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NUCLEAR RADIATION: HOW DANGEROUS IS IT?

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FOREWORD

Robert R. White
Director, Academy Forum

Since its inception in 1972 the Academy Forum has given attention to the larger problems of energy as well as to some of the components of its possible solutions. Following the oil embargo of 1973 several hundred citizens and experts gathered to appraise the future alternatives and risks of the energy situation at that time. This Forum brought out insights that "We will forget at our peril," its cochairman summarized.

In the spring of 1977 the Forum convened two days of workshops and plenary sessions to study the most recent data on the use of coal. The audience, drawn from across the United States, struggled with the conflicts of national needs versus regional interests in its attempt to find a consensus.

This publication is the report of the first Forum in the 1979-1980 series on nuclear activities. Held at the National Academy of Sciences on September 27, 1979, it was moderated by Daniel Koshland, Professor of Biochemistry at the University of California, Berkeley, and Chairman of the Forum Advisory Committee, who resolutely confined the discussion to the effects of nuclear radiation. Other Forums in this series take up nuclear waste (November 19, 1979), the safety of nuclear reactors (Spring 1980), and practical alternatives to nuclear energy and oil (early Summer 1980).

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INTRODUCTION

Daniel E. Koshland, Jr.

Professor of Biochemistry
University of California, Berkeley

I would like to welcome you to this Academy Forum on nuclear radiation. As your Moderator, I will introduce the Panel in a moment.

The National Academy of Sciences, by charter, has the role of advising the Government. But as science intrudes more and more in our daily lives, this advice is not only at the scientific level but also increasingly on issues in which societal values and scientific precision interact.

Scientists in general have a utopian idea that the Government should come to us with a subject and its related issues, such as nuclear radiation, and say: "We have an infinite amount of money; we have a reasonable length of time, something like 20 years; go to your laboratories, and get the data, and then come back and give us the answer."

In the real world scientists usually hear: "We have $7.56 left over from an overrun on the bombers; we have until next Tuesday to review the data; we'd like your opinion." That is slightly exaggerated, but it is true that most decisions, such as those surrounding the subject of this Forum, have to be made before all the scientific data are accumulated.

Yet, some information usually is available. And in that kind of interface, the following situation frequently develops: the scientists, wanting to be accurate and precise and not go beyond the data that have accumulated, appear to be mixed up and arguing among themselves; the public policy makers become impatient at getting a firm decision. At this point two types of errors can occur: consensus appears to develop when it really is not so, and the public gets misled by scientists' reassurances when there is serious doubt; or, in other cases, controversies are exaggerated.

So the Academy Forum was designed not to give a final decision, but rather to clarify at this interface what the state of the art is, what facts have a consensus, and what other facts have a disagreement in order to allow the policy makers to make good decisions. What we have developed is a format in which we specifically take an individual subject and try to bring experts who are outstanding in their fields from all parts of the world to participate in the discussion.

I now would like to introduce the Panel, and I will start with the three scientists who are direct experts in radiation and are doing research in that area.

Dr. Charles Land has his Ph.D. from the University of Chicago, taught at the University of Oregon, served on the Atomic Bomb Casualty Commission in Hiroshima and is now on the staff of the Environmental Epidemiology Branch of the National Cancer Institute.

Dr. Edward Webster was born in England, has a Ph.D. from the University of London, is Chief of Radiological Sciences at the

Massachusetts General Hospital, Professor of Radiology at the Harvard Medical School, and is a member of the National Council on Radiation Protection.

Dr. Karl Morgan received his Ph.D. at Duke in physics, and while at the University of Chicago was one of six scientists who essentially were the fathers of modern health physics. It was the beginning of health physics, the understanding of the interface of physics and biology, which led to much of our knowledge of radiation effects. Dr. Morgan was Director of the Health Physics Division at Oak Ridge for many years and then moved to the Georgia Institute of Technology.

Dr. Arthur Upton, who holds the M.D. from the University of Michigan, was Chairman of Pathology at the State University of New York for a number of years, and is now Director of the National Cancer Institute. In that role Dr. Upton is very much interested in research on radiation, but he also has concerns with carcinogens in general. He and Dr. Vagelos, whom I will also introduce, are here as sort of outside experts to help evaluate the general problems that we will be discussing.

Dr. Vagelos got his degree at Columbia University, was for a while Chairman of the distinguished Department of Biochemistry at Washington University, and is now President of Merck Sharp & Dohme Research Laboratories, where he is concerned with drugs in general and radiation only peripherally.

Burns Roper, Chairman of the Board of the Roper Organization, attended Yale University. His research and polling techniques have developed the understanding of public opinion into a science. In addition to affiliations with numerous professional organizations related to his expertise, Mr. Roper is a member of the Boards of Freedom House and the Environmental Fund, a member of the Corporation of UNICEF and of the National Institute of Social Sciences.

Before going further, I would like to introduce a few people in the audience. I think you will see on the program that I am really speaking for a larger organization, the General Advisory Committee of the Academy Forum of the National Academy of Sciences. Some of the members of that Committee are here in the role of monitors. They are not required to speak, but if they think we are too biased in one direction or too far off in another direction, they have the obligation and privilege to bring us to task.

I would like to introduce Dr. Frederick Robbins, the distinguished authority on viruses, a Nobel Laureate, and Dean of the School of Medicine at Case Western Reserve University; Dr. David Baltimore, who is with the Center for Cancer Research at the Massachusetts Institute of Technology, also a Nobel Laureate, and an expert on viruses; Dr. Alvin Weinberg, former Director of the Oak Ridge National Laboratory and now Director of the Oak Ridge Institute for Energy Analysis; and Dr. Philip Handler, President of the National Academy of Sciences and a distinguished biochemist.

Now, I'd like to tell you briefly with this panoply of experts how we plan to proceed. In the first place, we have all agreed that we do not wish to get bogged down in scientific jargon. We hope to present some data in a way that will be intelligible to the layman as well as the scientist. And I'm making a caveat: the scientists on this program will use the terms roetgens, rems, and rads as though they were interchangeable, even though it will offend some purists in science. These are units of radiation. We will try to use the rem insofar as possible. To give you a little feeling as laymen of what it means, a tenth of a rem is the figure that all of us get in terms of radiation in a year, just walking around in our normal lives and living in a suburb far from a nuclear plant. Thirty percent of that is the natural radioactivity within our bodies, potassium-40 largely; the rest of it is due to radiations from the atmosphere and the soil and so forth. Five hundred rems is the figure that is generally accepted as the dosage that will cause 50 percent of the exposed population to die. For that reason it is called the LD-50 or average LD-50 dose.

We also have decided that the best way to discuss this subject is to deal with three specific questions:

The first of these will deal with the scientific matter; namely, how dangerous is the radiation from a strictly scientific point of view of prediction of cancer, genetic defects and so forth?

The second question deals with how do you design a rational risk policy in regard to nuclear radiation -- something that is acceptable to our consciences as well as to society.

The third question deals with what is the public perception of the risk in this area of nuclear radiation and how should it affect our policy decisions?

My plan for the evening is to bring up each of these questions in order, to have the Panel discuss their answers to the question, give them a chance to interrogate each other, then to turn to the audience if you have any questions. I hope that we will have time at the end to summarize and to ask for any areas that either the audience or the Panel feel have been left out. I feel strongly about this. As a professor at Berkeley who is subject to these anonymous evaluations, I recently had a student who wrote: "This was an extremely well-rounded course; everything not included in the lectures was included in the final examination." So I hope that those of you who feel that we aren't covering everything in the questions will wait for the round-trip session.

With that as an introduction, I would like to read the first question which we would like to center our attention on and it is as follows: What are the health issues in receiving low, chronic doses of ionizing radiation? What are the risks from a sudden short burst of quite high levels, such as might occur in the neighborhood of a nuclear reactor? I'd like to ask Dr. Webster to start.

EDWARD W. WEBSTER
Chief, Radiological Sciences
Massachusetts General Hospital

The issues regarding health effects concern what those effects are and their likelihood. These effects have been established gradually over a period of about 80 years, since the discovery of radiation in the 1890's. And since that time, we've learned a great deal about radiation.

At the start, I want to distinguish between two different kinds of radiation which are involved in a discussion of radiation hazard.

First of all, we have a very weakly ionizing kind of radiation; by that I mean that these radiations deposit rather a small amount of energy in the sensitive parts of our body cells as they pass through. Included in those kinds of radiation we have x-rays, gamma rays and beta rays. Then there's another kind which we believe to be more damaging: the heavily ionizing radiations, which deposit relatively large amounts of energy in the cells through which they pass; these are the neutrons that are released by atomic bombs and the alpha particles that are released by some radioactive materials which are significant in the nuclear power business, plutonium and radon gas.

The effects that I am going to discuss are three. The first and most important is cancer that develops in human populations long after their exposure to radiation. The second effect is less important, or at least is considered so by many people, numerically speaking: these are the genetic effects that do not appear in the people who are exposed but only in their offspring and maybe several generations of their offspring. So this is a hazard for the future. And thirdly, I'm going to talk about the effects on the unborn child, on the fetus.

It is important to recognize that for the very low doses of radiation which are involved in the environment, like background for example, or maybe a few times background, we have really no direct observations of human effects. So we don't really know if there's any effect; all we can do is to consider what happens at high doses and then project from what happens at high doses to what we believe would happen at these low doses. And there are several ways of doing this.

The simplest way, and this has been quite commonly done by scientific people, is to assume that the effects -- let's talk about cancer effects, for example -- are proportional to the dose of radiation. That is, if you have 1 rem instead of 50 rems, then the effect is 50 times less. And for the weakly ionizing radiation, that is, x-rays and gamma rays, there is some evidence of this but it's not strong evidence. There is, in fact, considerable controversy about whether you can use this proportional projection.

On the other hand, there is much less controversy about the highly ionizing radiations, the neutrons and the alpha particles, and I think most people would subscribe to the idea of a linear, proportional relationship.

An example of the proportional approach, which we call linearity, is shown in my Figure 1.

You see a straight dotted line on that graph, and that represents the amount of thyroid cancer that we have observed in people who have been exposed to rather large doses of radiation. Notice that the bottom line indicates thyroid dose in rads -- (or rems) -- and the highest dose in these cancer cases is between 600 and 700 rems. This would be a localized dose received by the thyroid gland. And you notice that the data points, these are the black dots, roughly form an ascending straight line. However, you will also notice from the numbers in the parentheses that there aren't many cases of cancer represented by each dot, and so there is considerable uncertainty about the position of those dots.

This particular set of data is a relatively good set of data for human populations and is based on 2,000 children in whom the upper chest and neck were irradiated by x-rays, and in those 2,000 children after about 25 years of follow-up study when the cancer started to appear, there were only 20 cancers; 20 cancers in 2,000 children. So it's not a very large effect, even though the doses were high.

