7/24/81

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter of	)	
SOUTH CAROLINA ELECTRIC AND GAS COMPANY	)	Docket No. 50-395 OL
(Virgil C. Summer Nuclear Station Unit 1)	)	

REBUTTAL TESTIMONY OF
EDWARD F. BRANAGAN, JR. REGARDING CONTENTION A10

Q.1. Could you please state your name and affiliation.

A. My name is Edward Branagan. I am a Radiological Pnysicist with the Radiological Assessment Branch in the Office of Nuclear Reactor Regulation. A copy of my professional qualifications are attached.

Q.2. Do your responsibilities include evaluating health effect models for the NRC.

A. Yes.

Q.3. What is the purpose of this testimony?

A. This testimony is designed to address the validity of the health effect models used in the Staff Final Environmental Statement (FES) (NUREG-0719)[1] and the prefiled testimony of Drs. K. Z. Morgan,[2] Michio Kaku[3] and Helen Caldicott[4] served on behalf of the Intervenor.

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Q.4. What is the basis for the health effect estimates in the FES?

A. Health effects which could be attributed to radiation dose commitments associated with exposure to radioactive effluents from normal

operations and from the fuel cycle were estimated in the FES. Potential cancer mortalities were determined by multiplying the dose commitment (in units of person-rem) by an appropriate risk estimator (in units of potential premature deaths per 10^6 person-rem). The risk estimators used in the FES were based on models described in a National Academy of Sciences report entitled "The Effect on Populations of Exposure to Low Levels of Ionizing Radiation."[5] This report is known informally as the BEIR I Report after its authoring Committee on the Biological Effects of Ionizing Radiation. The BEIR I Report consisted of a comprehensive review and reevaluation of the scientific basis of radiation exposure on humans by scientists who were eminent in their fields.

The cancer risk estimators in the FES were based on the linear non-threshold dose response model and the "absolute risk" projection model described in the BEIR I Report. In the text of the FES (§4.5.5), it was noted that risk estimators derived from the "relative risk" model (using the linear non-threshold dose response model) would provide risk estimators about four times greater than those used in the text. In regard to the use of the linear non-threshold model, the National Council on Radiation Protection and Measurements (NCRP) cautions that:

> [L]inear interpolation between the naturally occurring spontaneous incidence and the incidence observed following exposure at intermediate-to-high doses and dose rates generally overestimates the risk of low-LET [linear energy transfer] radiation at low doses and low dose rates. This observation has also been incorporated in reports by the ICRP (1977), NCRP (1975), and UNSCEAR (1977).[6]

All of the whole body dose to offsite individuals from exposure to radioactive effluents from routine reactor operations is due to low dose rates from low-LET radiation.

The whole body risk estimators that were used in the FES are compared with risk estimators from other sources of information in Attachment 2. The risk estimators that are compared in Attachment 2 include values from the BEIR I Report, the National Academy of Sciences BEIR III Report which was published in 1980, the International Commission on Radiological Protection (ICRP), and the United Nations Scientific Committee on the Effects of Atomic Radiation (UNSCEAR). [5,7-9] These organizations, along with the NCRP, represent the views of the overwhelming majority of the members of the scientific community. The risk estimators used in the FES are consistent with the values from these other sources of information.

Q.5. What is your position on Dr. Morgan's prefiled testimony?

A. The essence of Dr. Morgan's prefiled testimony is that there are uncertainties in the risk estimators that can be derived from epidemiological studies of persons exposed to ionizing radiation. Dr. Morgan offers numerous reasons to question the accuracy of risk estimators derived from epidemiological studies. Dr. Morgan argues that:

1. "[T]he standards have been based on two sources: (1) the survivors of the bombings of Hiroshima and Nagasaki; and (2) the therapeutic use of radiation, in particular the local exposures of high doses to persons suffering with the disease ankylosing spondylitis." Prefiled testimony at 2.

2. "The data from Hiroshima and Nagasaki were taken and used without any correction whatsoever for the effects of fire, blast, and a traumatic situation...." Prefiled testimony at 3.

3. Risk estimators derived from the ankylosing spondylitis patients were based on a "select population that dies early of common diseases -- heart failure, tuberculosis, pneumonia, emphysema, chronic bronchitis." Prefiled testimony at 4.

4. "[I]t was very poor judgment on the part of the BEIR III Committee to base their principal support on the survivors of Hiroshima and Nagasaki.... I understand from discussions with scientists over the country that there is a paper in print (see "New A-Bomb Studies Alter Radiation Estimates", Science, Vol. 212, May 22, 1981) this month -- the issue hasn't been received by me up to the present time -- that it is published through the studies and is a result of the calculations and estimates of a group at the Lawrence Livermore Laboratory in California which indicates there was a very serious overestimate of the dose in these two cities.... Now it would appear that the risk is greater than the BEIR III Report would suggest." Prefiled testimony at 5,7.

5. "Furthermore, there's a tremendous amount of data, much of it data on human exposures, which shows you get a better fit if you use the super linear hypothesis, namely when you get down to low doses, the number of malignancies per rem is greater than at large doses - that is not only is there no low safe level of exposure, but the risk of cancer per unit of exposure is greater at low doses than at high doses." Prefiled testimony at 7.

6. "Fortunately, we know quite a bit about the risks, perhaps the uncertainty is not spread over a factor of 10 one way or the other. Some other risks-chemicals-the uncertainty is greater." Prefiled testimony at 17.

In regard to Dr. Morgan's testimony, it should be noted that Dr. Morgan states that "perhaps the uncertainty is not spread over a factor of 10 one way or the other." Although Dr. Morgan does not provide a specific value for a mortality risk estimator for whole body exposure in his prefiled testimony, he has used a value of 600 total potential cancer cases per 10^6 person-rem in the past. [10] BEIR III has indicated that the number of potential non-fatal cancers is about 1.5 to 2.0 times the number of fatal cancers. Using these conversion factors, Dr. Morgan's total cancer risk estimator for whole body exposure implies a fatal cancer risk of about 300-400 potential deaths/10^6 person-rem. In his testimony on June 26, 1981, Dr. Morgan stated that his most recent value for a total fatal cancer risk estimator for whole body exposure was about 900 deaths per 10^6 person-rem. Dr. Morgan's most recent value is higher

than the highest values in the National Academy of Sciences BEIR I Report, BEIR III Report, ICRP 26 and UNSCEAR (i.e., compare Dr. Morgan's most recent value with values in attachment 2). It is possible that no cancers will occur because the doses and dose rates in the vicinity of the reactor are much less than the doses and dose rates at which excess cancers have been detected in human populations.

In regard to Dr. Morgan's first point, that the standards have been based primarily on data from exposure of two groups (i.e., the A-Bomb survivors and the ankylosing spondylitis patients), it should be noted that there is a substantial body of literature, in addition to studies of the two preceding groups, on the risks of radiation exposure at dose levels much higher than those estimated in the FES. Some of the other major groups that have been studied include (1) radium dial painters, (2) early radiologists and dentists, (3) five different miner populations, (4) children irradiated for thymus enlargement, (5) tinea capitis patients, (6) patients receiving breast irradiation and (7) children whose mothers were irradiated during pregnancy. Studies of these groups support the risk estimators that can be derived from the A-Bomb survivors and the ankylosing spondylitis patients.

For example, in the BEIR III Report, Dr. Radford, Chairman of the BEIR III Committee, stated (p. 241):

> "An important question is the extent to which the Japanese data are consistent with the data from all the other studies... In general, the concordance is excellent for the major cancers where several data sets exist such as breast, thyroid and lung cancer. Other sites show various degrees of agreeme But the most important comparison is for total cancer incidence coefficients derived for each sex from the Nagasaki Tumor Registry data. From data presented in the April 1979 draft, these are found to be about 2/3 as great as the sum-of-sites coefficients summarized in Table V-14. This degree

> of concordance of results from human studies of a great range of exposure conditions, ethnic makeup and basis for radiation exposure is truly remarkable."

Consequently, the existing data base is adequate to estimate potential health effects. The range of values stated in the literature is sufficient to characterize the risk.

