The hearing will come to order, please.

There is a phone call for Dr. Ralph Lapp. Is he present?

NO response.

Mr. Engelhardt?

MR. ENGELHARDT: Mr. Chairman, we have completed our rebuttal case with the exception of two remaining unfinished pieces of business and that is some additional material that we would like to present through Dr. Nelson who has previously testified as a rebuttal witness. And we have some additional testimony to provide throught Lester Rogers in response to a question by the Board.

If the schedule of events will not be disrupted entirely,we would propose to offer these two gentlemen tomorrow morning at the opening session and thus would now be prepared to bring back our panel of expert rebuttal witnesses for such cross examination by the parties as may be desired.

CHAIRMAN SKALLERUP: Let us have a conference with counsel.

(Discussion off the record.)

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CHAIRMAN SKALLERUP: Will the hearing please come to order. We just had a conference in order to expeditiously proceed with the balance of the hearing, and next we will hear Mr. Rogers and after that Miss Evans will cross examine the Applicant's rebuttal. At 9:30 tomorrow we will reconvene and the Coalition will cross examine the Applicant's rebuttal and Commission rebuttal. At one o'clock LIFE will continue its cross examination of Applicant's rebuttal and undertake examination of Commission rebuttal.

We will adjourn at 4:15 because the building will be used for other purposes, and we will reconvene at St. Johns Lutheran Church at 7:00 in the evening to hear Mr. Lau's case and to continue with cross examination that evening until a reasonable hour and for the balance of the week until the case is terminated.

Is this a correct understanding?

MR. CHARNOFF: I believe that is correct, sir.

MR. ENGELHARDT: Mr. Chairman, there is one matter we have to include in that schedule and that is Dr. Nelson has been asked by the Board to provide some additional information on the chart which we will be ready for tomorrow at any convenient time you want to put it into the schedule.

CHAIRMAN SKALLERUP: If he is prepared, let him appear at the beginning of the hearing tomorrow.

MR. ENGELHARDT: Fine.

Whereupon,

LESTER ROGERS

resumed the stand as a witness on behalf of the Regulatory Staff and, having been previously duly sworn, was examined and testified further as follows:

THE WITNESS: This is in response to the Board's request for clarification on the relationship between the various sections of Part 20 and their applications. Section 2.106, 10 CFR Part 20, sets forth the AEC regulatory requirements for controlling releases of radioactive material in effluents to unrestricted areas from all AEC-licensed activities.

The basic objective of the regulations is to limit releases of radioactive material to the environment from each licensed activity so that radiation exposures of the general public from the cumulative effects of all licensed activities, when added to exposures from other sources, not including exposures from natural background and medical procedures, are not likely to exceed radiation protection guides recommended by the Federal Radiation Council, now in the Environmental Protection Agency, and approved by the President, that is, 500 millirems per year for individuals in the population, 170 millirems per year to the average of suitable samples of the population groups.

Part 20 applies to a broad variety of licensed

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activities such as use of radioisotopes in hospitals, universities, research institutions, industry, research reactors, power reactors, chemical reprocessing plants, et cetera.

These activities differ greatly with respect to the types and volumes of effluents generated. For this reason flexibility is provided in Section 2.106 in the application of release limits to various types of activities in achieving the basic objective of limiting exposures off-site.

Section 2.106 of Part 20 sets forth concentration limits that are generally applied directly at the point of release through a stack pipe or conduit prior to any environmental dilution and without taking into account the specific characteristics of a particular site and environment.

These release limits generally assure that taking into account environmental dilution, radiation exposures to individuals in unrestricted areas will not exceed more than a small fraction of radiation protection guides.

For many licensed activities, such as medical, research and industrial uses of radioisotopes, the volumes of effluents are small, the concentrations of effluents are extremely low and it is entirely practicable for these kinds of activities to meet the restrictive limits under 2.106(a). For some licensed activities it is not practicable

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to meet these restrictive limits. Section 2.106(b) provides that application for a license or amendment may include proposed limits higher than those specified in 2.106(a). The Commission will approve the proposed limits if the Applicant demonstrates that (1) he has made a reasonable effort to minimize the radioactivity contained in effluents to unrestricted areas, and (2) that it is not likely that radioactive material in the effluent would result in the exposure of an individual to concentrations of radioactive material in air and water exceeding the limits specified in Appendix B, Table 2 of Part 20.