Another way of projecting high-dose data would be to assume that the effect is less than linear, and I will give you an example of that, too, from a different kind of scientific study.

In Figure 2 the important part of this particular graph is down on the bottom left-hand side. This is the result of an experiment in which many mice, as the test species, were irradiated by gamma rays. The reasons why people do animal experiments are, first, that they are controllable; and, secondly, you can involve a lot of animals in the experiment and document the effect of the radiation very much more precisely. In Figure 2 there are little bars going up and down from the data points, and the length of those bars represents the uncertainty of the position of those points. Clearly, we have a high degree of certainty in this experiment, which is one of the advantages to doing a mouse or a rat experiment.

You will notice that at the second point on the left, which is at 10 rems or 10 rads, there was no increase in cancer observed; in fact, it would be very hard to draw a straight line through those points, especially through the lower four points. This is an example, then, of a less than linear relationship to radiation dose. This, by the way, is an experiment with gamma rays, not with neutrons.

Figure 3 shows an example of the same thing; that is, a nonlinear relation, this time in a human population. The figure concerns the observations of leukemia that developed in the survivors of the two atomic bombs in Japan. Notice that there are two relationships to dose. The upper one, labeled "Hiroshima," is fairly straight, and I think you could say that the effect there in producing leukemia was linear with dose. The bottom line, however, particularly at the lowest doses, appears to be less than linear (concave upward); there were few cases of leukemia developing at doses below about 100 rads.

Now, there's a reason, we believe, for this difference. In Hiroshima there was a relatively large amount of the highly ionizing radiation; specifically, about a quarter of the radiation dose was produced by neutrons. But in Nagasaki there were practically no neutrons, and the exposure was almost entirely from gamma rays. Since the certainty of the data is not very good, as shown by the size of the error bars, you could still regard that lower line as consistent with linearity, but there is

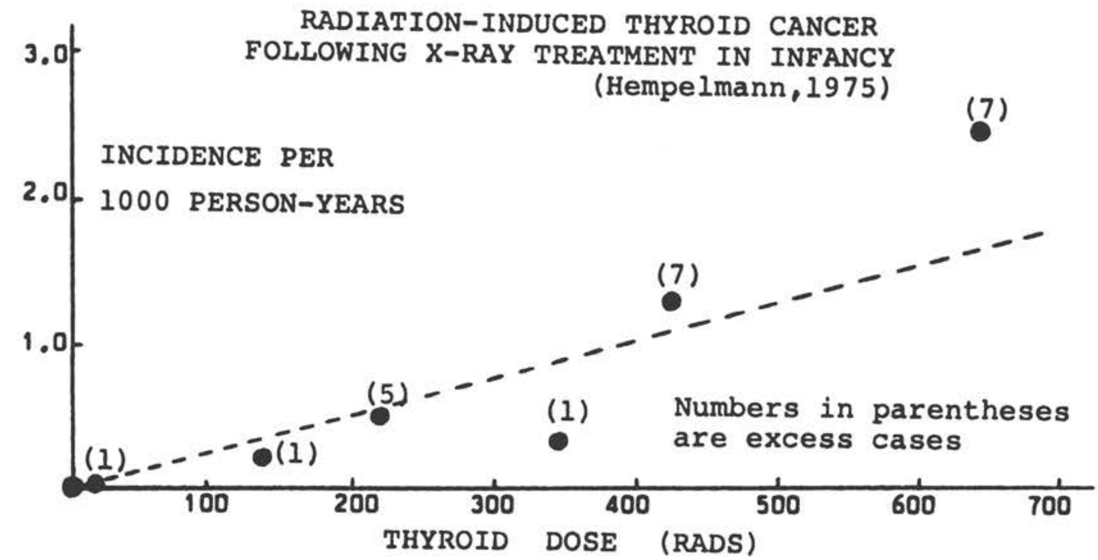


FIGURE 1

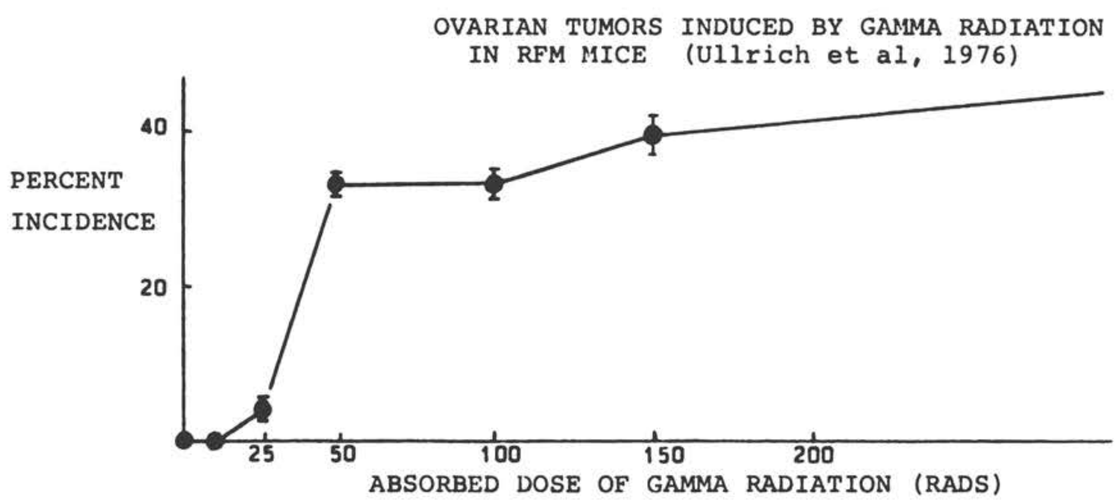


FIGURE 2

clearly a strong probability that the effect is nonlinear -- less than linear.

What do we conclude from this kind of data? I think it would be a very conservative position to say let's assume that the risk of cancer is linear with dose, because that gives us a maximum estimate of the low-dose effect, even though it may not be linear for x-rays and gamma rays.

On the basis of linearity, let's consider a million people who would receive 1 rem of radiation; that's 10 times the background radiation received in one year. What would we expect in terms of extra cancer deaths in those million people? And I think a reasonable answer to that, in our present state of knowledge, is about 100. That's a nice, easy round number: 100 fatal cases of cancer in those million people for 1 rem. There would, therefore, be 1 extra death in every 10,000 people so exposed.

That figure should be compared to the normal amount of cancer you would expect to find in that population. And in that same 10,000 people where there is 1 radiation-induced cancer death, we would expect to find 1,700 normal cancer deaths, unconnected with radiation. So we're comparing the 1 radiation death with the 1,700 normal deaths. Generally, this radiation-induced cancer would occur about 20 years after the exposure to radiation.

Let me illustrate this particular statistic with a group of radiation workers. Let's say there are a million people working with radiation in the country, and their present average dose is about a half a rem per year. Let's suppose they do this job for 10 years; at a half a rem a year, that's 5 rems of dose. In that million people we would expect, therefore, from the 5 rems, 500 cancer deaths to occur based on linearity, in comparison with the 170,000 cancer deaths normally occurring. In other words, about one cancer death out of every 300 would be radiation connected, and this risk would be 2 to 10 times lower if we were to assume that the relationship was nonlinear. I gave you some examples of that possibility.

Now let's turn to unborn children, children who are not yet delivered at the time of the radiation. Here again there are conflicting observations. However, it is possible the fetus may be about 5 times more sensitive to radiation than adults. So if unborn children in 10,000 pregnant women each receive 1 rem of radiation, instead of 1 cancer death in 10,000 persons, there might be 5 cancer deaths in the 10,000 children before the age of 10.

There are two other effects which we must deal with; these are the genetic and the developmental defect problems in children; children who are born with some deformity following radiation.

The genetic effects are produced by the irradiation of either the father or the mother, and show up in their children. Estimates of the likelihood of such effects have not changed recently; the geneticists are fairly well agreed in their estimates. In 1 million children whose parents have received 1 rem of radiation, 10 times the annual background, it is predicted that somewhere between 5 and 75 of those 1 million children -- 5 to 75 is the range of uncertainty -- will show a serious

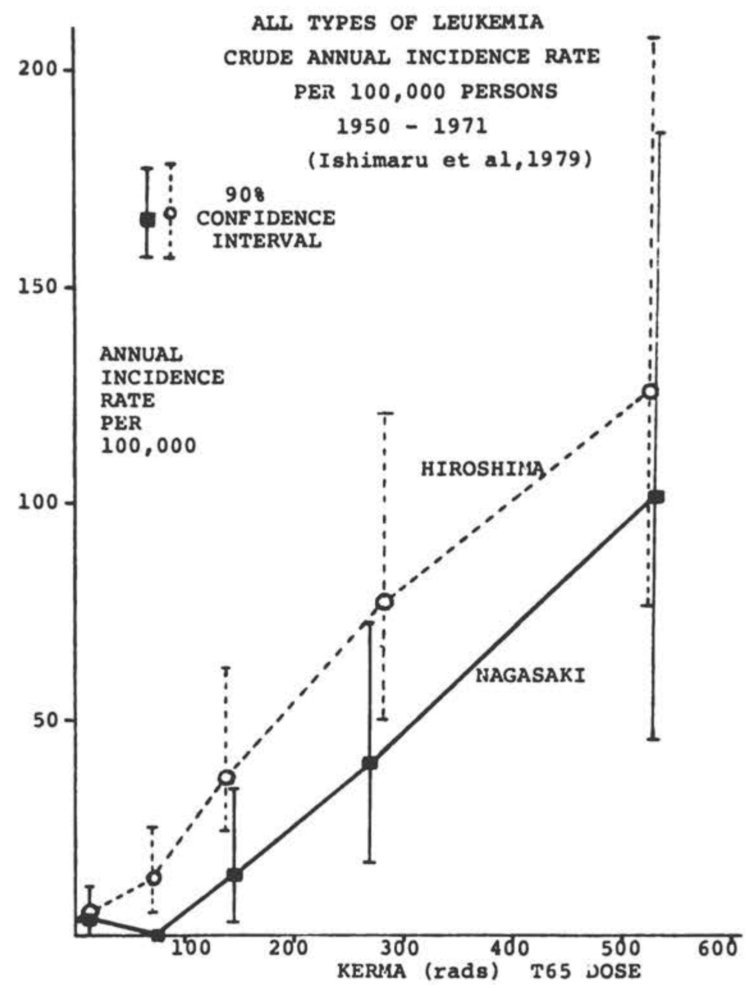

ALL TYPES OF LEUKEMIA
CRUDE ANNUAL INCIDENCE RATE
PER 100,000 PERSONS
1950 - 1971
(Ishimaru et al,1979)
90%
CONFIDENCE
INTERVAL
ANNUAL
INCIDENCE
RATE
PER
100,000
HIROSHIMA
NAGASAKI
200
150
100
50
100
200
300
400
500
600
KERMA (rads) T65 DOSE

FIGURE 3

genetic defect, such as dwarfism or mental defectiveness, in the child.