In regard to Dr. Morgan's second point that the A-Bomb survivors were a select population, it should be noted that the BEIR III Report discussed this viewpoint and concluded (pp. 156, 157):

> "Whether the risk estimates derived from the experience of the Japanese atomic-bomb survivors are generally applicable is best determined empirically, by applying the test of consistency with other human data. When this is done...risk estimates derived from the atomic-bomb experience are seen to be generally consistent with those based on other human exposure. The only very marked exception is the absence of a carcinogenic effect among those exposed in utero....
>
> That genetic effects have not thus far been found does not necessarily argue against the general applicability of the atomic-bomb experience: no direct evidence of a genetic effect has been forthcoming for man, and presumptions as to the order of magnitude of any such effect (see Chapter IV) suggest that it is too small to be easily seen in samples of the size available to investigators in Japan. Nor is the absence of a general life-shortening effect any indication that the atomic-bomb experience is a dangerous basis for generalization....
>
> The argument of Kneale and Stewart rests on observations, made in the Oxford Survey of Childhood Cancers, that many childhood illnesses and injuries are more frequent before death from cancer than in live controls, and progressively so as death approaches. The argument that early-occurring radiation-induced cancers would not come to light because of an "exceptionally high infection death rate for several years after the event" is not borne out. There were no major epidemics in Hiroshima and Nagasaki. (footnote omitted)

In summary, the BEIR III Report considered the "effects of fire blast and a traumatic situation" on the A-Bomb survivors, but nonetheless

used the A-Bomb survivor data to estimate risk to the general public since the A-Bomb data was supported by many other studies.

In regard to Dr. Morgan's third point that the ankylosing spondylitis patients were a select population, it should be noted that there are other factors (e.g., differences in doses and dose rates) that should be taken into account. For example, the BEIR III Report (p. 355) concluded in regard to the risk of leukemia:

> "It should be recognized that risk estimated at a selected point in the high-dose region may overestimate the magnitude of hazards of low-dose exposures by a factor of 2-10....The estimated risks from the atomic-bomb and ankylosing spondylitis treatment represents upper limits, in that both are derived from high-dose-rate exposures.

Consequently, the risk estimators in the FES, which are based in part on the data from the ankylosing spondylitis patients, may actually overestimate the risk, rather than underestimate the risk, because the doses and dose rates to individuals in the vicinity of the Summer reactor will be much less than the doses and dose rates in the ankylosing spondylitis patients.

Dr. Morgan's fourth point was that a May 22, 1981 article in Science (Attachment 3) indicates that an overestimate of dose in the A-Bomb studies will result in a risk greater than that concluded in the BEIR III Report.[11] The NRC Staff would note the following: first, the article that Dr. Morgan referenced was in the News and Comment section of Science. Although the article referred to new dose calculations by W. Loewe and E. Mendelson at Lawrence Livermore Laboratory (LLL), G. Kerr of Oak Ridge National Laboratory (ORNL), D. Kaul of Science Applications, Inc. (SAI), and J. Marcum of Research Development Associates, it did not

reference any papers by these individuals to document the claims in the article.

Second, the NRC Staff has obtained preprinted copies of two LLL papers concerning the revised dose estimates at Hiroshima and Nagasaki and their implications concerning cancer risks. [12,13] The LLL papers indicate that the May 22nd Science article is misleading in a number of important areas. The May 22nd Science article stresses that the RBE for neutrons may have to be increased. However, Straume's and Dobson's preliminary conclusion is that:

> "Evidence for high neutron RBE is lacking for leukemia and breast cancer. However, data for total malignancies and for chromosome aberrations suggest that neutron RBEs increase with decreasing dose, because of the diminishing effectiveness of gamma rays, and may reach quite high values (100 cannot be excluded.)" [13]

Consequently, for total malignancies and chromosome aberrations Straume's and Dobson's preliminary conclusion is not that neutrons are much more dangerous than has been previously thought, but rather the effectiveness of gamma rays for inducing total malignancies and chromosome aberrations is less than was previously thought.

Third, several of the principle authors, who are reevaluating the A-Bomb survivor data, have written letters to the editors of Science concerning the May 22, 1981 article (Attachment 5). These letters indicate that the May 22, 1981 article is misleading. On May 31, 1981 the Radiation Research Society held a workshop entitled "Late Effects Workshop Dosimetry of the Atomic Bomb Survivors." The June 19, 1981 issue of Science contained a summary of this workshop (Attachment 4). The June 19th article was entitled "New A-Bomb Data Shown to Radiation Experts" with a subtitle "conference goers are impressed with the revised

picture of Hiroshima, but foresee little change in risk estimates." [14] The subtitle of the June 19th article is in sharp contrast to the Title of the May 22nd article (i.e., "New A-Bomb Studies Alter Radiation Estimates"). This article indicates that while most of the participants agreed that the new data is preliminary (i.e., it is still being evaluated), their initial reaction is that the new data will have only a small impact on risk figures. According to the Science article Dr. Radford is of the minority opinion and:

> [He] believes that the Livermore data strengthen his argument that a linear no-threshold model is the correct one for describing the carcinogenic effects of exposure to low levels of radiation. And if this is correct, he says, the risk estimates published by the National Academy of Sciences in its 1980 report on the Biological Effects of Ionizing Radiation (BEIR) should be restated. He thinks the risks for contracting fatal cancer from radiation should be doubled. He would fix the risk at 250 to 500 excess deaths per rad of increased radiation per 1 million people, not 100 to 250 deaths, as he says BEIR and other documents have fixed it.

It should be noted that the risk estimates for whole body exposure used in the FES were based on the linear non-threshold model of BEIR I. Consequently, the range for risk estimators for whole body exposure used in the FES is consistent with the views of the majority of the members of the scientific community and even the more conservative views of some minority members such as Dr. Radford.

In regard to Dr. Morgan's fifth point that there is a tremendous amount of data to indicate that "the risk of cancer per unit of exposure is greater at low doses than at high doses," the NCRP published an extensive report on this subject entitled "Influence of Dose and Its Distribution in Time on Dose-Response Relationships for Low-LET Radiations." (NCRP No. 64). NCRP No. 64 (p. 2) stated:

> "Although extensive data from human beings permit reasonable risk assessments to be made for exposures to intermediate to high doses of low-LET radiation, these data are not adequate to demonstrate conclusively that a dose rate effect either does or does not exist. The experimental evidence...is so extensive, however, that it would be extraordinary if such dependence did not apply to the same endpoints in the human being as well."[6]

NCRP No. 64 proposed the use of a dose rate effectiveness factor (DREF) for low-LET radiation in linerally extrapolating from intermediate-to-high doses and dose rates to low doses and dose rates. The NCRP recommended that a DREF of 2 to 10 could be used to reduce risk estimates for low-LET radiation derived from either the absolute or relative risk models. All of the dose to offsite individuals from exposure to radioactive effluents from routine operations is low-LET radiation. Since the NRC Staff did not use a DREF to reduce risk estimates, it is more likely that the risk to individuals in the vicinity of the reactor has been overestimated, rather than underestimated, in the FES.

In summary, Dr. Morgan's central risk estimator for whole body exposure is higher than the highest values recommended by the major radiation protection organizations. The existing data base is adequate to estimate potential health effects. Dr. Morgan's testimony concerning the effects of radiation on human beings does not provide any substantial evidence that would change the validity of the favorable benefit-cost balance struck at the construction permit stage. The risk estimators that were used in the FES are consistent with values that can be derived from the BEIR I Report, the BEIR III Report, ICRP, and UNSCEAR.[5,7-9] The risk estimators that were used in the FES are also consistent with the

recommendations of the NCRP.[6] These organizations represent the views of the overwhelming majority of the members of the scientific community.

Q.7. What is your position on Dr. Kaku's prefiled testimony?

A. Dr. Kaku summarized his testimony as follows: (1) "There may be large uncertainties or hidden biases in the early radiation studies, especially the Hiroshima-Nagasaki and British x-ray patient data. (2) more epidemiological studies, not the consensus of standard setting bodies, can determine the real dangers of low level radiation, and (3) newer studies indicate that the standard setting bodies may have underestimated the effects of radiation." Prefiled testimony at 2.

Dr. Kaku's three points are very similar to Dr. Morgan's points. These points have already been discussed in response to Dr. Morgan's 6 reasons for questioning the accuracy of risk estimators derived from epidemiological studies. 1/

In summary, Dr. Kaku's testimony concerning the effects of radiation on human beings does not provide any substantial evidence that would change the validity of the favorable benefit-cost balance struck at the

1/ The response to Dr. Kaku's first point is in the Staff's response to Dr. Morgan's points one, two and three. The Staff's response to Dr. Morgan's first point answers Dr. Kaku's second point. The response to Dr. Morgan's fourth point replies to Dr. Kaku's third point.

construction permit stage. The existing data base is adequate to estimate potential health effects. The range of values stated in the literature is sufficient to characterize the risk. The risk estimators that were used in the FES are consistent with values that can be derived from the BEIR I Report, the BEIR III Report, ICRP, and UNSCEAR.[5,7-9] The risk estimators that were used in the FES are also consistent with the recommendations of the NCRP.[6] These organizations represent the views of the overwhelming majority of the members of the scientific community.

Q.8. What is your position on Dr. Caldicott's prefiled testimony?