In the case of noble gases released from power reactors, exposure to these concentration values result in a whole body exposure of 500 millirem per year. Section 2.106(b) allows one to establish release limits taking into account the specific characteristics of a particular site and environment such as meteorology, hydrology, topography, population density, et cetera.

It is under this provision that technical specifications, which are conditions imposed on operating licenses, are developed to limit releases of radioactive material in effluents from nuclear power reactors. 2.106(c) spells out some of the detailed information that must be included in an application to establish release limits under this section.

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Section 2.106(e) is a generally applicable provision that is designed to assure that for any licensed activity operating under either 2.106(a) or 2.106(b), the total quantities of radioactive material released in air or water during a specified period of time do not result in intakes of radioactive material from air, food and water that would result in doses to the critical organ of a suitable sample of an exposed population group from all sources of exposure in excess of one-third the dose limits recommended by the NCRP and ICRP.

Finally, Section 2.106 (C) provides that in accordance with recommendations of the Federal Radiation Council approved by the President, persons engaged in activities under licenses issued by the Atomic Energy Commission pursuant to the Atomic Energy Act of 1954 as amended should, in addition to complying with the requirements set forth in this part, make every reasonable effort to maintain radiation exposures and releases of radioactive materials in effluents to unrestricted areas as far below the limits specified in this part as practicable.

end 17

DB18 1n1 Section 50.34(a) sets forth requirements specifically applicable to nuclear power reactors that are designed to keep radioactive material in effluents from such reactors as low as practicable. In applying all of these basic provisions of the regulations to different licensed activities, the detailed license requirements and technical specifications would vary because of differences in the design of facilities, differences in operating characteristics, and differences in the characteristics of the environment in which the facilities are operating.

For example, in establishing release limits for nuclear power reactors under Section 2.106(b), technical specifications may verywell include limitations on procedures such as periods over which release limits may be averaged, that would not necessarily be applied universally to all types of licensed activities that differ widely in nature.

The procedure of applying a factor of 700 to calculations of stack release limits for halogens and particulates with a half-life greater than eight days is an administrative procedure used in implementing Section 2.106(e) which, of course, allows for full consideration of the biological concentration mechanisms which seem to concern Dr. Tamplin.

For example, biological concentration of iodine and cesium in abalone will be taken into account in establishing

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the release limits for a reactor now under construction on the West Coast. Other detailed requirements may be included in technical specifications in implementing Section 50.34(a) to keep levels of a radioactivity in power reactor effluents as low as practicable.

While the detailed administrative requirements applied to various facilities will vary, they all have the common objective of providing reasonable assurance that exposures to the public of well within FRC radiation protection guides and the limits set forth in Part 20.

That completes my statement.

CHAIRMAN SKALLERUP: Thank you, Mr. Rogers.

We will study it in the Transcript.

Miss Evans, are you prepared to proceed?

MISS EVANS: Yes, I am.

MISS EVANS: On behalf of Intervenor LIFE and William E. Reany, I will be cross-examining this afternoon the applicants --

CHAIRMAN SKALLERUP: Would you hold it a second, please?

Would you move the microphone in front of you, please.

MISS EVANS: On behalf of Intervenor LIFE and William E. Reany, I will proceed with cross-examination of

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the applicant. I would just like the record to reflect that after it was indicated that LIFE had no additional witnesses, that the Atomic Energy Commission and the utility company had all last week to prepare their rebuttal witnesses' testimony.

Although we had anticipated some rebuttal testimony to be offered by the AEC and the applicant, we did not anticipate to be given such a short time to prepare cross-examination of these rebuttal witnesses.

With the tremendous amount and conplexity of the subject matter presented here, it is very unfair to expect Intervenor, in this case a citizens' group, to prepare with their limited resources in such a short amount of time.

But, perhaps there are some questions I have prepared today that I can go ahead with and tomorrow Mrs. Bleicher will resume cross-examination on our behalf.