This should be compared with the known fact that of all the children born today, about 10 percent show some genetic defect. That is, out of the 1 million children, about 100,000 would show some genetic defect. That number is growing as medicine begins to recognize more genetic-type disease in the population. So we're comparing 5 to 75 (say 25 as a middle estimate) from the 1 rem of radiation with the 100,000 genetic defects evident in an unexposed population.

Finally, let me mention malformations or birth defects which can develop in children who are irradiated while being carried by their mothers. These risks are about the same order of magnitude as the ones I've been discussing, but it is believed that there is a threshold dose for these effects for x-rays and gamma rays. The threshold for the more significant effects, such as skeletal deformities, may be 20 rem or more. It is not believed that there is a threshold for the densely ionizing radiation, the alphas and the neutrons.

One good example of such a defect is a child born with diminished-sized, small-sized heads, which is usually accompanied by mental retardation. It is significant that following the dropping of the bomb in Nagasaki, where the radiation was from gamma rays, none of these cases occurred up to about 150 rems. So from this evidence it looks as if the x-and gamma rays carry a relatively low and perhaps zero risk at low doses. However, the neutron dose to the people in Hiroshima is associated with a much higher level of risk, down to rather low levels of neutron radiation such as a few rem. A good estimate is that about 1 case of microcephaly, as we call it, small head with mental retardation, would occur in 250 pregnancies where the mother received 1 rem; 1 case in 250. That's with the neutrons, not with the x-rays.

Now let's talk about a large, single burst of radiation. Since I've assumed linearity, we wouldn't expect the fact that the radiation was delivered promptly over a short time period to have much effect on the level of risk from the gamma radiation, which is the usual problem in such a release. So we can use the same risk estimate per rem.

I'd like to give you a hypothetical situation. Suppose there is an escape of a massive amount of radioactivity from a nuclear reactor, due to a partial meltdown. Specifically, there's a breach of containment, the radioactivity becomes free in the environment, and is deposited uniformly over a 40 square mile sector of the landscape. I'm going to assume a large release of radioactivity; the amount for those who understand the units of radioactivity is 100 megacuries. That's a very large amount, far greater than the release at Three Mile Island, larger than we would use in any industrial establishment, but it is only about 1 percent of all the radioactivity which can build up in a large reactor. And I'm going to assume that within that 40 square mile sector there are 10,000 people.

You could estimate that the mean whole body dose, the average dose, to the people in that environment would be about 200 rems in one day from penetrating radiation. However, in addition there would be several thousand rems of dose to the skin from the nonpenetrating beta rays. And I'm going to assume that these 10,000 people are evacuated in one day.

That's the scenario.

The eventual cancer mortality, the cancer death risk, resulting from the gamma rays would be about 1 case per 10,000 people for each rem; that is, 200 for a 200 rem dose; that's 200 cases of fatal cancer eventually developing in those 10,000 people. In addition, there would be lethal effects on the people who were very close, depending on the local distribution of the radioactivity; people who were so close that they received more than 500 rems would die within 30 days. In addition to the gamma ray effects there would be widespread severe skin burns from the nonpenetrating beta rays. Persons spending 24 hours in the open with a radioactivity level of about 12 curie per square meter could receive skin doses of several thousand rems. So as a ballpark number for this scenario, there would be about 200 cases of late cancer out of the 10,000 people, plus perhaps a score of radiation deaths in 30 days, plus perhaps a thousand cases of serious skin burns on exposed areas of the body.

KOSHLAND: Two hundred and -- could you refresh us again on the figure of how many cancers you expect naturally from 10,000 people?

WEBSTER: Out of 10,000 people, about 17 percent would normally die with cancer, so that's 1,700 people who would normally die with cancer. Some greater number, of course, would get cancer because cancer is partially curable. We must differentiate between getting cancer and dying with cancer; there may be a factor of 2 involved there.

KARL Z. MORGAN
Neely Professor, School of Nuclear Engineering
Georgia Institute of Technology

For this brief discussion, I am going to oversimplify the dose-effect relationship of ionizing radiation by making use of a very simple logarithmic relation as shown in Figure 1; that is, the effect is proportional to some power n of the dose or Effect = $C(\text{Dose})^n$. This would be the relationship for humans exposed to doses less than, say, a few hundred rems.

It follows from this that if n is equal to 1, we have the linear relationship; if it is greater than 1, say, 2, we have a relationship within the limits of error which we might say corresponds to the threshold hypothesis; and then there is the possibility that we go in the other direction and n would be less than 1. There is quite a bit of data suggesting that in some cases, n is equal to 1/2.

I will discuss only radiation-induced malignancies, but as indicated in Figure 2, some of these same arguments can be applied to genetic damage. It is noted that the early work of Russell[1] at Oak Ridge suggested that the genetic damage to mice per roentgen at low dose rates and low doses is only 10 percent of that at high dose and high dose rates. However, there are recent data from Lyon, et al.[2], as indicated by the curve in Figure 2 (marked with deltas), that suggest at very low

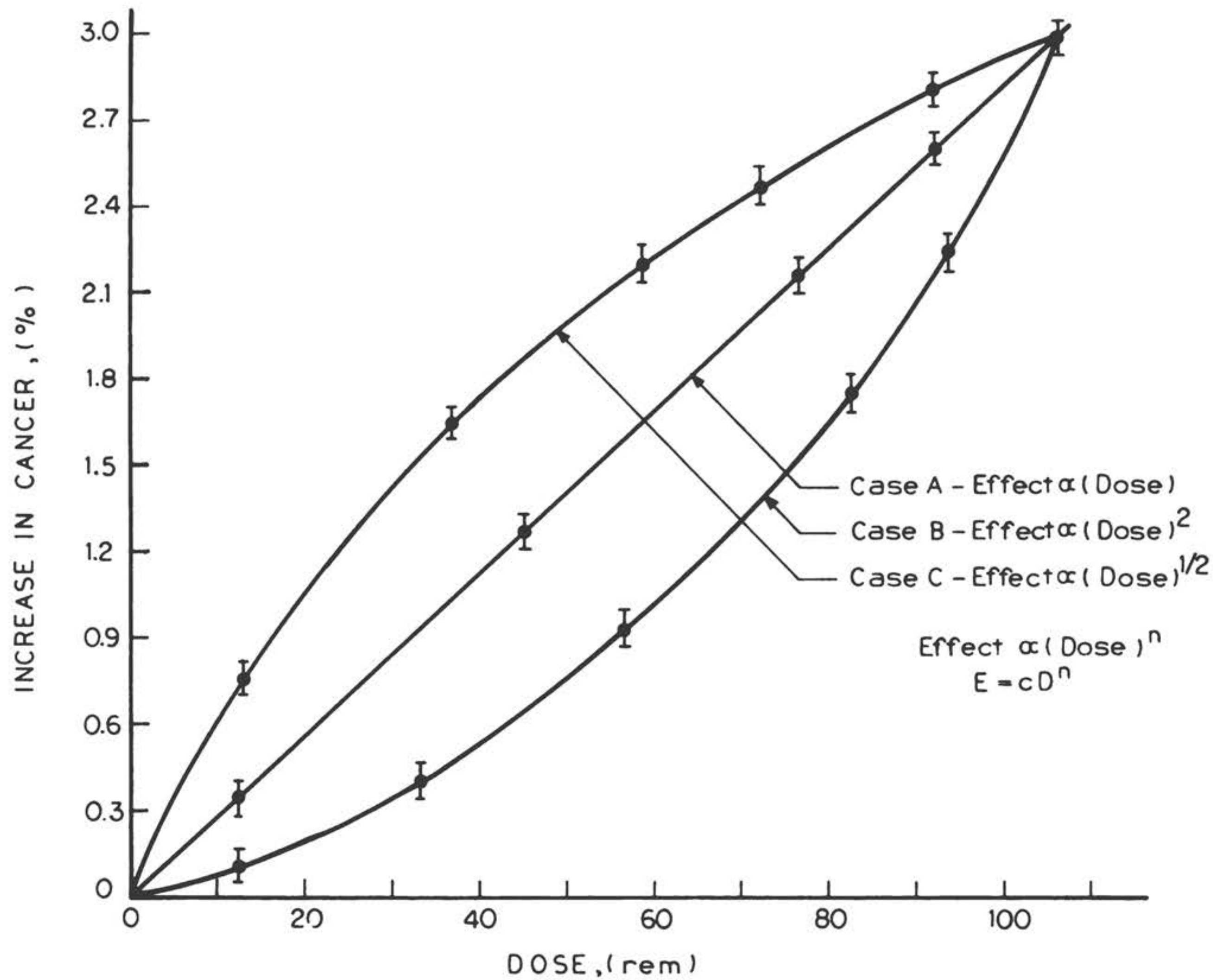
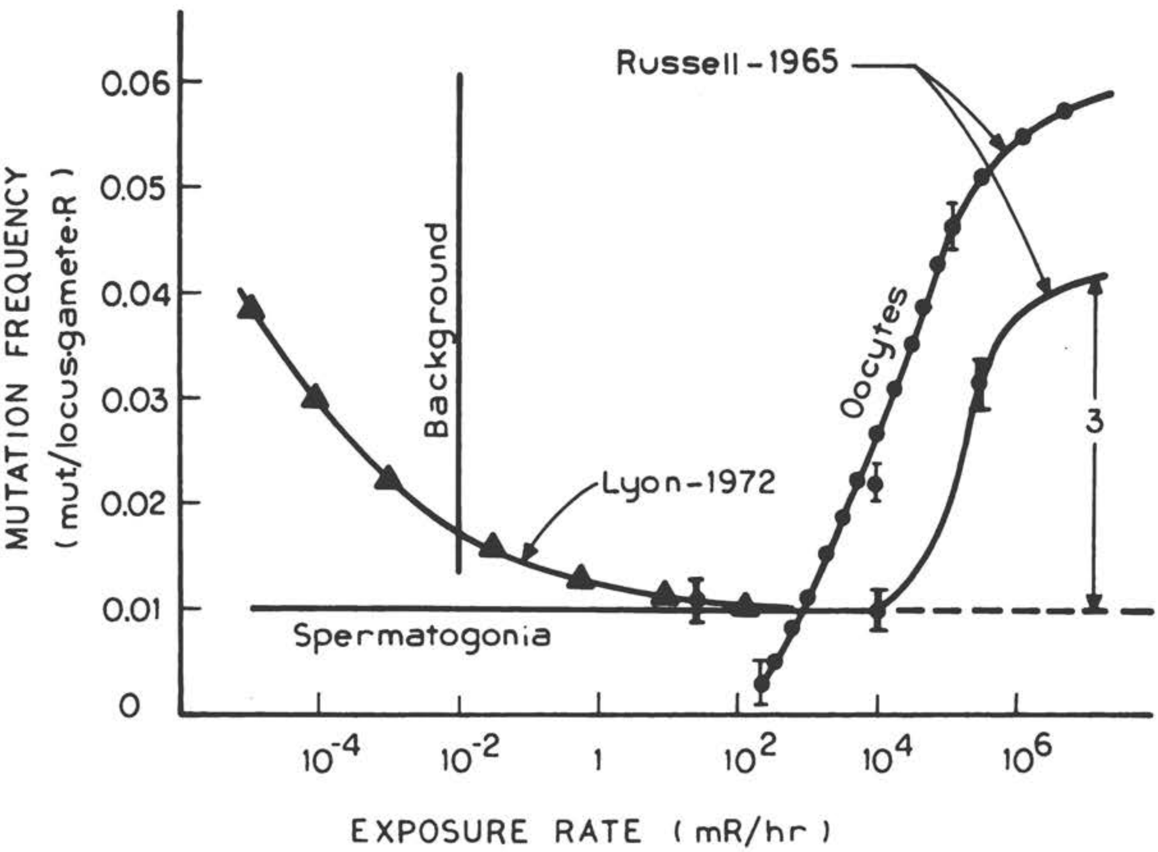