A. Although a few sentences in Dr. Caldicott's testimony may have some bearing on Contention A10, it is difficult to identify their relevence to the contention. This is epitomized by her "Conclusion" which focuses on the medical consequences of nuclear war and nuclear proliferation. Prefiled testimony at 23-24.

In response to Dr. Caldicott's statement on page 23 that it is "impossible to predict the magnitude of the increased incidence of genetic disease, leukemia and cancer which will result from radioactive contamination with long lived isotopes," it should be noted that Dr. Morgan's testimony and use of risk estimators in the past contradicts the preceeding statement by Dr. Caldicott. Neither Dr. Morgan or Dr. Kaku state that the risks are "impossible to predict," but rather that there are uncertainties associated with risk estimators. Risk estimators for exposure to radioactive materials were used in the FES. Those estimators are consistent with values that can be derived from the

BEIR I Report, BEIR III Report, ICRP, and UNSCEAR.[5,7-9] The risk estimators that were used in the FES are also consistent with the recommendations of the NCRP.[6] These organizations represent the views of the overwhelming majority of the members of the scientific community.

In summary, Dr. Caldicott's testimony concerning the effects of radiation on human beings does not provide any substantial evidence that would change the validity of the favorable benefit-cost balance struck at the construction permit stage.

ATTACHMENTS

1. Professional Qualifications.
2. Comparison of FES Whole Body Risk Estimators with Values from Other Sources of Estimators.
3. Reference 11.
4. Reference 14.
5. Science, 213, pp. 6 and 8.

REFERENCES

1. USNRC, Final Environmental Statement Related to the Operation of Virgil C. Summer Nuclear Station Unit No. 1, NUREG-0719, May 1981.
2. Prefiled Testimony of Dr. Karl Z. Morgan, May 26, 1981.
3. Prefiled Testimony of Dr. Michio Kaku Concerning the Health Effects of Low Level Radiation, May 26, 1981.
4. Prefiled Testimony of Dr. Helen Caldicott, May 26, 1981.
5. Advisory Committee on the Biological Effects of Ionizing Radiation, The Effect on Populations of Exposure to Low Levels of Ionizing Radiation, National Academy of Sciences, 1972.
6. National Council on Radiation Protection and Measurements, Influence of Dose and Its Distribution in Time on Dose-Response Relationships for Low-LET Radiations, NCRP Report No. 64, April 1980.
7. National Academy of Sciences, The Effects on Populations of Exposure to Low Levels of Ionizing Radiation (BEIR III), 1980.
8. International Commission on Radiological Protection, Recommendations of the International Commission On Radiological Protection, ICRP Publication 26, January 1977.
9. United Nations Scientific Committee on the Effects of Atomic Radiation, Sources and Effects of Ionizing Radiation, 1977, United Nations, 1977.
10. K.Z. Morgan, "How Dangerous is Low-Level Radiation?," New Scientist, p. 18, April 1979.
11. E. Marshall, "New A-Bomb Studies Alter Radiation Estimates," Science, 212, 900, May 22, 1981.

12. W.E. Loewe, E. Mendelsohn, "Revised Dose Estimates at Hiroshima and Nagasaki," UCRL-85446, PREPRINT, October, 1980.

13. T. Straume, R. Lowry Dobson, "Implications of New Hiroshima and Nagasaki Dose Estimates: Cancer Risks and Neutron RBE," UCRL-85697, PREPRINT, March, 1981.

14. E. Marshall, "New A-Bomb Data Shown to Radiation Experts," Science, 212, 1364, June 19, 1981.

Professional Qualifications

My name is Edward F. Branagan, Jr. I am a Radiological Physicist with the Radiological Assessment Branch in the Office of Nuclear Reactor Regulation. Presently, I am responsible for evaluating the environmental radiological impacts from nuclear power reactors. In particular, I am responsible for evaluating radioecological models and health effect models for use in reactor licensing. I have been with the Radiological Assessment Branch for about 2 years.

I received a B.A. in Physics from Catholic University in 1969, an M.A. in Science Teaching from Catholic University in 1970, and a Ph.D. in Radiation Biophysics from Kansas University in 1976. While completing my course work for my Ph.D., I was an instructor of Radiation Technology at Haskell Junior College. My research work was in the area of DNA base damage, and was supported by a U.S. Public Health Service traineeship. My dissertation was entitled "Nuclear Magnetic Resonance Spectroscopy of Gamma-Irradiated DNA Bases."

Since joining the NRC in 1976, I have been with both the Office of Nuclear Material Safety and Safeguards (NMSS), and with the Office of Nuclear Reactor Regulation (NRR). In NMSS I was involved in project management and technical work. I was the project manager for two contracts that the NRC had with Oak Ridge National Laboratory. These contracts were concerned with estimating radiation doses from radon-222 and radium-226 releases from uranium mills. As part of my work on NRC's Draft Generic Environmental Impact Statement on Uranium Milling (DGEIS), I calculated health effects from uranium mill tailings. Upon publication of the DGEIS, I presented a paper entitled "Health Effects of Uranium Mining and Milling for Commercial Nuclear Power" at a Conference on Health Implications of New Energy Technologies. Since joining NRR, I have worked on several projects: (1) managed and main author of a report entitled "Staff Review of 'Radioecological Assessment of the Wyhl Nuclear Power Plant'" (NUREG-0668), (2) served as a technical contact on an NRC contract with Argonne National Laboratory involving development of a computer program to calculate health effects from radiation, (3) served as a technical monitor on an NRC contract with Idaho National Engineering Laboratory involving estimated and measured concentrations of radionuclides in the environment; (4) served as a technical monitor on an NRC contract with Lawrence Livermore Laboratory concerning a literature review of values for parameters in terrestrial radionuclide transport models; and (5) served as a technical monitor with Oak Ridge National Laboratory concerning a statistical analysis of dose estimates via food pathways.

Presently, I am a member of the Health Physics Society and the American Association for the Advancement of Science.

TABLE 1

COMPARISON OF FES WHOLE BODY CANCER MORTALITY RISK ESTIMATORS (PER 10^6 person-rem) WITH VALUES FROM OTHER SOURCES OF ESTIMATES[a]

Source of Estimate	Dose-Response Models[b]	Projection Model: Continuous Lifetime Exposure to 1 Rad/Yr (Low-LET) Absolute	Relative
BEIR, 1980	LQ-L, $\overline{\text{LQ-L}}$	67	169
1972 BEIR[c]	Linear	115	568
UNSCEAR 1977	Linear	75-175	
ICRP[d]	Linear	100-125	
FES[d]	Linear	135	500

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a Except where noted all values are taken from Table V-4 of BEIR III.

b For BEIR 1980, the first model is used for leukemia, the second for other forms of cancer. The corresponding estimates when the other models are used (thereby providing an envelope of risk estimates) are:

L-L, $\overline{\text{L-L}}$	158	403

c Updated to 1970 U.S. population.

d Values for the FES and ICRP are taken from Ref. 1 and 8, respectively.

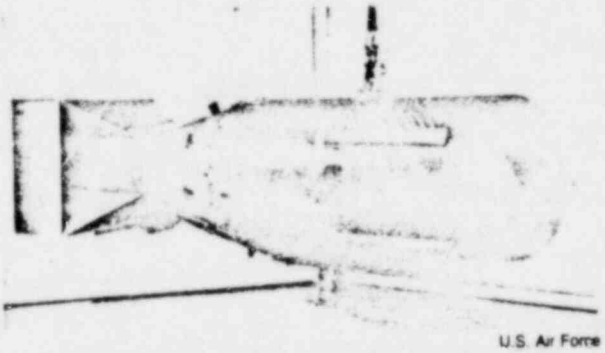
New A-Bomb Studies Alter Radiation Estimates

The basis of 15 years of radiation research may be in error; radiation toxicity may be understated

Some of the most important data on the effects of nuclear radiation on humans may be wrong, according to new research being done at the Lawrence Livermore weapons laboratory in California and the Oak Ridge National Laboratory in Tennessee. The new findings are far from welcome, as one consultant in this work says, for all the revisions "are moving in the wrong direction"—a direction that will worry the advocates of nuclear power. Government physicists have recalculated the data on the radiation fields created by the atomic blasts at Hiroshima and Nagasaki and produced some unexpected results. Their statistics show that most of the cancer caused by those bombs came from low LET gamma rays,* suggesting that this common type of radiation is more hazardous than had been assumed before.

The impetus for the revision comes primarily from Livermore, where physicists William Loewe and Edgar Mendelsohn last year used a computer to reconstruct the two explosions. Their findings are being checked and complemented by a group at Oak Ridge led by George Kerr. He began work on a similar project in 1977, shelved it, and then returned to the task in earnest when Loewe's data became known. Dean Kaul of Science Applications, Inc., in Chicago also carried out some early calculations that sparked interest in the issue. Kerr, Kaul, and Jess Marcum of Research and Development Associates in Santa Monica, California, have been funded by the Defense Nuclear Agency to explore the problem and check some of the old assumptions which have not yet been reexamined.