CHAIRMAN SKALLERUP: I would like to make a statement at this point, and that is that your counsel is not here today and your counsel missed some other sessions of this hearing and one of the, I think, tested ways of preparing for cross-examination is to take notes at the time the direct testimony is being given.

So that to a degree you may have been at a disadvantage, but this is because of the choosing of your counsel and I for one do not think that you have been prejudiced.

MISS EVANS: Well, it is just that in our case we

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don't have scientific expertise that perhaps the other parties have. It takes just more than me taking notes to be able to cross-examine in the capacity we would like to be able to participate in in these hearings.

CHAIRMAN SKALLERUP: Well, you have to use the resources available to you.

MISS EVANS: We are trying. I will proceed then.

The question is for, I believe, Mr. Lowell Roe.

In reference to his statement on the design of the liquid radioactive processing system for the Davis-Besse station. This is a question that I would like to have clarified for our information and for the record.

With reference to the first sentence, "The design of the liquid radioactive processing system for the Davis-Besse station incorporates the most effective efficient proven technology for reducing the radioactive contents of the processed liquid," I would like to have Mr. Roe, if he could, provide me the evidentiary basis, or at least outline a summary of the evidentiary basis for the statement I just quoted on the adequacy of the liquid radioactive waste system in Davis-Besse.

MR. CHARNOFF: I am going to ask Mr. Roe to respond to that question. I would like to observe, Mr. Chairman, that this question addressed to Mr. Roe was in response to some statements made by Dr. Sternglass on

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behalf of the Coalition that we had expressed some reservation as to its relevance to their contentions.

But in any event, there is a question and I won't make a point of it now but there is a question as to whether this area of inquiry is within LIFE's contentions. But I am going, for purpose of the record at least, to ask Mr. Roe to respond to the question, noting, however, that there is a question as to the relevance of this question to LIFE's contention.

CHAIRMAN SKALLERUP: I have serious doubts whether it is relevant to you contention. And I would ask whether you really have a clear idea of the scope of your contention.

MISS EVANS: Yes, I do. If I could offer a comment here, our question is if -- not question but statement -- if such advanced technology exists and is tested to limit the effluents of nuclear plants to the lowest possible amount and if this is the case for reactors to be constructed and those operating now, perhaps at least for our nuclear power plants they could be, they cuild operate under a limit lower than Part 20.

We are trying to establish perhaps what part this would be, you might say as a solution to the problem we are presenting here. By saying that we feel the standards are inadequate, we are trying to see what possible dosages or effluent levels the nuclear power industry, power plants

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industry, could operate under.

MR. CHARNOFF: I won't proceed further with the argument on this. I would just like the record to show the remarks I made before and ask Mr. Roe at this point to respond to the question.

WITNESS ROE: Yes. The equipment for the liquid rad waste system is outlined in Section 11 of the PSAR. This includes the type of equipment which I had discussed in the statement yesterday, the degasification, filtration, ion exchange and distillation equipment.

Also included are the decontamination factors assumed for this equipment.

MISS EVANS: Well, I know what the components of the system are. I am asking you to outline the basis for that statement that you made concerning the most efficient proven technology, because perhaps outline the basis for this, and I don't know if you have it at your fingertips, but I am interested in what is the most efficient proven technology?

For instance, it was brought out earlier in the hearings, in reference to another type of radioactive waste in the Oak Ridge system that was being investigated. I am trying to establish what the most efficient proven technology is they are going to operate under.

If he could outline this, I know what the components are --

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MR. ROE: The first part of your question there, I simply don't know of additional equipment beyond what we have outlined that is available. The Oak Ridge system which I believe we had commented on before is an unproven system and it bears no relationship to the liquid rad waste treatment system.

MISS EVANS: I understand that. But I had wanted you to provide me with assurance that the evidence, the evidential assurance of the statement you made yesterday that you are operating under the most efficient proven technology.

I am aware of what is in Part 11 of the PSAR. I am aware of the components of the rad waste system. But I am trying to establish the background for the fact that you stated yesterday.

MR. ROE: This equipment that we have outlined to be installed here compared with many other plants, we would expect that to be at this low release and as low or lower than anybody else that is installing this equipment.