FIGURE 1 Various Models Showing the Increase in Cancer Risk at Low Levels of Exposure to Ionizing Radiation



<u>CORRECTIONS</u>

For dose rate : $1/2 \times 1/3 = 1/6$

For dose : $1/2$

Total : $1/6 \times 1/2 \simeq 1/10$

FIGURE 2 Variation of Mutation Frequency with Dose Rate doses and dose rates, when you get down to the neighborhood of background radiation, the mutation curve might actually be going up again at very low dose rates.

So, it is my feeling that we should be very cautious in making use of this factor of 10 percent which we apply in reducing the estimate of genetic risk at very low doses and dose rates.

Studies of cancer risk at low doses have failed to give evidence of a safe threshold dose, but rather have supported a nonthreshold dose-effect relationship. Also, a number of human studies have suggested the risk of cancer from low exposure is much greater than had been considered some years earlier.

During this period of the past decade, national and international standard-setting bodies have discarded the threshold hypothesis in favor of the linear hypothesis, i.e., they are using $n=1$ instead of $n>1$. However, there are some in these organizations that are skeptical and feel that this provides a very generous factor of safety that may be unnecessarily overconservative.

A few of the reasons for the divergence of opinion of scientists and why the linear hypothesis often underestimates rather than overestimates the cancer risk are summarized in Figure 3 and will be discussed briefly. First of all, we have the matter of overkill. At high doses the cancer incidence curve reaches a maximum and drops over in parabolic fashion, as can be seen here, because the persons are so damaged with radiation exposure they do not survive long enough to die of cancer.

In other words, in such an analysis one forgets that what is taking place at the higher doses and dose rates begins to have an appreciable effect at the intermediate doses, so we should make correction for overkill over the entire experiment curve and represent the risk by line B instead of line A.

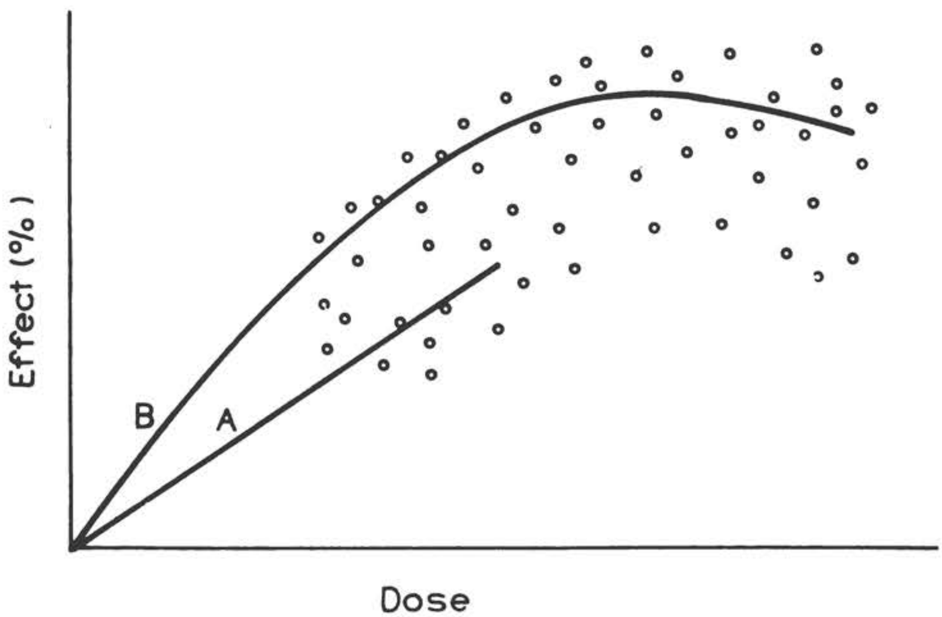
Then we have the short follow-up of both animal and human studies. I think we would all agree that this can only underestimate the risk, because if you were to follow a given population for a longer period of time and get more malignancies of various types, then your risk coefficient could only increase.

Regarding animal versus human studies, I will, in the interest of time, mention only one experiment to emphasize its importance. Warren and Gates[3], some years ago, found that when they exposed mice to a given dose of x-rays, they got, in one case, a large number of leukemias and considerable life shortening; but when they gave the same dose to another group of mice of a different strain, they got essentially no effect at all. This kind of observation causes the health physicist to be rather skeptical when someone attempts to translate data on animal studies to man. In this case, the transition was only from one strain of mice to another; the differences from mouse to man can be expected to be much greater.

The short lifespan of animals is a serious handicap in these studies. Of course, it is usually necessary to use animals of relatively short lifespan, say, 5 to 20 years, but man is a 70-year animal, and we believe many of the observed effects are not related to the fraction of lifespan over which they are studied following radiation exposure, but rather to

Why the Linear Hypothesis Underestimates the Cancer Risk :

1-Overkill



2-Short Followup

3-Animal vs. Human Studies

4-Animals of Short Life Span

5-Cell Sterilization

6-Heterogeneity of Population

7-Damage to Immune Surveillance System

FIGURE 3 Factors Responsible for Some of the Controversy About Effects of Low Level Exposure to Ionizing Radiation the time interval since the exposure was received.

Many studies have been made on humans where the organ doses are so large that cell sterilization destroys preferentially precursors of cancer cells, or those weak cells which are most likely to develop into malignant cells later on, and kills small clones of cells that are already malignant but not yet recognized (cancers in situ). One of the classic examples I could refer to is the fact that some of our standards-setting bodies, such as NCRP, ICRP, and UNSCEAR, have in some cases used human data on iodine-131 exposure resulting in high doses of radiation and extrapolated these data down to relatively low doses to determine the risk of cancer from low level exposure to iodine-131. They seem to forget, and perhaps someone should remind these people, that a dead thyroid cell is not going to become the precursor of a thyroid carcinoma cell.

Heterogeneity of the human population is undoubtedly a major reason why in some cases the cancer risk to man per rem exposure is greater at low doses than at high doses or when n in the above equation is less than one. The studies showing an increase of statistical significance in the incidence of cancer of bone marrow, of the pancreas and the lung that is related to low radiation doses, i.e., the Hanford data[4], have been widely criticized by supporters of the nuclear industry. In some cases the study has been criticized because there are too many uncontrolled variables. We are told this population includes people who were both sick and well, some were on drugs; there were the fat and the slim, the black and white, the young and old; there were chemical hazards; there were genetic differences; there were smokers and nonsmokers, et cetera. I was Editor-in-Chief of the Health Physics Journal at the time this particular paper by Mancuso, Stewart and Kneale[4] was received and reviewed by some of our country's most competent reviewers before it was accepted and published. I was criticized for publishing a paper showing radiation-induced cancer in such a heterogeneous population. I can hardly imagine more ridiculous criticism. Without doubt this is the largest human population for whom we have accurate radiation exposure records over a long period of time. I can only interpret such criticisms as saying essentially that one should ignore these human data, and instead, base our standards on low-level exposure to inbred animals where all these variables can be carefully controlled. It is perhaps of interest to note that a number of competent scientists who were employed by government agencies to find out what is wrong with this study have been forced to agree with the finding of the study that there was an increase of statistical significance in the incidence of cancer of the pancreas and bone marrow which relates to the radiation dose.

I believe it is primarily the heterogeneity or the many subgroups in a population that causes a higher incidence of malignancies per rem at low doses than at high doses. Studies of Bross[5], for example, in the Tri-State Study seem to confirm the existence of subgroups in the population that are more susceptible to radiation-induced malignancies and are influenced by cocarcinogenic and synergistic factors. For example, he found a large increase in the cancers in his study group that were exposed to x-rays in utero if they later developed certain