Although they differ in some of the details they stress, all of these scientists agree that the accepted figures for high LET (neutron) radiation at Hiroshima are grossly overstated. For example, the neutron radiation at a distance of 1180 meters from the epicenter of the blast appears to have been overestimated by a factor of 6 to 10. Since the effects on human health remain the same, one must conclude that the gamma rays were more toxic than had been thought.



U.S. Air Force

Did it produce neutrons or mostly gamma rays?

Duplicate of the bomb that hit Hiroshima

If this research proves correct—and it has survived a few peer challenges already—it will necessitate the rewriting of many basic documents on the hazards of radiation, including the chief attempt to define such risks published in 1980 by the National Academy of Sciences. That study, the work of the Committee on the Biological Effects of Ionizing Radiation (the BEIR report), was fraught with controversy on this very question.

Although much of the BEIR report was released to the press in May 1979, the Academy decided to recall and rewrite it because of dissension among the authors. Some of them, led by Columbia University biophysicist Harald Rossi, argued that the paper overstated the cancer-causing effects of low LET radiation. Their arguments leaned heavily on Japanese data and particularly on the thesis that many of the cancers in Hiroshima were produced by high LET neutron radiation.

Using the old Hiroshima radiation data as evidence, Rossi argued that the BEIR committee should lower the cancer risk estimates published in an earlier BEIR report in 1972. Instead, the committee raised the risk estimates. Rossi considered this an alarmist move and withdrew his support from the document. In the end, the Academy felt compelled to write a report that effectively split the difference between Rossi's point of view and that of his chief adversary, the committee chairman, Edward Radford, an epidemiologist at the University of Pittsburgh. The risk estimates in the final report of July 1980 were not as high as Radford argued they should be nor even as high as those in the 1972 report. Neither Radford nor Rossi endorsed the document.

Rossi concedes that the Livermore calculations may do away with the evidence for his theory that neutrons were responsible for the high cancer incidence in Hiroshima. But he does not expect to alter his general view that the hazards of radiation are exaggerated. Radford, in contrast, says the new Hiroshima data vindicate his position and invalidate Rossi's. Furthermore, Radford considers the BEIR 1980 report obsolete and expects that the probabilities it gives for the risk of dying of cancer after exposure to gamma radiation will be doubled. Likewise, he thinks the probabilities for contracting any form of cancer after irradiation will be quadrupled.

The importance of the new research is that it completely changes the scheme of radiation doses that people are supposed to have received in Japan, particularly in Hiroshima. Until now, it was thought that the Hiroshima blast was unique in that it produced a large field of fast neutrons, a high form of radiation. Neutron radiation considered more dangerous than low LET radiation, a category that includes x-rays, electrons, and gamma rays. Its singular presence in Hiroshima was said to make the cancer risk found there anomalous. Most of the radiation people encounter is not of this kind. The wastes from nuclear reactors, for example, emit gamma rays. Thus, a

*The terms "low LET" and "high LET" (for linear energy transfer) refer to the physical quality of the ray. Low LET radiation loses relatively little energy as it travels along its course, and includes electrons, gamma rays, and x-rays. High LET radiation loses energy more rapidly as it travels, and includes beams of neutrons and protons.

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number of scientists have always considered Hiroshima a special, high-risk case, and in studying the peacetime hazards of radiation, they have discounted some of the cancer data from that city.

As it happens, the cancer mortality data from Hiroshima are the most valuable in the world. Unlike the data from Nagasaki, they are abundant enough to reveal a clear relationship between doses of radiation received and ill effects. That relationship is defined by a linear equation: an increase in dose above the natural background radiation correlates with a proportional increase in ill effects. The pattern suggests that any increase in radiation, no matter how small, directly increases the risk of getting cancer. The mortality data from Nagasaki are sketchier, making them susceptible to a variety of interpretations. The significant point is that if the new bomb calculations are accurate, the data from Nagasaki and Hiroshima can be combined and treated as a single, coherent pattern of response to low LET radiation. It is too early to say precisely what that pattern will look like, because now the doses must be recalculated for each radiation victim. But most of the researchers who spoke to *Science* said the new data would probably increase the risk estimates for gamma radiation.

Radford, an advocate of this point of view, claims that the argument over Hiroshima and its mortality data has been a distraction from the main body of scientific evidence. He says the 1980 BEIR report miscalculated in emphasizing mortality data so heavily, for death certificates do not give a very accurate reading of the number of cancers or even cancer deaths in a community. Radford thinks it was a mistake to pay so much attention to Rossi's theory about deaths in Hiroshima, for he claims the theory is contradicted by "90 percent" of the epidemiological data on record. He is pleased that the Hiroshima data may now look consistent with all the rest.

"The implications are far reaching for health regulation and nuclear power in this country in general," says David Auton, a physicist in the office of target and damage assessment of the Defense Nuclear Agency. His office is funding the research at Oak Ridge that may confirm the new dose estimates. As he describes the situation, the health physics community faces a nasty dilemma, if the new bomb data are accurate. On one hand, the standard-setters may adhere to Rossi's principle, which maintains that many of the cancers produced in Hiroshima were caused by fast neutrons. But the number of neutrons thought to have been present is now so small that one must account for their effects by increasing the estimate of their potency. The resultant killing power of neutrons is "incredible," Auton says. Industrial safety rules would have to be revised, reducing exposure limits for neutron radiation to one-tenth of the present limits. For critical jobs, companies would have to employ ten times as many people.

On the other hand, the health physics community may abandon the Rossi principle and conclude that nearly all the cancers in Hiroshima were produced by gamma rays, not neutrons. That news will not be welcome either.

Auton wishes frankly that someone else were funding this research, which he thinks is important for future health and energy policy. His office is doing it because "nobody else was interested." The controversy has been brewing for at least 4 years, for that is how long it has been since a government consultant first raised serious questions about the validity of the Hiroshima data. According to Auton, however, it was just 5 months ago that he was approached by Harold Wyckoff, chairman of a special committee assigned to study this question for the National Council on Radiation Protection and Measurements. It is a private organization that collects and publishes radiation risk information. Since no other agency would fund the research, Auton says, he agreed to have the Defense Department pick up the tab for work being done at Oak Ridge, and thus come up with some answers for Wyckoff. The funding began about a month ago.

"This work is of marginal interest to us and we really can't afford to spend very much money studying civil effects," Auton says, but it is important to resolve the uncertainties. It might make more sense for the Department of Energy or the Nuclear Regulatory Commission to pay for this work, and "the electric power people really should be interested," according to Auton. It is important that the new research be credible. Auton agrees that it would be best if the sponsor were an independent group not associated with the weapons program or the nuclear industry.

U.S. Air Force

Hiroshima, 1945

Some concrete buildings survived the blast.

Arthur Upton, the former director of the National Cancer Institute and an expert in radiobiology, has followed this controversy closely since he learned of the new bomb data last fall. It is an important issue, he says, and should be the subject of more research, sponsored by a neutral scientific organization such as the joint U.S.–Japanese Radiation Effects Research Foundation. If the new dose estimates are correct, Upton says, "I am not sure one can substantiate the Rossi thesis." It may remain important for radiobiology, for there are differences in the way that plants and animals respond in the laboratory to high and low LET radiation. Upton agrees with Radford that the new data greatly strengthen the argument that there is no "safe" level of exposure to radiation, in that every incremental bit of exposure increases the chances of injury.

One of the curious aspects of this research is the manner in which it was published. The record serves as a compelling argument for declassifying as much as possible of what is done at government labs, for many of the assumptions in this case might have been challenged sooner had the underlying data been available for scrutiny.

The Rosetta stone of Japanese radiation dosimetry is known as T65D, which stands for tentative dose estimates compiled in 1965. The figures were assembled by physicist John Auxier of Oak

Ridge in a painstaking analysis of measurements made during and after the Japanese blasts, interviews with the bombardiers, and a test explosion in the Nevada desert. Some of his work was classified because it described in detail the makeup and radioactive output of the Little Boy (Hiroshima) and Fat Man (Nagasaki) bombs. Auxier's methods of computing the doses, which underlie 15 years of research on health effects in Japan, were never described in detail. In 1977, however, the government published a quasi-technical narrative by Auxier (*Ichiban*, Energy Research and Development Administration, TID 27080) giving some additional information on Auxier's methods.