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MISS EVANS: All I am trying to establish here is the evidential basis for the statement that you made yesterday that this is the most efficient proven technology. I don't have the documents that you cite in the PSAR. But if you have or can describe to me -- I am not interested in dates -- but perhaps the experimental, the evidential basis for the statement you made. How is this the most efficient proven technology?

MR. CHARNOFF: Mr. Chairman, I think the testimony has shown that the PSAR, which is part of the record in this case and which has been made available to LIFE, describes the system.

Mr. Roe has testified that the system that we are going to install here and that is described in the PSAR would produce as low quantity and quality of effluents as any system now in operation.

If LIFE is interested in pursuing the point of demonstrating that there are alternate systems that are more efficient, without regard at the moment as to whether that is relevant to their contention, LIFE was free to bring on direct testimony to this effect.

I think the question has been asked several times and it has been answered several times. At this point I would object to the repetitive nature of the question.

CHAIRMAN SKALLERUP: I think the repetitive nature

of the question is due to the fact that she hasn't had an answer as to why Mr. Roe believes that this is the most efficient number of units to be employed. What comparison have you made, for example, with other systems? And upon what do you base your opinion that it is the most efficient?

MR. CHARNOFF: I am going to let Mr. Roe answer that, but I think he stated in his testimony that the expected effluent releases from this plant will be as low or lower than the operating experience at other plants, which suggests to me that he has compared the results of this system with other plants and therefore it is the best proven technology.

CHAIRMAN SKALLERUP: Then Mr. Roe should say so.

MR. CHARNOFF: I think he did say so, sir. But I would be glad to have Mr. Roe affirm that.

MR. ROE: If I didn't say it in those words, that is certainly what I intended, that the equipment that we have installed or plan to install has been compared with other plant installations, and for this reason we believe that our effluents will be as low or lower than operating plants now.

MISS EVANS: Could you give me the evidential matter you used to come to this conclusion that your system is the most efficient proven technology for reducing the radioactive content of the processed liquid from Davis-Besse?

MR. ROE: The operating reports from existing stations outlining the releases that they have made over a period of time.

MISS EVANS: Is there a system -- is there a plant in operation at the present time with the same sort of system? I believe you answered that. Have all of these systems to be utilized in Davis-Besse fully demonstrated their performance in nuclear station operations elsewhere?

MR. ROE: The individual components that make up the complete rad waste stream, yes. I can't say that it has been demonstrated that all of the components that we plan to have have been demonstrated in a complete stream.

MISS EVANS: So then perhaps all the evidence, the operating evidence, does not exist for every component that you are going to use in the Davis-Besse Nuclear Power Plant?

MR. CHARNOFF: Mr. Chairman, I think we should establish how this relates to the LIFE issue. It seems to me that we have testified before that our releases will be well below Part 20.

If the point that Miss Evans wishes to argue is that, as I understood her to say, that if the plant can operate at less than Part 20, the Part 20 standards ought to be lower, the evidence is already in that this plant is expected to operate below Part 20. The nature of the questions that I believe she is asking are directed to the question as to whether we have a system, or whether there is a system

that can work at still more efficient performance than the one we are proposing. But that kind of question then would go to the issue as to whether we are conforming with Part 20, namely, the lowest practical standard that is in Part 20. That is not the issue of LIFE nor of any other Intervenor in this particular case.

MISS EVANS: You mentioned earlier that --

CHAIRMAN SKALLERUP: Off the record.

(Discussion off the record.)

CHAIRMAN SKALLERUP: On the record.

MISS EVANS: I would like to resume with a question that relates to what Dr. Goldman said on behalf of the Applicant yesterday or in response to a question asked by Mr. Charnoff and this was considering Dr. Gofman's and Dr. Tamplin's statements with respect to the present AEC standards.

I would like to have Mr. Goldman read section 2.106 (e), the first sentence, to me, please.

MR. MORTON GOLDMAN: 2.106(e). "In addition to limiting concentrations in effluent streams, the Commission may limit quantities of radioactive materials released in air or water during the specified period of time if it appears that the daily intake of radioactive material from air, food or water by a suitable sample of an exposed population group averaged over a period not exceeding one year would otherwise

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exceed the daily intake resulting from continuous exposure to air or water containing one-third the concentration of radioactive materials specified in Appendix B, Table 2 of this Part."