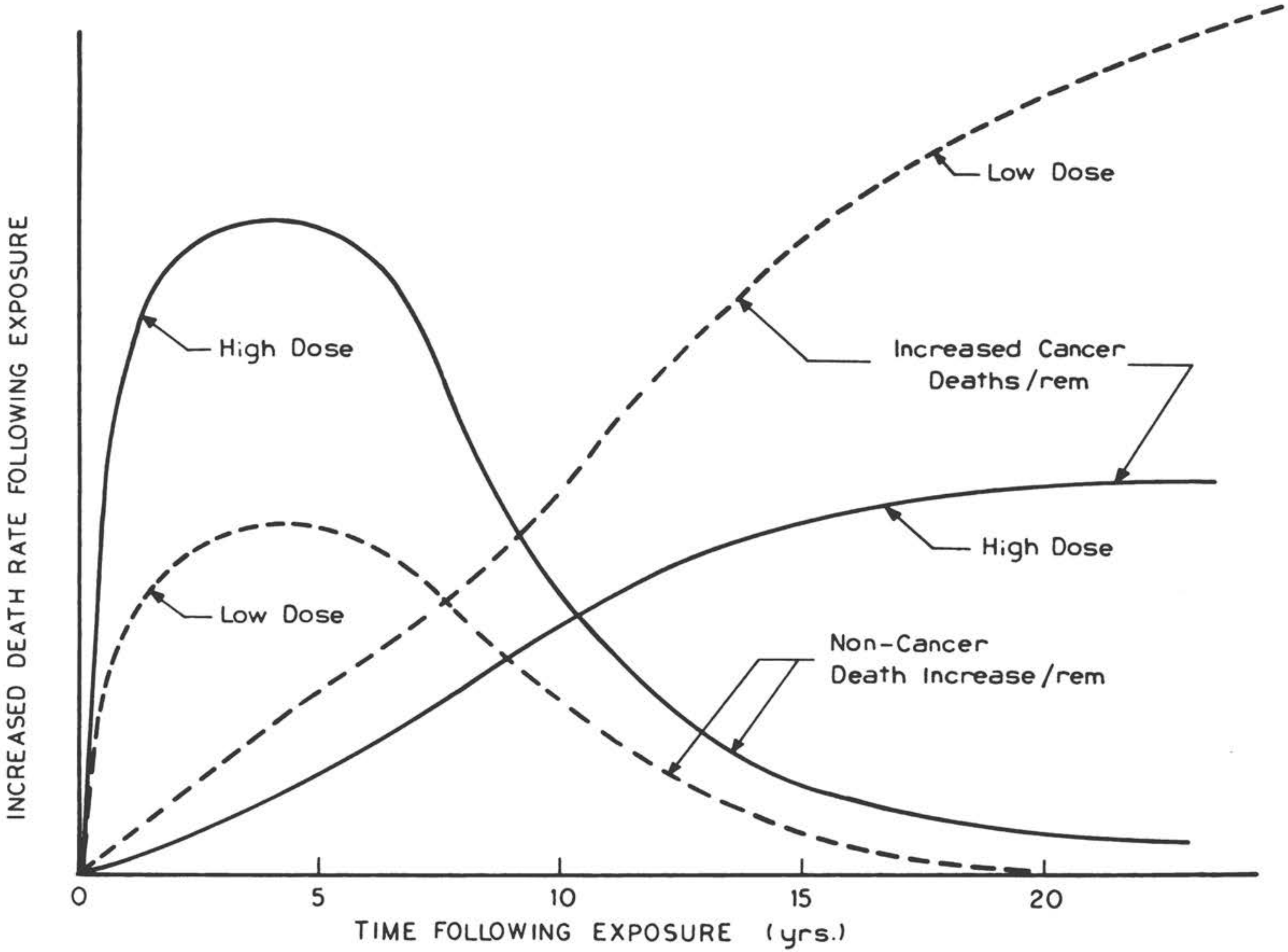


FIGURE 4 Early Non-Cancer Deaths and Late Cancer Deaths Due to Exposure Both to High Level and Low Level Exposure to Ionizing Radiation

respiratory diseases in early childhood.

Should we continue to base our standards primarily on the data from the survivors of the atomic bombing at Hiroshima and Nagasaki, or on the cancer incidence among the ankylosing spondylitis patients who were given large doses of ionizing radiation as part of their therapy treatment? I shall now point out what I think are some strong arguments why both of these studies, the studies which are the foundation stones of our present standards, tend to seriously underestimate the risk of radiation-induced malignancies. It is indeed odd that the standards-setting bodies and our government agencies seem to wear blinders when they accept, without question, these two studies as though they were the inspired word and gospel truth, when, in fact, they are fraught with serious unaccessed biases.

Damage to man's immune system or reticuloendothelial system by ionizing radiation is a very important factor and probably is very significant in determining the shapes of the curves shown in Figure 1. Normally, man's immune system holds in check all sources of foreign protein including small colonies of cancers before they become clinically recognized. However, radiation damages the ability of these scavenger cells (leucocytes and especially the lymphocytes) to recognize and remove viruses and bacteria as well as cancers in situ. Thus, at large doses, as shown in Figure 4, there is a large increase in noncancer deaths per rem, and a low increase in cancers per rem. For those exposed to low radiation doses there is a low increase in noncancer deaths per rem and a high increase in cancer per rem.

This, of course, is because of the short latent or incubation period of many of the common diseases such as pneumonia which develop fast when a large fraction of the immune surveillance cells have been damaged or destroyed by high radiation doses. Thus, the weak persons who were most likely targets for development of a malignancy were the members of the Japanese survivor population that were removed early and did not live long enough to develop a malignancy.

I consider it unfortunate that these data from Hiroshima and Nagasaki and the ankylosing spondylitis patients have been misused and given primary importance in setting radiation protection standards by the International Commission on Radiological Protection, the National Council on Radiation Protection, the UNSCEAR Committee and, to some extent, by the BEIR-I Committee. I have not seen the BEIR-II and BEIR-III reports as yet. I hope they, too, do not ignore completely the factors which I have just discussed.

The ABCC data on survivors of atomic bombings at Hiroshima and Nagasaki identified the radiation-induced cancers (as given by A in Figure 5) and this is the difference in the cancers among the blast and fire victims and the low-exposure group as controls. Ideally, they should have identified C in this figure; that is, the difference in cancers per rem of the blast and fire victims with exposure as against those without exposure, and this was not done. Then, if possible, an effort should have been made to correct for the bias due to blast, fire, disease, loss of loved ones, etc.

Kneale and Stewart[6] have shown that a year or more before the cancers

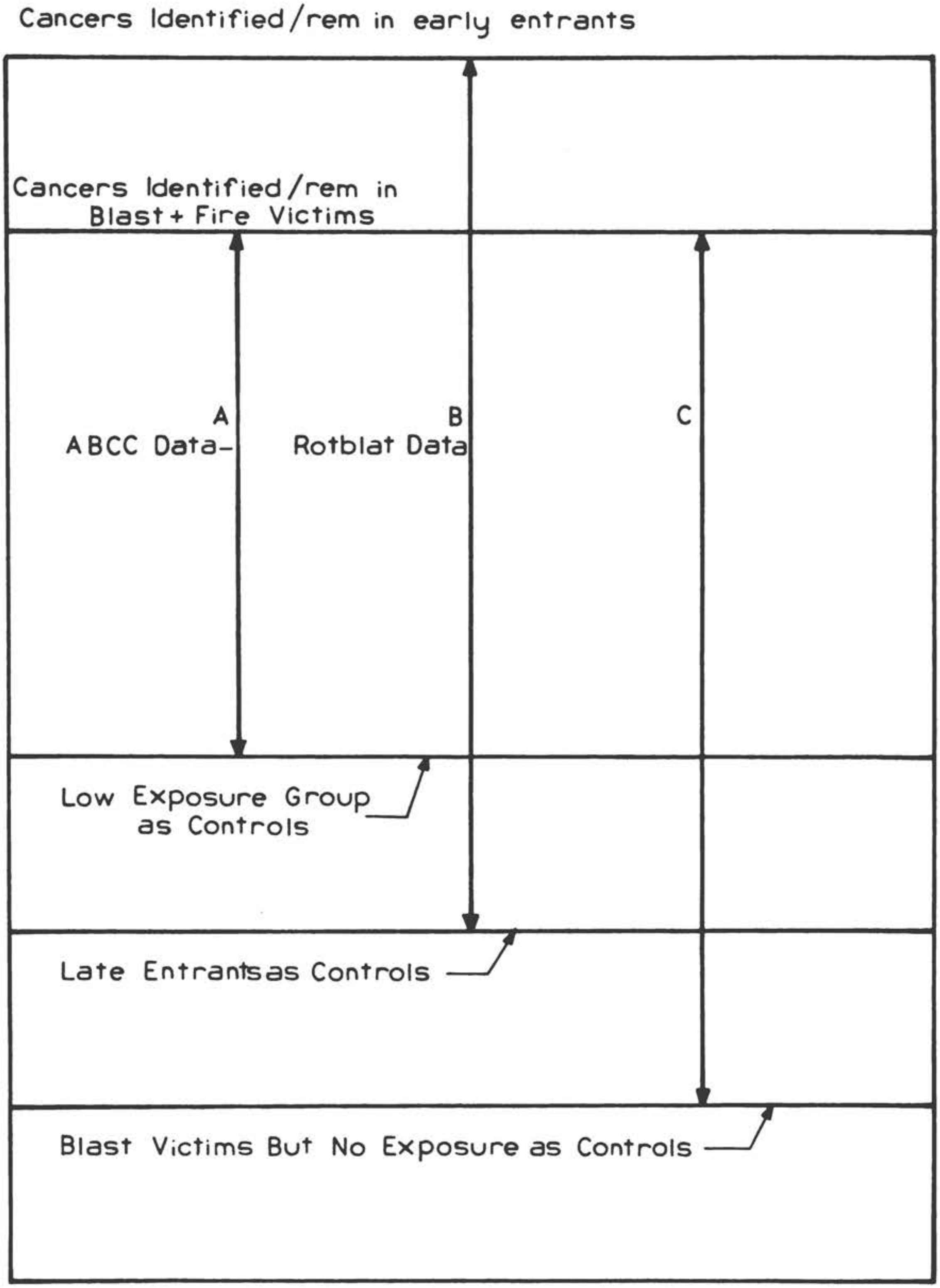


FIGURE 5 Showing How the Atomic Bomb Casualty Commission Data (ABCC) Tend to Underestimate the Cancer Risk of Radiation Exposure

developed to the point of clinical recognition among the children in the ABCC study, they were showing signs of being abnormally sensitive to infection. And Kneale[7] more recently has shown that the terminal phase of preleukemia is associated with a high risk of dying of pneumonia.

However, long before this and in the early period right after the bomb explosion, it would be the weaker and those more prone to develop cancer later in life that succumbed first to death from the radiation syndrome. Likewise the less healthy fetuses and sickly children who were more likely to be programmed to die of cancer were the ones who did not make it through this early traumatic period of suffering and distress. Thus, the stronger and the less cancer-prone survivors became the population upon which cancer risk to a normal population has been judged incorrectly by the standards-setting bodies and by our government agencies.

Rotblat[8] based the cancer risk on B, which as you see in Figure 5 is the cancers found in a group that entered Hiroshima shortly after the blast so they were subjected to fallout radiation and the neutron-induced activity. They were compared with a group that came later after this radioactivity had mostly decayed. He found this figure B in Figure 5 to be larger by a factor of 8 than had been determined by ABCC analyses.

The other human exposure population that is very extensively used, the foundation stone or hallmark in setting these radiation protection standards, is the ankylosing spondylitis patients who have been given very large doses of radiation to a small fraction of the body.

The increased incidence of cancer per rem (i.e., A in Figure 6) among the ankylosing spondylitis patients is that which was above the incidence in the general population taken as controls. However, studies have shown that the ankylosing spondylitis patients have a lower incidence of cancer than the general population because the disease shortens their lifespans and lessens the likelihood that they will live to old age where radiation-induced cancer incidence would be manifest. So, this as you see again, would tend to bias the data in the wrong direction and would underestimate the risk of malignancy from these exposures.

As a closing remark it seems most incongruous that our standards-setting bodies and our government agencies seem to feel so obliged to search for weaknesses in human studies of effects of low-level exposure of man to ionizing radiation and fail to recognize even greater weaknesses and biases in the high-level exposure data on which they naively base their standards.