As questions about these figures arose in the late 1970's, the National Council on Radiation Protection (NCRP) asked Auxier to justify his estimates with more supporting information. After working on this project for several months, Auxier explained that he could not reproduce all the data because some had been lost. He explained to *Science* that when Oak Ridge was reorganized in 1972, he was moved from one place to another, and his old classified files were left behind in his laboratory. Auxier says that the records division at Oak Ridge made a mistake in shipping the files: the valuable data were sent to the shredder.

The NCRP continued to ask for confirmation of the T65D numbers because they had become important in the debate on the hazards of radiation and because new data were becoming available. In 1976, the Los Alamos Scientific Laboratory in New Mexico, a weapons design center, released an estimate of the radioactive output of the Hiroshima bomb for the first time. The figures were not published, but given in a private letter to C. P. Knowles of Research and Development Associates, who was trying to help the Defense Nuclear Agency pin down the precise explosive power of the Fat Man bomb. This is one of the key uncertainties in the record; some say the blast equaled the power of 12.5 kilotons of TNT, and others say it may have been as potent as 15 kilotons. Several people in the weapons and biophysics community soon obtained copies of the letter, including Kerr at Oak Ridge and Kaul at Science Applications. Using the new data and computer techniques not available when Auxier did his research, Kaul and Kerr in separate projects came up with numbers that were at odds with the T65D results.

Kerr's laboratory is the best equipped and best funded for this expensive computer work, Kaul says, and for that reason it has been given the primary responsibility for reviewing the old numbers. Kerr's task is complicated by the fact that he is in a sense Auxier's successor at Oak Ridge and works just down the hall from this senior official whose work he has been asked to review.

Auxier, meanwhile, says that his data are the best available, not likely to be changed much by the work of latter-day

Technology Transfer Reappraised

Transfer of technology from industrialized countries to developing countries emerged in the 1970's as a highly charged issue in the so-called North-South dialogue. Less-developed countries protested that control of technology by the industrialized North keeps them in a state of technological dependence.

A report* just issued by the Organization for Economic Cooperation and Development (OECD) in Paris questions major assumptions on which the technology transfer debate has been conducted. It argues that technology transfer has been mutually beneficial for industrialized and for developing countries, or at least some of them.

The report notes that technology transfer has helped a group of "industrializing" developing countries to participate, on stronger terms, in the world trading system. These include Brazil, Mexico, South Korea, Taiwan, Hong Kong, and Singapore.

The report's main challenge to the notion of technological dependence is its assertion that "technological monopolies are temporary," that change is propelled by a "technology cycle." New technology introduced in one country is transferred under tight control first to other developed countries and then to less-developed countries. As licensing and sale of the technology spreads, it becomes standardized.

Proof that this process is working is seen in the rise in imports by industrial countries of manufactured goods from developing countries. Moreover, some industrializing countries are themselves exporting technology, mostly in the form of turnkey plants and equipment.

Feedback from technology transfer also affects industrial countries. The impact has been most conspicuous in the decline of traditional industries, notably clothing, footwear, and light manufacturing, that have faced offshore competition. Loss of jobs has created a protectionist backlash that includes criticism of technology transfer. But, says the report, technology transfer has benefited the United States and other OECD countries by creating export markets for their capital-goods industries during a period of slow growth.

By focusing on the industrializing countries, the report offers a selective view of the problems facing developing countries. It does note in passing that for the poorest countries, the cost of imported oil, trade deficits, and foreign debt make the outlook bleak. Even for the industrializing countries, the burden of energy costs, deficits, and debt have "led to pessimism regarding future financing of development."

The report was prepared by the staff of OECD, which is essentially a club of governments of western industrial nations plus Japan. OECD serves as a data gathering and intergovernmental policy-planning organization. It is, therefore, not surprising that the report assesses technology transfer mainly from the sellers' point of view.

In broad terms, what the report's authors say is occurring is a major restructuring of the international industrial system. For the industrial countries an "adaptive strategy" is counseled. With a two-way trade in industrial products now established, the North can retain its comparative advantage only by keeping its "innovatory capacity" at a high level. Pressure to transfer R & D activities to developing countries will build as their scientific infrastructures strengthen. The report borrows from Lewis Carroll to observe that industrial countries must "keep running to stay in the same place."—JOHN WALSH

**North/South Technology Transfers: The Adjustments Ahead*, Organization for Economic Cooperation and Development, Paris, 1981. $12.

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revisionists. His judgment is widely respected. As the grand old man in this field, he is in a position to influence funding decisions on new research. Auxier told *Science* there is no need for an independent review of the discrepancies between his data and Kerr's, expressing an opinion which may have made it difficult to get the present review started. Auton, the Defense Nuclear Agency official who makes the funding decisions, says that he has great respect for Auxier's work, a respect based as much on Auxier's standing in the community as on his ability to "drag out corroborative data."

Kerr has never published any of his work outside the laboratory, he says, because he prefers to be "timid" about it. Earlier controversies have taught him to move cautiously in matters as important as this, and he still thinks there could be some weaknesses in the new bomb data.

This stalemate existed for several years until the summer of 1980 when Loewe decided to rework the calculations. He started the project because the old Hiroshima data and Rossi's recent warnings about the potency of neutrons worried people in the lab. Livermore scientists are involved in weapons research and are frequently exposed to neutron radiation. They wanted to know more about the dangers. Loewe's investigation, completed last October, found both the Hiroshima data and Rossi's principle to be unsubstantiated. Loewe argues that there is no evidence showing that neutrons were present in significant quantities in Hiroshima.

Loewe, Kerr, Auxier, and others in this controversy will present their arguments at a meeting sponsored by the Radiation Research Society on 31 May in Minneapolis. Auton calls it "the beginning of an important dialogue," one which he probably will not be able to attend because the new Administration has reduced the bureaucracy's travel allowances. But Auton hopes the meeting will lead to a general and independent review of the issues. "If the weapons folks" make it a strictly internal project, he says, "I just have a concern that nobody will believe the results."

—ELIOT MARSHALL

Science Adviser Post Has Nominee in View

The job, turned down by several candidates, may now be offered to a man who is not a member of the science establishment

The choice of science adviser to President Reagan has been narrowed down to a single candidate: George A. (Jay) Keyworth, a 41-year-old physicist from the Los Alamos Scientific Laboratory. Although the job had not formally been offered to Keyworth as of this writing, Administration officials expect an announcement by the end of May, but caution that something could still go awry even at this late stage of the selection process.

When Keyworth's name came up as a potential candidate late in April, it drew a mixture of surprise and unease from the scientific establishment. The surprise stems from the fact that Keyworth is virtually unknown outside his field. And the unease is related to the fact that his candidacy was being vigorously supported by Edward Teller, the so-called "father of the hydrogen bomb," and Harold Agnew, president of General Atomics and former director of Los Alamos. Both are well known for their hawkish defense views.

Those who know Keyworth describe him as smart and personable. His research has been concerned mostly with nuclear structure and low-energy nuclear reactions, and for the past 3 years he has directed the physics division at Los Alamos. One scientific colleague, Arthur Kerman of MIT, describes Keyworth as "a very good scientist who is a lot broader than his background would indicate."

Outsider causes unease

Candidate George Keyworth

His background does not, however, include service on the usual round of government science committees. Hence he has little experience with federal science policy and has made few links to the scientific establishment. "He doesn't provide any channel between the national (scientific) community and the White House," complains one veteran of science and government affairs.

Such concerns are abruptly dismissed by Keyworth's supporters. Although he "lacks obvious credentials, that doesn't mean he will not do a superb job," says one. Agnew scoffs that "he has all the right credentials—all he doesn't have is 20 years membership in the club." In a telephone interview with *Science*, Agnew also said that he thinks much of the unease about Keyworth is simply due to the fact that he is an outsider—"If you get a bunch of chickens together and you put in a new rooster, they start clucking and running around," he remarks.

As for Keyworth's shortage of links to the scientific establishment, Agnew says that "defense will be the thrust of this Administration, and somebody who has the respect of the people in the defense labs is needed." He adds: "For the past four years, you have had a geologist in charge, and the defense community has suffered."

How did somebody from outside the traditional ranks of candidates for science adviser get selected? Keyworth says he was approached about the job early in April, and "it came as a surprise to me." The post was formally offered in March to Arthur Bueche, head of research and development at General Electric, but he was forced to turn it down for personal reasons. Several other people were subsequently sounded out about

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New A-Bomb Data Shown to Radiation Experts

Conference goers are impressed with the revised picture of Hiroshima, but foresee little change in risk estimates

Minneapolis. Physicist William Loewe spoke at the annual meeting of the Radiation Research Society here on 31 May and gave the first public presentation of the work he and Edgar Mendelsohn have done at the Lawrence Livermore National Laboratory. They have drastically revised the estimates of radioactive fallout from the Hiroshima and Nagasaki atomic bombs. The most important single finding they reported was that no neutron radiation of any statistical significance was present at Hiroshima, suggesting that nearly all the bomb-related cancers were produced by gamma rays. If correct, this means there are no good human data for judging the toxicity of neutron radiation.