That is the entire section.

MISS EVANS: Thank you. Then in reference to your statement yesterday in the testimony, and I don't have the transcript page, you said "Section 2.106(e) limits the quantity discharged from facilities of intake of radioactive materials from air, water or food by a suitable sample of an exposed population group would exceed one-third of the intake represented by the MPC values."

I would like to ask you is it correct that 2.106(e) does not necessarily mean the AEC requires the limiting of MPC concentrations according to -- concentrations in the appendix for air and water, it only gives authority to the Commission to do so under certain circumstances after it is deemed necessary?

MR. MORTON GOLDMAN: It does say the Commission may limit if it appears that, yes. But it is not necessarily after the fact. It may be prospective, that is based on analyses before the plant goes into operation.

MISS EVANS: Would you clarify your last two statements. I am lost.

MR. MORTON GOLDMAN: I think this section has

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been interpreted by some people, certainly by Drs. Gofman and Tamplin in other cases, that this only applies after a situation has developed, that the Commission may limit quantities based on the one-third intake. In other words, on the basis of monitoring of the environment, or other bases for estimation of dose of an existing situation. What I am saying is that based on my own experience with the licensing process that this section has been invoked prior to the operation of a plant if in fact it appears on the basis of analyses and calculations that this situation may exist before the plant goes into operation.

So it is something that can be applied in advance of an actual situation developing, rather than after the fact of a situation having developed.

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MISS EVANS: But it seems to me this is not what the Commission regulations are saying, that this is only an instance that must have been done outside of these regulations. This regulation states that this will be done after these samples have been taken and after the intake from the air, water and food samples by exposed population groups has been established to exceed one-third the concentration of 0.5 millirems.

DR. MORTON GOLDMAN: I would be very happy if the Staff would interpret it that way, because it would make life a lot simpler, at least at the beginning. But unfortunately they don't, from my own experience. They do apply this before the fact.

I think Mr. Rogers stated in his response to the Board's question just a few moments ago that the Staff was going to consider the reconcentration in abalone in a West Coast plant in setting the technical specifications for that plant prior to the plant's going into operation.

MISS EVANS: But it would still seem to me that the regulations indicate that they do not do so until afterwards. It may be a matter of record, what we talked about this morning, Mr. Rogers' testimony, the technical specifications, that come out before the plant. But in Part 20 there is no provision for this on the record that they do this beforehand.

MR. CHARNOFF: Mr. Chairman, this is not a question, this is an interpretation of the regulation by Miss Evans which Dr. Goldman has suggested is in error. But in any event, it is a question for LIFE to propose as a matter of legal argument, it seems to me.

The regulation is clear on its face, they interpret it one way, Dr. Goldman suggests it is interpreted another way, but it is certainly not a question to be asked of Dr. Goldman in his technical expertise.

CHAIRMAN SKALLERUP: That is correct.

MISS EVANS: Then I will rest with the cross-examination until Mrs. Bleicher is able to assist me tomorrow at 1 o'clock.

MR. CHARNOFF: Mr. Chairman, this is consistent with the schedule we have talked about. I would like the record to show of course that as of yesterday it had been reported to us by Mrs. Bleicher that she would be here this afternoon to conduct cross-examination of the Applicant.

CHAIRMAN SKALLERUP: Are there any other matters?

MR. BARON: If you want to keep on going for a few minutes, I might be able to get in some questions right now. They wouldn't be very long and they wouldn't require a lengthy answer I am sure.

CHAIRMAN SKALLERUP: Proceed.

MR. BARON: It is with regard to Applicant's

Exhibits 5 and 6, the letters received from the Adjutant General's Department and theSecretary of Defense.

I assume these were received as a result of Mr. Roe or someone from the Applicant requesting this information.

My question: What information was given in the request for assurance? In other words, was it explained that the plant would stand and operate for 40 years? That is one of the items that I am looking for.

If somebody says "Give me some assurances," I would assume it would also be proper to give a basis for the question or reason for the question. Maybe there are copies available of the letters that went out to these two departments.