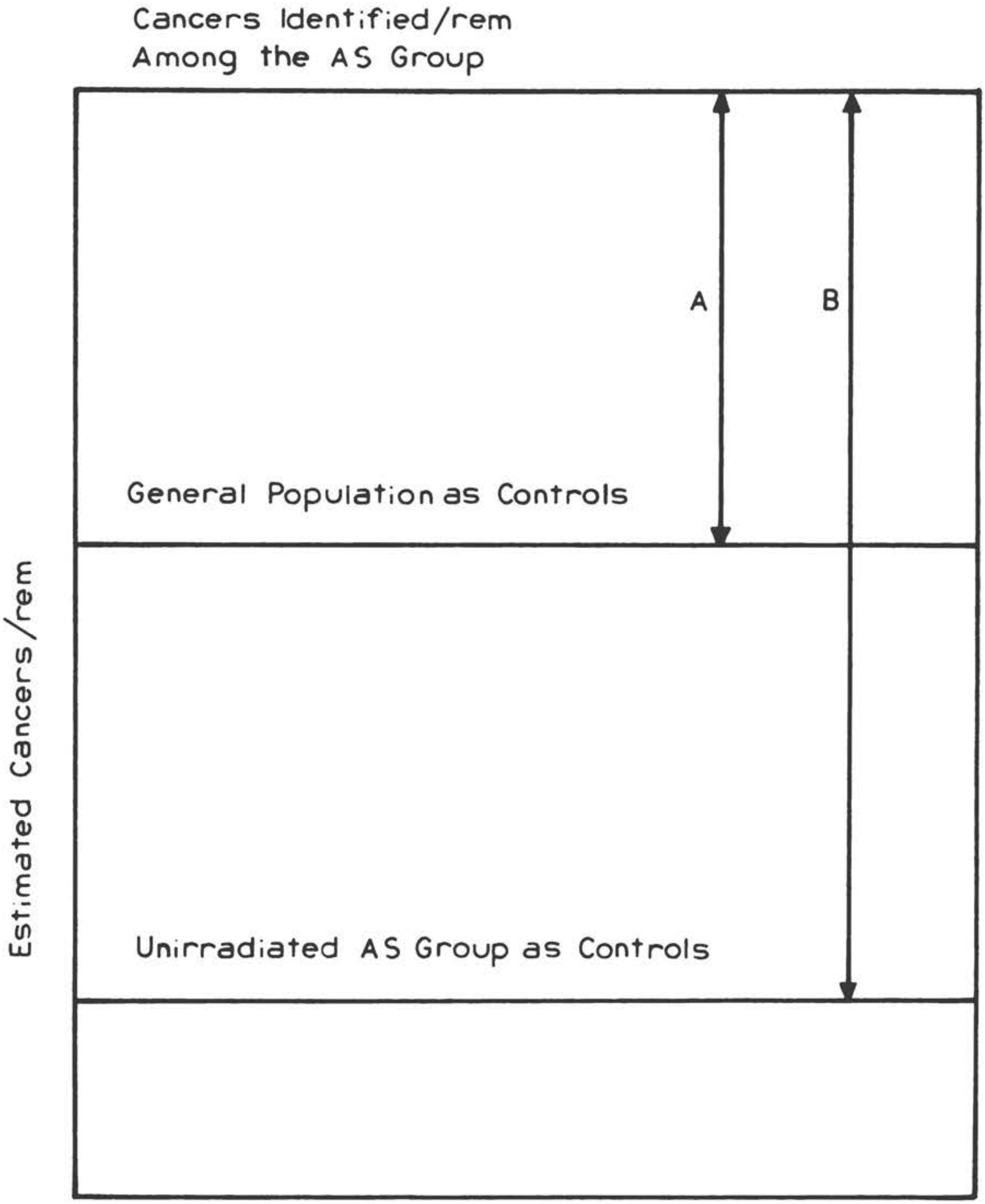


FIGURE 6 Showing How the Data on Ankylosing Spondylitis Patients (AS Group) Exposed to Ionizing Radiation Tend to Underestimate the Cancer Risk

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KOSHLAND: Thank you, Dr. Morgan. I'm going to come back and ask you to relate these factors to Dr. Webster's figure. Right now, let us turn to Dr. Land, who will present the case for another interpretation.

CHARLES E. LAND
Health Statistician, Environmental Epidemiology Branch
National Cancer Institute

[Dr. Land has requested that the following introductory paragraph be inserted into the record]:

I should at least have voiced some disagreement with some of the points raised by Dr. Morgan. First, I think Dr. Morgan has been far too uncritical in accepting some of Dr. Bross's more recent analyses of the Tri-State Study data. The inferences of very high cancer risks for certain susceptible subgroups simply do not stand up under critical examination, and I think that this has been amply demonstrated in the recent scientific literature. Second, the argument that preleukemic conditions among A-bomb survivors may have predisposed to early death from infectious disease, so that leukemias were underascertained, ignores the fact that this factor must also have operated in the case of leukemias not caused by radiation in this population, and hence relative measures of risk (from which, for example, estimates of doubling dose are derived) would be unaffected. Also, the leukemogenic effect of radiation is small in absolute terms, and could not be expected to operate as an important competing risk with respect to other forms of cancer caused by radiation. There are easier ways to explain the discrepancies between the risk estimates obtained by Mancuso, Stewart, and Kneale and those obtained from the much larger studies of A-bomb survivors and ankylosing spondylitis patients, than to conclude that the results of the larger studies must be wrong and those of the smaller study right.

I want to point out that in this issue, there really isn't any disagreement that radiation causes cancer, and there's little disagreement about what kinds of radiation cause cancer. There isn't much disagreement about cancer risk from high doses of radiation; that is, from about 100 to 500 rad, and the reason for that is that, in general, high doses cause enough cancer that it is noticeable when compared with normal risk. We've studied groups exposed to high doses, principally A-bomb survivors and patients given x-ray therapy for various diseases. The picture is clearest for cancers with fairly low, natural levels like leukemia, or cancers with marked sensitivity and ease of diagnosis, and breast cancer is one of these.

The key thing we have to keep coming back to all the time, both in studies of populations exposed to high doses and also in extrapolation of results to low doses in considering the question of low dose risk, is consistency of estimates. Do studies agree with each other?

For studies of populations exposed to high doses, the consistency is there and is reassuring. One of the best reasons for trusting the results from the studies of A-bomb survivors and medically exposed populations is that they tend to agree with each other. While it is not difficult to conceive of possible biases that could affect any one of these studies, it is very difficult to believe that all could be biased in the same direction. But as we've seen, there is disagreement about risks at low doses, but I feel it's important to note that while estimates may differ by factors of threefold, fivefold, they are small relative to natural risk.

In the first BEIR report, the estimate for the cancer mortality risk from a single rad of radiation, or a single rem, was between 117 and 621 per million per rad. And I'll talk a little bit later about the reason for the difference between the estimates of 117 and 621, but it's important to point out that these estimates of 117 and 621 excess deaths per million exposed are to be compared with the natural lifetime risk for a million people of 170,000 expected cancer deaths.

The most disagreement today is whether the risk may be even smaller than those given by the BEIR-I Committee, and as a matter of fact, it's

Dr. Webster who really is probably one of the foremost exponents of the view that the risk is smaller. But then there are exceptions, and I think Dr. Morgan is one of these; he believes the risk is even higher.

But why should there be disagreements about low-dose risk? The main one is that because the effect is small, we need to have extremely large numbers to estimate it. That is, as a general rule, if an effect is decreased by a factor of 10 and you assume for the sake of argument that the effect of 10 rads is 1/10 of the effect of 100 rads, you need -- if you're going to study people exposed to that dose -- you need 100 times as many people to study the effect of 10 rads as you need to study the effect of 100 rads. And you need 10,000 times as many to study the effect of 1 rad.

As an example, to estimate confidently and with some reasonable assurance, the risk of breast cancer following a 1-rad mammogram, which is probably on the order of 6 excess breast cancer cases per million women for each year after exposure if you allow a 10-year period of grace for the cancer to develop, you would need on the order of 100 million women, of, say, half of whom were exposed and half weren't.

If one used smaller numbers, if, for example, one said well, I can't get 100 million but maybe I can get a million, doing a study of that size -- and I think a million is a lot -- would most likely result in either an estimate consistent with zero or a very, very high estimate. In fact, it's not just most likely; these are the two choices. You either have one or the other. In order for an estimate to be statistically different from zero, it would have to be high with a small study. That's the problem. That's why even though most of the A-bomb survivors were exposed to doses less than 10 rads, the really evidential part of this series is the high-dose part.

Now, the disagreement among scientists about the low-dose risk comes from three sources. The first is the dose-response curve. A dose-response curve is a rule by which high-dose observations determine low-dose risk estimates. And you've seen possible general forms of dose-response functions. The second source of disagreement comes from the disagreement about the relationship between the effects of sparsely ionizing radiation like x-ray and gamma ray, and densely ionizing radiation like neutrons or alpha particles, and this is important because our best source of information on the radiation-induced risk of lung cancer is from uranium miners and A-bomb survivors of Hiroshima. The uranium miners are exposed to alpha particles, densely ionizing; the A-bomb survivors in Hiroshima but not Nagasaki had a substantial part of their dose from neutrons. These two disagreements are very closely related.

The third source of disagreement is that given risk information on the first 30 years, say, following exposure, you can get an estimate of risk for that period. But the population is going to live longer, so how do you project the risk to the end of life? I think this was alluded to by Dr. Morgan. But it isn't a matter of if you get more deaths you're going to get a higher risk estimate, because you'd usually do it in terms of how many years of exposure to risk rather than just to numbers of people.

But this disagreement, or this uncertainty, about what rule one should use to project to the end of life is responsible for this difference in the first BEIR report, the difference between 117 per rad per million and 621 per rad per million.

Figure 1 shows breast cancer among A-bomb survivors in both Hiroshima and Nagasaki, age-adjusted, and the little points with the vertical dotted lines are the data points. This is over the whole dose range, doses from zero up to over 250 rads to breast tissue. There are three curves here, fitted curves, all of which are consistent with conventional current radiobiological theory. One of them is the linear curve; one is a curve that's linear but also has this cell sterilization component so it comes up and bends over; and then the third one, the most complex, is a curve that is curvilinear but then comes up and bends over. As a matter of fact, with these data you can't fit one that just comes up like this because the data don't really look like that.

These curves all fit these data reasonably well. On the right in Figure 1 is a blow-up of part of the curve. This shows you the kind of variation one gets from these curves for small doses. The linear curve really gives the smallest excess risk per rad at low doses, but they really aren't all that different.

I'd like to say that I think that internal evidence suggests that the linear curve is the most suitable, and I think this illustrates the fundamental point I would like to bring out: for reasons I gave earlier you don't study the effects of radiation at low doses by studying populations exposed to low doses. You can't do it. You just can't get the numbers. So what you do is you study populations, at least a good part of which have been exposed to high doses, and you look for other aspects of the dose response which are theoretically associated with different curves of this type.