The audience was receptive, and several old hands said they found Loewe's work impressive. No general consensus was reached on whether or not Loewe's data should replace the old estimates of atomic radiation prepared in 1965 by John Auxier of the Oak Ridge National Laboratory.

Most of the participants agreed on one thing, however: they were unhappy with the way the news of the possible revision was reported, and they were annoyed by the interpretation given by University of Pittsburgh epidemiologist Edward Radford, who has said that it may be necessary to double or quadruple the risk figures for getting cancer after exposure to radiation (*Science*, 22 May, page 900). Speaker after speaker echoed the theme sounded early in the meeting, that not enough work has been done to permit a conclusion such as the one Radford reached. Harold Wyckoff, chairman of a task force created in 1976 expressly to review this problem for the National Council on Radiation Protection and Measurements (NCRP), said, "I would strongly disagree with anyone using this data to determine risk coefficients." It is too early to do that, he said.

Loewe agreed that it would be wrong to draw broad conclusions based on his preliminary work, but he did tell the Minneapolis *Tribune* that he thought the new data will have a negligible impact on risk figures. Others, such as Warren Sinclair, president of the NCRP and an organizer of the meeting, were stronger in their denunciation of Radford, suggesting that the new Livermore data may even make radiation look less harmful than before.

If the sponsors of the meeting were unhappy with the way Loewe's work was presented to the public, other members were as unhappy with the way the information had been circulated (or not circulated) within the community. Perhaps the most outspoken was Seymour Jablon, the National Academy of Sciences' staff officer for joint U.S.–Japanese research on late effects of atomic radiation. He is a veteran observer.

Jablon rose during the general discussion to make three points. The NCRP has known since 1976 that there might be flaws in the Japanese data, he pointed out. "Meanwhile, the EPA is busy setting [occupational radiation] standards; other people interested in standards have been making noises. It really is urgent that we get on with this job. . . . Given the unique experience at Hiroshima and Nagasaki and the tens of millions of dollars which have been spent trying to accumulate the human biological data, it really is appalling to think that we stand here, 36 years later, debating orders of magnitude in the doses." He pleaded with federal officials present to give aid to complete the research quickly and shore up the $100 million investment in Japanese data.

> "Given the unique experience at Hiroshima . . . it really is appalling to think that we stand here, 36 years later, debating orders of magnitude in the doses," Seymour Jablon said.

Second, Jablon said, "I think it's going to be absolutely necessary in this murky situation that any dosimetry system that is finally decided upon be reasonable in terms of biological influences that we know about. . . . And since the problem is of wider scope than merely physics, perhaps it would be advisable to consider adding some biological expertise to the [NCRP] task force."

Third, Jablon said, "I think that the way this whole problem developed is very unfortunate. Most of us, certainly I, heard about the problem . . . by word of mouth. The next thing was to receive pieces of paper which were not for publication, quotation, or citation. . . . I am told the Japanese Diet is about to have a debate on the subject, and still there is nothing published that one can point to and rebut or accept or whatever."

NCRP President Sinclair responded by saying that there was already one biologist on the NCRP task force, and that he would consider adding more when an attempt is made to extrapolate health effects from the bomb data. One of the physicists who has been at work on the problem the longest, George Kerr of Oak Ridge National Laboratory, said that he thought the data had not been published sooner because they were not strong enough to stand up to peer review. (Two relevant papers have now been submitted to *Health Physics* as technical notes: "Revised dose estimates at Hiroshima and Nagasaki," by Loewe and Mendelsohn, and "Implications of new Hiroshima and Nagasaki dose estimates: Cancer risks and neutron RBE," by Tore Straume and R. Lowry Dobson.)

Radford, who is not a member of the Radiation Research Society, skipped the meeting. He expressed disappointment, however, at the attitude that "we can't say anything until we have everything in hand," as he described it. According to Radford, that attitude can be used to delay reaching any conclusion: "It's what the tobacco industry did for years with the epidemiological evidence relating cancer to smoking. They just said,

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"Well, that last study wasn't perfect, so we'll ignore it.' "

The net effect of the new research, Radford insists, is not hard to summarize: the radiation data for the two cities of Hiroshima and Nagasaki are now likely to come out looking very similar. "You can state that as a general principle," says Radford, "and I do state it. That being the case, they confirm the fact that it was primarily gamma rays that produced the cancers, and that the neutrons, for all practical purposes, contributed so little that they're not important."

Radford believes that the Livermore data strengthen his argument that a linear no-threshold model is the correct one for describing the carcinogenic effects of exposure to low levels of radiation. And if this is correct, he says, the risk estimates published by the National Academy of Sciences in its 1980 report on the Biological Effects of Ionizing Radiation (BEIR) should be restated. He thinks the risks for contracting fatal cancer from radiation should be doubled. He would fix the risk at 250 to 500 excess deaths per rad of increased radiation per 1 million people, not 100 to 250 deaths, as he says BEIR and other documents have fixed it. Radford would also like to see the risks stated in terms of cancer incidence, not mortality, so as to recognize that real injury is done by cancers which do not necessarily kill. Including these figures, Radford says, would make it necessary to further raise the main risk coefficient used in the BEIR report.

Loewe did not discuss Radford's interpretation at the meeting, except to say that he could not understand how such views could be supported. Loewe said he did not see how one could draw a straight line through the old or new radiation effects data. Indeed, two scientists from Livermore who have been working in conjunction with Loewe, Tore Straume and R. Lowry Dobson, presented a paper suggesting that the new bomb data may lower the risk estimates for low doses of gamma radiation. They, too, were skeptical of all that Radford had said.

So many variables have been cited in this controversy that it may be worthwhile explaining just which data belong to whom. Radford, first of all, has done no new research on this issue. He is an epidemiologist with strong opinions on the subject, and he has seized upon Loewe's work as fresh evidence to support his view that many documents understate the hazards of low-level radiation. Radford also says that in defending this outlook he is working against the professional bias of health physicists, which, he claims, is to minimize the dangers of radiation.

Harald Rossi is a Columbia University biophysicist who challenged Radford's views as alarmist when both were serving on the BEIR committee. (Radford was the chairman.) Rossi argued that the hazards of gamma radiation were exaggerated, and he cited the Japanese bomb data to support his case. As part of this thesis, Rossi put forward the idea that many of the fatal cancers at Hiroshima had been caused by neutrons, not gamma rays. Neutron radiation is found rarely in nature, and as a practical matter it is of concern only to people exposed to nuclear weapons and the innards of operating nuclear plants. Rossi's work prompted the NCRP to send out a special advisory to weapons laboratories warning them that their safety standards might be inadequate because neutrons might be more dangerous than had been thought. That was 3 years ago.

Loewe and Mendelsohn were swept into this debate in 1979 because they worked at Livermore, a weapons lab, and were concerned about the NCRP advisory. Livermore did not change its safety standards, but it did finance some computer work by Loewe and Mendelsohn, who attacked the evidence for Rossi's thesis. Their calculations, now made public, do not demonstrate that neutrons are safe. They simply show that neutrons were so scarce in the Japanese blasts that one cannot measure their effects with accuracy. At the same time, the Livermore work significantly *increases* the estimate of gamma radiation in Hiroshima and slightly *decreases* the gamma radiation in Nagasaki.

Using this data, Dobson and Straume have made preliminary new estimates of the toxicity of gamma and neutron radiation. Their paper concludes, among other things, that if one uses the total cancer deaths as a guide, low doses of gamma radiation look less harmful than before. (Other statistical guides produce different results.) They also suggest that it may still be possible to blame the small number of neutrons in Hiroshima for many of the cancer fatalities. Asked about this part of the Straume-Dobson paper, Rossi said he considered it just "an interesting exercise," no more. He believes that if the Livermore data are correct, they will make it impossible to say anything conclusive about neutrons in Hiroshima.

An important caveat applies to all of the recent work on radiation in Japan: it does not include corrections for changes in the shielding provided by buildings or by body tissue. According to Jess Marcum, a contractor for Oak Ridge for a review of the data, significant revisions of the Livermore dose estimates may be necessary before one can reach a conclusion on toxicity. Marcum says he has spent about 1 month researching shielding by buildings and has discovered that the estimates of gamma doses in many cases will have to be lowered. In the area of interest, 1000 to 1700 meters from the epicenter of the blast, Marcum calculates that indoor gamma ray doses will have to be reduced by a little more than 60 percent. The net effect, he believes, will be to make gamma doses for individuals in Hiroshima about the same as in the old estimates produced at Oak Ridge in 1965, while the Nagasaki doses will be lower than the 1965 figures.