To put it another way, if you will permit me, were the two addressees of the original requests fully apprised of what will be sitting there? They don't have a copy of the PSAR, and they may have as little familiarity with this area of science as any layman.

MR. ROE: We were talking to competent people in both areas here who were fully aware of the type of plant that we were talking about, it being a nuclear plant. I believe the Staff had said in the Secretary of Defense letter that their copy had been forwarded to them through --

CHAIRMAN SKALLERUP: Mr. Walske. Do you have the Walske letter?

MR. BARON: I didn't see that. Here it is, I have it here, it is Exhibit 3.

Mr. Roe, what you are saying though is that a full exaplanation or full apprisal was given to these two departments of what the plant consists of, or would consist of, so that they understood the significance of the thing, so that in the forming of these answers they are telling you that no matter that this is going to sit there for 40 years and be as powerful as anything could be for the 40 years, we understand this, and we are going to take all of the precautions necessary? That is what I am getting at.

MR. CHARNOFF: May I suggest, Mr. Chairman, that if Mr. Baron would examine the letter from Mr. Walske, for example, on page 1716, the letter speaks for itself, and it says "On January 14, 1971, Mr. Packard, Deputy Secretary of Defense, wrote to Mr. Davis, President of Toledo Edison Company confirming the Department of Defense's awareness of the plans for the construction and operation of the Davis-Besse facility."

It suggests that the Department of Defense is aware of the Davis-Besse facility. We have to assume that when people talk about a power plant, it is not something that goes into operation for several days or weeks, but it has some life.

Mr. Walske is Assistant to the Secretary of

Defense.

CHAIRMAN SKALLERUP: For atomic energy.

MR. CHARNOFF: Yes.

End #20

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MR. BARON: Do you have a copy of the letter that Howard Fox wrote to the Adjutant General?

MR. ROE: There was no letter written.

MR. BARON: What did he do, just call?

MR. ROE: Yes. There were personal contacts made with the Adjutant General to reaffirm their awareness of the Davis-Besse station.

MR. BARON: But you don't know to what extent their awareness was? The awareness of an individual as to what Davis Besse stands for could be quite different from one person to another.

MR. ROE: No. The reaffirmation on the letter from the Adjustant General -- most of the material was a restatement of the material contained in an August letter, I believe, of the Commandant of Camp Perry. So that it was a restatement on the Adjutant General's level and some amplification of that information.

MR. BARON: Was there any effort made with regard to the Uniroyal people or the TRW people? It seems to me there was some indication that Uniroyal owned that Erie testing ground and at the present time it was leased to TRW, which lease had perhaps months to go.

Was there any further contact made with those people as to what possible useage they might want to make of it?

MR. ROE: There was some contact with TRW. There

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war an indication from them that they would not be renewing their lease.

However, the second page of the Adjutant General's letter does state that all of the firing, any firing from the Erie Industrial Park there would be in strict accordance with -- I can quote it.

"Any firing from the Erie Industrial Park must be conducted in accordance with strict safety precautions and in accordance with the same procedures in force for firing from Camp Perry."

This gives the assurance from the Adjutant General that any operations there will be under strict control.

MR. BARON: The way I interpret this letter then is that the Industrial Park is under the control of Camp Perry. Is that what you are saying?

MR. ROE: The firing, any firing from Camp Perry using the restricted areas is under their control.

MR. BARON: That is what this joint use agreement is that is referred to on the first page, the last paragraph? "TRW Jet and Ordnance Division has entered into a joint use agreement with us which permits them to test their weapons on Tuesday and Thursday each week."

MR. ROE: This is my understanding.

MR. BARON: All I am getting at obviously is that that particular location will be controlled by somebody

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and it is your interpretation of this letter that it is controlled by Camp Perry?

MR. ROE: That is correct.

MR. BARON: That is a fact to which you are attesting, is that right?

MR. ROE: Yes.

MR. BARON: Those were the easiest things I could come up with at the moment. You are going to have to allow me to wait until tomorrow for the rest.

CHAIRMAN SKALLERUP: That being the case, we will adjourn until 9:30 tomorrow at this place.

(Thereupon, at 4:45 p.m. the hearing was recessed, to reconvene at 9:30 a.m. the following day.)