One of the consistencies that one might expect to see with the linear curve is that the two cities of Hiroshima and Nagasaki agree. That is, the neutron effect and the gamma-ray effect should be about the same, and that, in fact, for breast cancer -- and breast cancer may be an exception and it may be the only one -- you do see this and that's an argument, sort of a backward argument, for linearity.

Figure 2 shows leukemia incidence data for the Nagasaki A-bomb survivors. Now, I'm showing just Nagasaki, not the two cities combined, because for leukemia you see a big difference in the dose response between Nagasaki and Hiroshima that suggests two things. It suggests that the neutron effect is greater than the gamma-ray effect. It also, if you'll accept a certain amount of theory, suggests that the true dose-response curve is curved for low-LET radiation, for the sparsely ionizing gamma radiation, is curvilinear, curved upward.

Once again, here are three curves that fit these data pretty well, and it's rather difficult to tell them apart when you see them in the full range. But if you look at the enlargement on the right, you can see that it really makes a big difference which curve you decide to fit. The linear gives the highest one; and the complex curve, quadratic with cell killing, is the lowest; and probably this has the most respectability, at least right now.

Now, I'd just like to say one more thing. There is a related problem:

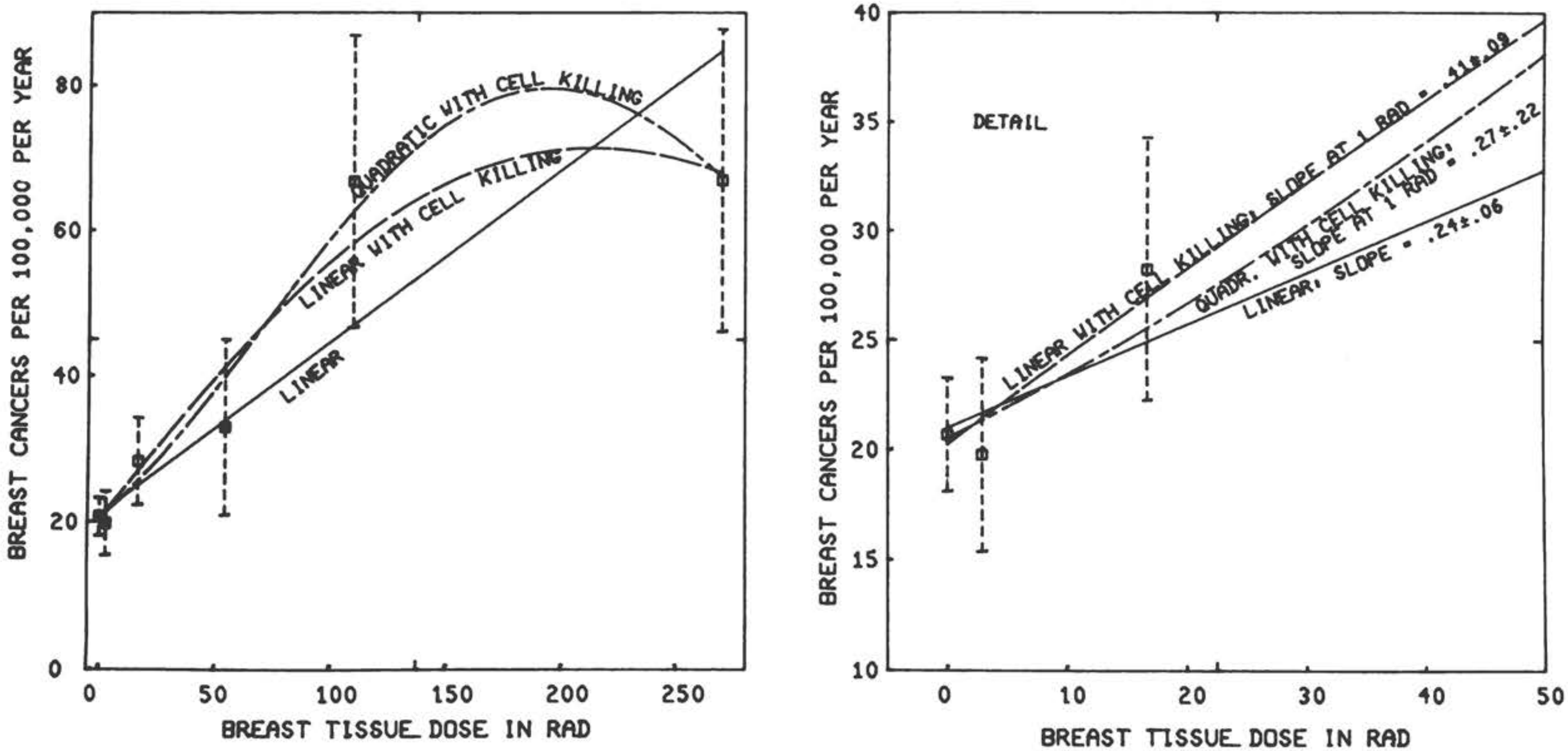

FIGURE 1 Breast Cancer among A-Bomb Survivors in both Hiroshima and Nagasaki

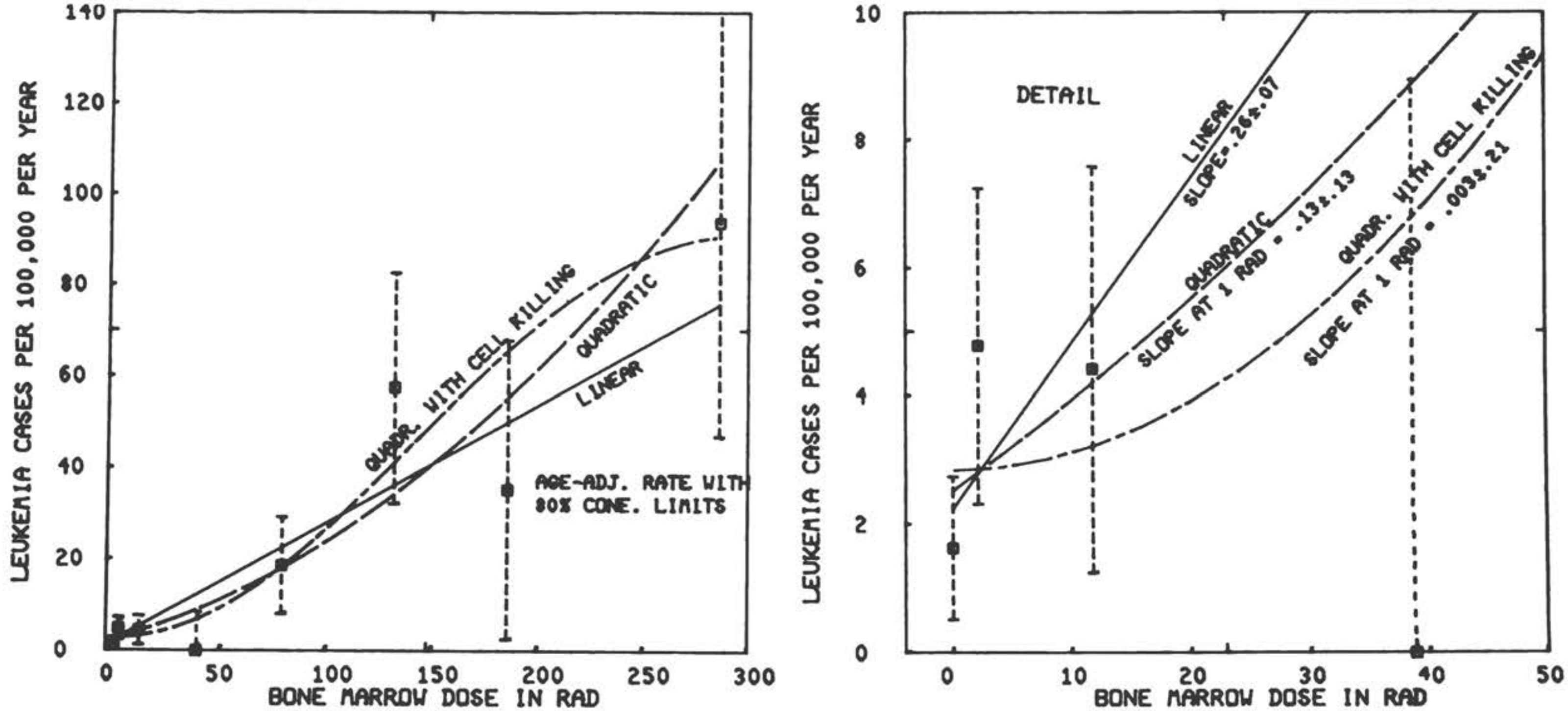

FIGURE 2 Leukemia Incidence Data for A-Bomb Survivors in Nagasaki Only

most data pertain to a single exposure or to a few exposures. Risks from chronic exposure may differ. Most likely, there's a relationship between this difference, if any, and the shape of the dose-response curve. If you have an upward curving dose-response curve, probably the risk from chronic exposure is going to be lower than for equivalent dose in a single exposure.

KOSHLAND: If you take the figures that Dr. Webster gave and you took your curvilinear or less-than-linear extrapolation, what factor would that reduce the number by? Would it be a factor of 10? I know you hate to make a flat statement, but would you interpret this for the audience.

LAND: The crucial question here is whether this less-than-linear curve has a linear component. If it has no linear component, then essentially, at very low doses you're going to get practically nothing. But if it has a linear component, then you've got a great deal of freedom, and I don't know. You might say that it might cut it in half, it might divide it by 5. I don't know, it depends on the data really.

KOSHLAND: What I'm trying to get is the limit, whether it is much lower or higher. Dr. Morgan, your figure would be how much higher than that number? A factor of 10?

MORGAN: It could be quite a bit higher. I have here a paper from Baum at Brookhaven which draws some other conclusions. For example, he looks at various types of malignancies and gets a coefficient less than 1 in most cases. So I think it depends on the way you're looking at the data, and if the coefficient is less than 1, then as you approach zero, of course, your slope is greater so the cancer risk might be a factor of 10 or more greater per rem dose.

KOSHLAND: I'd like to hear from Dr. Upton now.

ARTHUR C. UPTON
Director
National Cancer Institute

I think it's evident in all of these remarks that one is groping for some appropriate extrapolation model on which to predict what is going to happen in the low-dose domain where observations are not available. And it's clear that to do this, we need to have a theoretical framework, we need to make some assumptions about mechanisms, or better yet, we need to understand mechanisms.