According to Jess Marcum, significant revisions of the Livermore dose estimates may be necessary before one can reach a conclusion about toxicity.

In addition, George Kerr of Oak Ridge is recalculating the shielding effect of body tissue for certain "target" organs such as the breast, thyroid, colon, and so on. Marcum reports second hand (Kerr is in Europe) that the net effect of this final adjustment may be to produce no change in the leukemia risk factors for the two cities, but to increase slightly the risk for breast cancer, bringing the latter into agreement with U.S. medical data on breast cancer caused by x-rays. If true, this is an "extraordinary conclusion," Marcum says, because it will give credibility to the research done by Loewe, Marcum, and Kerr, as well as to the Japanese epidemiological data.

One of the few things that is clear in all this is that Livermore's research has irreversibly toppled the status quo. It also seems clear that the federal government would be well advised to finance the work necessary to bring a new estimate of radiation dosimetry into focus as quickly as possible.—ELIOT MARSHALL

Letters

Radiation Estimates

The article by Eliot Marshall on the estimates of radiation dose received by the survivors in Hiroshima and Nagasaki (News and Comment, 22 May, p. 900) is essentially accurate concerning what various people have said about the situation, but it gives an erroneous view of the implications. It is unfortunate that, although the revisions proposed by William Loewe and Edgar Mendelsohn at the Lawrence Livermore National Laboratory of the T65 dosimetry have been widely circulated in summary form, they have not been published and therefore have not yet been subjected to the scrutiny of the scientific community. The Livermore revisions are being publicized instead by individuals whose statements concerning them are unfortunately, sometimes in error. Thus, Edward Radford, in a recent statement to the Environmental Protection Agency concerning the proposed Federal Radiation Protection Guidance for Occupational Exposures, stated that ". . . there is indication from the new evaluation that the gamma ray doses in both cities have been overestimated in the 1965 data, and for this reason the risk estimates previously derived even on the linear dose-response hypothesis are too low" Radford is wrong on both counts: Loewe and Mendelsohn assert that the gamma dose in Hiroshima was grossly *under*estimated in the T65 dosimetry, while the gamma doses in Nagasaki and the neutron doses in both cities were overestimated. The net result of all this is that the risks for cancer, per rad, estimated on the linear hypothesis, change very little.

Radiobiologists and students of radiation carcinogenesis have been intensely interested in the contrast between the effects of high LET and low LET (linear energy transfer) radiation because of the light that might be cast on the nature of the intracellular event that results in a cancerous cell. This discussion will have to begin again from square one if the Livermore dosimetry revision, or anything like it, is deemed to be the most likely after probing scrutiny by the scientific community. But the usefulness of the Hiroshima-Nagasaki data for the purposes of radiation protection is not challenged by this dispute concerning the dosimetry, nor are the actual linear risk estimates affected appreciably.

As Marshall makes clear in his article, the workers at Livermore reexamined the Hiroshima-Nagasaki dosimetry at this late date under the impetus of Harald Rossi's contention that the risks from neutrons are larger than had previously been supposed. Since the weapons laboratories have a real stake in the question of the magnitude of risks from neutrons, and hence the appropriate exposure limits, they cannot be regarded as disinterested parties. As Marshall says, the record of this controversy is a compelling argument for bringing the data into the public arena through the traditional modes of open publication and peer review and discussion. It is unacceptable to base discussion of important public policy decisions, such as occupational exposure limits, on rumor, hearsay, and privately circulated, privileged documents.

SEYMOUR JABLON
Advisory Committee on the Radiation Effects Research Foundation, National Academy of Sciences–National Research Council, Washington, D.C. 20418

We wish to comment on Marshall's article dealing with our new estimates of the Hiroshima and Nagasaki radiation doses (really free-in-air kermas). Of concern to us is an inference drawn by Marshall and by some of the persons whom Marshall interviewed and quoted; this inference in turn sets the tone for the entire article. We take exception to statements that our results show gamma radiation is much more hazardous than previously assumed.

Marshall states that our much lower neutron dose estimates for Hiroshima imply "that the gamma rays were more toxic than had been thought." He fails to mention that the reduction in neutron dose was accompanied by a substantial increase in gamma dose, which counters that argument. The following example illustrates that point:

Hiroshima doses at 2 kilometers

Estimates	Tissue rads	
	Neutrons	Gamma rays
T65D*	0.54	1.9
LLNL†	0.064	7.7

*Tentative dose estimates compiled in 1965. †Lawrence Livermore National Laboratory estimates.

For Nagasaki, the T65D estimates have shown a predominant gamma dose. Therefore the radiobiological effects observed in Nagasaki have previously been attributed solely to gamma radiation. Although different, to first order our gamma dose for Nagasaki is similar to that of T65D. Thus any conclusions drawn in the past concerning the radiobiological effects of gamma radiation in Nagasaki could not change significantly on the basis of our new dose estimates alone. In addition, leukemia data for Hiroshima now tracks the Nagasaki experience when plotted against our dose values (*1*). We therefore fail to see how our work can be a basis for assigning a greater hazard to gamma radiation than was the case for T65D. (However, see below for additional discussion.)

We believe that definitive conclusions concerning the implications of our new doses with respect to radiobiological effects cannot be drawn until further work has been completed. The data base containing information on the individual Japanese survivors must be revised to reflect our new estimates. In addition, a recalculation of gamma building transmission factors [they might be reduced by a factor of roughly 1.6 (*2*)] and body transmission factors appear to be important. Such new calculations would increase the gamma radiation risk, in proportion to any decrease of the transmission factors.

We also wish to correct the following statement in Marshall's article: "This stalemate existed for several years until the summer of 1980 when Loewe decided to rework the calculations." A correct statement would be: "Unaware of ongoing work at Oak Ridge National Laboratory, in 1978 Loewe and Mendelsohn carried out what they believed to be the first calculations of Hiroshima and Nagasaki doses to have an air-ground interface explicitly included. In August

1980, after identifying significant errors in previous calculations elsewhere and establishing agreement of their own calculations with in situ data at Hiroshima, they presented their results in a preliminary but detailed report which received widespread distribution."

WILLIAM E. LOEWE
EDGAR MENDELSOHN
Lawrence Livermore National Laboratory, University of California, Livermore 94550

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1. W. E. Loewe and E. Mendelsohn, "Revised dose estimates at Hiroshima and Nagasaki" (UCRL 85446 preprint, 1 October 1980), available from Lawrence Livermore National Laboratory, Livermore, Calif.; *Health Phys.*, in press.
2. J. Marcum, "House attenuation factors for radiation at Hiroshima and Nagasaki" (Memorandum, R & D Associates, Marina del Rey, Calif., 15 May 1981).

A number of conclusions in Marshall's article do not follow from the new dose estimates for Hiroshima and Nagasaki calculated by Loewe and Mendelsohn.

We have recently reported (*1*) an analysis of the biological implications of the new doses. The Loewe-Mendelsohn estimates (*2*) were combined with published biomedical data from the Japanese A-bomb survivors (*3*) and dose-response relations were analyzed for several major effects, including leukemia, breast cancer, and total malignancies. What the combined data show can be summarized as follows.

• The mortality data for leukemia and total malignancies show dose-response curves for low LET (linear energy transfer) that are clearly sigmoidal in shape; the data cannot be fitted adequately by linear regressions. The carcinogenic efficiency (effect per unit dose) of gamma rays is less at low doses than it is at high doses. In the case of breast cancer incidence, although the situation is not so clear, the data are again well fitted by a sigmoidal (linear-quadratic) curve.

• Because of statistical uncertainties in the A-bomb–survivor data, the dose-response curves are still unable definitively to demonstrate either the presence or the absence of low LET "threshold" doses for human malignancies.

• Risk coefficients for leukemia and breast cancer are consistent with generally accepted values (*4*) at low doses. The low LET coefficient for total malignancies, however, appears lower than the earlier estimates (*4*). Only at high doses, above those relevant to radiation protection standards, do the risk coefficients (for certain malignancies, for example, leukemia) become significantly higher.

• Clear evidence for high neutron RBE (relative biological effectiveness, compared to low LET radiation) is lacking from the new dose-response curves for leukemia and breast cancer (in both cases curves for the two cities are not significantly different from each other). In the case of total malignancies, however, significant differences between the cities are seen. Table 9 gives more excess deaths in Hiroshima than in Nagasaki at any given dose. If these differences are a result of the larger neutron component in Hiroshima (the new dose estimates still show more neutrons in Hiroshima than in Nagasaki), the data suggest that the RBE of neutrons increases with decreasing dose (because of the decreasing effectiveness of gamma rays) and may reach quite high values; interestingly, a significant fraction of the A-bomb–related cancer deaths would appear to be the result of neutron radiation [this was also found for the earlier T65 doses (*5*)].

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5. H. H. Rossi, *Radiat. Res.* 84, 395 (1980).

Marshall generally associates those named in his article on Japanese A-bomb survivor dose estimates with the proposition that the likely outcome of dose reassessment will be to increase the risk of cancer ascribed to exposure to low LET radiation. I wish to disassociate myself from this thesis.

The reanalysis of the Japanese dosimetry is a complex task, affecting not only the neutron dose but the gamma ray dose as well. Evidence presented to date suggests that, while estimates of neutron doses at both cities may increase, the estimate for the Hiroshima gamma ray dose may increase relative to that at Nagasaki. The degree to which these variations may occur has not yet been established and depends on the device radiation output, atmospheric conditions, and local shielding. To date, only the effects of the former two parameters have been investigated by Science Applications, Inc., Oak Ridge National Laboratory, and Lawrence Livermore National Laboratory.

It may well be asked why the T65D estimates are being questioned now, 15 years after their publication. The reason for this is that the T65D values were derived almost wholly from measurements made during field tests of weapons and other radiation sources in the 1950's and early 1960's. As such, claims of substantial accuracy could be made for them at a time when purely analytical methods of predicting device radiation output, transport and shielding effects were in their infancy and their results subject to considerable uncertainty. Since that time considerable advancement has been made in the development of analytical techniques and the physical data required for their use. Within the last 5 years this advancement has reached the stage at which the dose measurements made during the atmospheric tests can be reproduced analytically to within the uncertainty of the measurements themselves. The claim by Rossi and Mays (*1*) that the risk of leukemia from neutron exposure should be raised by a factor of 10 based on current A-bomb survivor epidemiology and the T65D estimates provided the impetus for the reanalysis of survivor dosimetry, using state-of-the-art analytical techniques, and precipitated the current controversy.

State-of-the-art analytical methods and data may be used successfully to produce highly accurate dose reconstructions for Japanese A-bomb survivors. However, current results from such efforts must be considered incomplete and preliminary. Many effects, such as those of radiation free-field variations on the character of local shielding, have yet to be determined. The scientific community will require substantial proof of the accuracy of the analytical techniques before accepting results of such methods in lieu of the largely empirical T65D values. The programs to produce this information are just how beginning.

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Table 4.10 Calculated dose commitments to a maximum individual from Summer station operation, calculated air doses, and calculated selected release values

~~All doses to the individual are in millirems per year per site except as noted~~

Source / Design Objective	RM-50-2 ~~dose~~ design objective[a] values	Calculated ~~doses~~ values
Liquid effluents		
Dose to total body from all pathways, mrem/yr/site	5	0.05
Dose to any organ from all pathways,	5	0.06
Non-tritium releases, Ci/yr/unit	5 ~~Ci/yr/unit~~	C.26
Noble gas effluents (at site boundary)		
Gamma dose in air, millirads ~~per~~ years/site	10	0.23
Beta dose in air, millirads ~~per~~ years/site	20	0.57
Dose to total body of an individual,	5	0.14
Dose to skin of an individual,	15	0.42
Radioiodine and particulates[b] mrem/yr/site		
Dose to any organ from all pathways,	15	0.75
I-131 releases, Ci/yr/unit	1 ~~Ci/yr/unit~~	0.07

[a] Guides on design objectives proposed by the NRC staff on Feb. 20, 1974 consider doses to individuals from all units on site. From U.S. Atomic Energy Commission, "Concluding Statement of Position of the Regulatory Staff," Docket No. RM-2, Washington, D.C., Feb. 20, 1974, pp. 25-30 published as Annex to Appendix I to 10 CFR Part 50.

[b] Carbon-14 and tritium were added to this category.

Table 4.11 Annual total-body population dose commitments in the year 2000

Category	U.S. population dose commitment (person-rem per year)
Natural background radiation[a]	27,000,000
Summer station operation	
Plant workers	1300[b]
General public	
Radioiodine and particulates	27
Liquid effluents	1.1
Noble gas effluents	0.8
Transportation of fuel and waste	7

[a] Calculated using the average U.S. background dose (102 millirems per year) in U.S. Environmental Protection Agency, Natural Radiation Exposure in the United States, Report ORP-SID-72-1, June 1972, and year 2000 projected U.S. population from the U.S. Dept. of Commerce, Bureau of the Census, Population Estimates and Projections, Series II, Series P-25, No. 541, February 1975.

[b] The average reactor annual dose is 410 person-rem.[68,69] Particular plants have experienced average lifetime annual doses as high as 1300 person-rem. For purposes of conservatism the staff has used the higher value in this assessment.

4.5.2.2 Dose commitments from radioactive liquid releases to the hydrosphere

Radioactive effluents released to the hydrosphere from the Summer station during normal operation will result in small radiation doses to individuals and populations. The NRC staff estimates of the expected liquid releases listed in Table 4.12 and the site hydrological considerations discussed in Sect. 2.3 of this Statement and summarized in Table 4.13 were used to estimate radiation dose commitments to individuals and populations. The results of the calculations are discussed below.

Radiation dose commitments to individuals

The estimated dose commitments to the maximum individual from liquid releases at selected offsite locations are listed in Tables 4.8, 4.9, and 4.10. The maximum individual is assumed to consume well above average quantities of the foods considered and spend more time at the shoreline than the average person (see Table E-5 in Regulatory Guide 1.109).

Radiation dose commitments to populations

Annual radiation dose commitment from liquid radioactive releases from the Summer nuclear station are estimated for two populations in the year 2000: (1) the population within 80 km (50 miles) of the station (Table 4.9) and (2) the entire U.S. population (Table 4.11). Dose commitments beyond 80 km (50 miles) are based on the assumptions discussed in Appendix B. For perspective, annual background radiation doses are given for the population within 80 km (50 miles) of the site (Table 4.9) and for the entire U.S population (Table 4.11). The total body population dose to the population within 80 km (50 miles) of the site from liquid radioactive releases from the Summer Nuclear Station (i.e., about 1 person-rem) is a small fraction (less than 0.001 percent) of the corresponding population dose from natural background radiation (i.e., about 105,000 person-rem). The total body population dose to the entire U.S. population from liquid radioactive releases from the Summer nuclear station (i.e., about 1.1 person-rem) is an even smaller fraction (less than 0.00001 percent) of the corresponding U.S. population dose from natural background radiation (i.e., about 27 million person-rem).

Table 4.12 Calculated releases of radioactive materials in liquid effluents from the Summer station

Nuclide	Ci/year	Nuclide	Ci/year
Corrosion and Acivation Products		Te-129m	~~.8E-5~~ 9E-5
		Te-129	~~6.5~~ 6E-5
Cr-51	1.1E-4[a]	I-130	1.9E-4
Mn-54	1E-3	Te-131m	5E-5
Fe-55	1.1E-4	I-131	1E-1
Co-58	5E-3	I-132	3.8E-3
Fe-59	6E-5	Te-132	9.4E-4
Co-60	8.8E-3	I-133	5.7E-2
Zr-95	1.4E-3	I-134	1E-5
Nb-95	2E-3	Cs-134	2.1E-2
Np-239	4E-5	I-135	8.3E-3
		Cs-136	2.7E-3
Fission products		Cs-137	3E-2
Br-83	4E-5	Ba-137m	5.7E-3
Rb-86	2E-5	Ba-140	1E-5
Sr-89	2E-5	La-140	1E-5
Mo-99	2.8E-3	Ce-144	5.2E-3
Tc-99m	3E-3	All others	4E-5
		Total except Tritium	0.26
Ru-103	1.4E-4		
Ru-106	2.4E-3		
Ag-110m	4.4E-4	Tritium	360
Te-127m	2E-5		
Te-127	2E-5		

[a]Read as 1.1 x 10^{-4}.

UNITED STATES OF AMERICA
NUCLEAR REGULATORY COMMISSION

BEFORE THE ATOMIC SAFETY AND LICENSING BOARD

In the Matter	)	
	)	
SOUTH CAROLINA ELECTRIC & GAS COMPANY	)	Docket No. 50-395
	)	
	)	
Virgil C. Summer Nuclear Station, Unit 1	)	

CERTIFICATE OF SERVICE

I hereby certify that corrected copies of REBUTTAL TESTIMONY OF EDWARD F. BRANAGAN, JR. REGARDING CONTENTION A10 in the above-captioned proceeding have been served on the following (indicated by a +) by deposit in the United States mail, first class or, as indicated by an asterisk, through deposit in the Nuclear Regulatory Commission's internal mail system, this 24th day of July, 1981. Copies were served on the Board and parties during the hearing on July 2, 1981.

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