In this connection, I think it's noteworthy that the curve that Dr. Webster showed for ovarian tumors in the mouse, so far as I know, illustrates the situation in which a tumor occurs after cells are killed. It is possible to induce these tumors in the ovary simply by transplanting the mouse ovary into the spleen and upsetting the hormonal balance of the system. I think it's now generally agreed that radiation kills the oocytes, sterilizes the ovary, and you have to give an appreciable dose to kill enough cells for this tumorigenic response to occur. Even though the mouse ovary is exquisitely radiosensitive, you have to give on the order of 10, 15, 20 rads before you see much of anything, and then the curve rises very steeply.

So I think that we simply cannot expect to fit every tumor system into a single dose-response pattern and say they're all linear or they're all curvilinear or this or that. We need to know more about them one by one.

Truly, for virtually all radiobiological systems that have been studied carefully in which good, quantitative information is available over a wide range of doses and dose rates, the response to weakly or sparsely ionizing radiations is quite different from the response to heavily or densely ionizing radiations. And usually, the shapes of the curves are different, although not always.

I think Dr. Morgan has stressed that there are uncertainties in the data and that we must be very cautious and very careful. We must not discard information that doesn't seem to fit simply because we don't like it for one reason or another; I would support the caution. I think that we still have enough ignorance so that we must be very careful about generalizations. I suspect that his concern about the possibility that some curves may increase as a power of the dose less than 1 may indeed turn out to be right, particularly for tumorigenesis or cancer induction by densely ionizing radiations. I don't think I have seen any evidence that this will happen with weakly ionizing radiations.

In this context it's reassuring that the estimates that expert committees all over the world are striving to come up with today are really not substantially larger than the estimates that were developed a decade ago. In fact, there are some suggestions that the estimates of a decade ago may have been a little on the high side. Certainly, the estimates of two decades ago were on the low side. We've learned a lot in 20 years.

Heterogeneity I think is to be expected, not just in the nature of the dose-effect relationship for different tumors, but from individual to individual. We have good evidence for age-dependent variations in sensitivity, and I think, as Dr. Morgan stressed, that there are homeostatic systems influencing susceptibility. So we must not assume that our estimates -- crude as they are and global or overall as they may be -- can relate to a particular individual, or even a particular subgroup of a population. They're not that precise; they represent averages over large numbers.

In saying that we shouldn't discard information, I say we must look at the animal data and the data we derive from radiobiology and cell systems if we're ever going to learn about mechanisms. It's the only way to proceed.

In the same context, I think we must not discard human data, but we must carefully evaluate every experiment; we must not give them all the same weight. That would be absurd.

Finally, I must say I don't see a serious disagreement among the panelists so far. I think they're all pretty much within a factor of 10 of each other, and that's truly remarkable, it seems to me, given the great distance we've come in the past 25 or 30 years in our knowledge of this subject. We're in far better condition to attempt to estimate the hazards associated with ionizing radiation than we are with chemicals. With any chemicals almost, we have enormous problems in defining distribution, metabolism, detoxification, and excretion, which badly confound analyses from one chemical to another, from one species to another, and even from day to day in the same individual.

KOSHLAND: Thank you very much. Dr. Vagelos?

VAGELOS: I would just say a word to support the point that Dr. Upton just made. Where the human data are lacking, because of the number problem that was raised by Dr. Land and Dr. Webster, then one has to go to animals and even bacteria to study mechanisms and to understand the low-dose effects.

Insofar as extrapolating animal data to humans, a vast amount of work has been done in the area of chemical carcinogens, studying them in animals where they can be done accurately and over relatively short lifespans of animals. Those data parallel very well the activity of those chemical carcinogens that have been tested and have been used in humans. So that animal data, in fact, are very useful in understanding population effects of radiation.

KOSHLAND: One of the places where there is a fair amount of data in terms of the linear extrapolation is cigarette smoking. You go from a range of, say, two packs a day, 40 cigarettes, and you can measure probably the rates for some of these folks of one cigarette a day and it looks maybe roughly linear. But these gentlemen have the problem of measuring the cancer rate of somebody who smokes a hundredth of a cigarette a day and then trying to figure the cancer rate in the population as a whole. So I think that sets the stage for the problem that is being addressed here.

Mr. Roper, would you like to make a comment?

ROPER: This is not my area of expertise, if I have any. The only thing I'd like to do is -- I keep going back and forth between getting the impression that Dr. Webster and Dr. Morgan are hundreds of miles apart, and then hearing that it comes out to a difference between 127 and 631, -- or whatever those numbers were -- out of 100,000. So, are we talking about a big difference in percentage terms? Or a big difference in the total scheme of things? That's the only thing that I think maybe needs a little bit of clarification.

KOSHLAND: Why don't I try to summarize, because I'd like to go on to the next question, and then be contradicted because I said it wrong.

As I see in our two questions, one is the sudden exposure, and we can argue about the multi-megacurie experiment giving 10,000 people 200 rems. As Dr. Webster said, this would be about 10 percent of the natural cancer rate for that population. And if you took, say, Dr. Land's curvilinear which might be a factor as much as 10 below that, and Dr. Morgan's could go about 10 above it. Is that correct, Dr. Morgan? It might be higher, but that would be the best estimate.

MORGAN: Of course, you could approach infinity or the cancers per rem would be very large with $n \ll 1$ (in Case C of my Figure 1) as the dose approaches zero.

KOSHLAND: Yes. And as far as the 1 rem a year; that is, the exposure of a million workers, again the rate in that case would be about 1/2 a percent of the natural rate, and I guess the same factors would apply. A factor of 10 lower by the curvilinear and a factor of 10 or more higher by the other approach. Is that correct?

So now I would open this particular question, the scientific level of risk, to questions from the audience.

MANUEL NAVIA: Looking at this problem and trying to extrapolate cancer numbers and correlating that to radiation dose is looking at the end of a very complicated process that really is not necessarily well understood to begin with, and for which there may be other factors.

Do you have any data or simpler model systems, say radiation damage at a simpler level? In other words, a correlation with radiation that doesn't involve cancer death, which is a very complicated process at the end of a very long line. Do you have any intermediate processes that you can perhaps correlate more closely with cancer deaths?

KOSHLAND: I think Dr. Upton might answer that one.

UPTON: One area that received systematic attention and led to good quantitative data quite early in radiation biology was the study of chromosome aberration induction. It turns out that the traversal of a cell by sparsely ionizing radiation is unlikely to deposit enough energy along the track of the traversing particle to break a chromosome fiber in more than one place or to break two adjacent chromosomes. The distribution of ionizing events is too infrequent.

Yet, if you don't produce two breaks close together in time and space, healing will occur with each break separately and there won't be an interaction between the two breaks. You won't have broken ends of different chromosomes being united illegitimately, as it were; you won't have inversions taking place in the same chromosome fibers. You don't get interchange, or two-event, aberrations unless you produce two breaks close together in space and time.

One traversal of low-LET or sparsely ionizing radiation is unlikely to do it, and will do it only very rarely. A single traversal of a densely ionizing radiation, on the other hand, has a high probability of producing two breaks in the same chromosome or in adjoining chromosomes and hence allowing two-event aberrations to occur.

So with densely ionizing radiation, the yield is strictly proportional to the number of traversals, or to the dose. But with sparsely ionizing radiations -- because there has to be an interaction between separate breaks -- the yield goes up as a quadratic function of the dose. Hence with sparsely ionizing radiation, there is a shallow linear dose response in the low-dose domain; but then as the dose is increased, successive traversals produce breaks that interact and a two-hit response results. Very carefully delineated kinetics!

We see, I think, good evidence for the same kind of kinetics with mutation, suggesting here that it's not two chromosome fibers that one must break simultaneously, but two strands in the DNA double helix for an irreparable type of lesion.

KOSHLAND: I understand that this is a big extrapolation, but would that tend to argue for the less-than-linear argument?

UPTON: Yes, but I think the radiobiologists take this kind of evidence as support for the contention that to the extent that cancer represents mutational damage or chromosome damage, one would not expect strictly one-hit kinetics all the way from the high-dose end of the curve down to zero.

PAMELA LINDSTROM: To give us an idea of what level of exposures we might expect to be exposed to, what would be an average, normal-operation radiation emitted in the air a mile or two from a nuclear power plant?

KOSHLAND: You mean under ordinary operation without a disaster. Maybe Dr. Webster would answer.

WEBSTER: A good example of that would be the release of radioactive gases from Three Mile Island.

LINDSTROM: No, without an accident.

WEBSTER: Well, that's very low. We're talking about a few millirems following a normal release.

LINDSTROM: Per what? Per year?

WEBSTER: Per release. It depends on the size of the release.

LINDSTROM: During normal operation?

WEBSTER: You mean if you're living outside a reactor fence, so to speak? That's limited to less than 10 millirems per year by the Nuclear Regulatory Commission, which is 1/10 of background. And that includes the exposure from the effluents. If that's what you want to know.

SPEAKER: How accurately are those emissions measured, in your opinion?

WEBSTER: There are monitors set up by the operators; that's required. So I think it's reasonably precise.

KOSHLAND: The question was how accurately is the Nuclear Regulatory Commission monitoring the normal operation of these plants.

SPEAKER: Actually, the Nuclear Regulatory Commission doesn't monitor them regularly.

WEBSTER: Well, you're asking a question about the reliability of the instruments and the people who operate the instruments. I personally think that these are reasonably expert people who have been trained to do this, and I think the instruments are very reliable. They are accurate to within 10 percent in general, which is pretty good.

SPEAKER: Do they accurately monitor all the effluent and day-by-day releases?

KOSHLAND: I think I'll ask Dr. Morgan to answer that.

SPEAKER: Do they accurately monitor all the different things, tritium and xenon and so on, regularly on their daily emissions?

MORGAN: There's quite a difference in what's measured from one utility to another. In the early period, there were some light-water reactors that gave average doses up to 10, 15, 20 mrem/y. At the present time, the levels are much less than that. All the utility dose records that I've seen recently are less than 5 mrem/y. It depends on the type of system and its years of operation. With the pressurized-water reactor, of course, a great deal of the dose is from tritium, and I don't think in most cases there are very good environmental measurements of the tritium activity.

Some of the plants have released very large quantities of tritium from their PWR's. With the BWR's, the boiling-water reactors, the principal radionuclides released, of course, are the noble gases. And again, I think we have operated in the past few years quite well at most of the nuclear power plants. The doses out at the perimeter of the plant are usually less than 5 millirems per year. This doesn't mean that you can't have streaming of gases during certain events where there would be some people who would get more than that.

But in answer to your question, I think that we have room for considerable improvement in making environmental measurements and should attach less weight to dose estimates based on measurement of stack effluents and meteorological data. I personally feel that the instrumentation in operation at Three Mile Island was not adequate, certainly not ideal. It did not approach what we could have had. So I'm not trying to justify what's done; I'm simply saying that I think we have a remarkable record with regard to the measured or estimated doses; that in some cases we may underestimate, in others overestimate the doses. But generally speaking, we're operating at less than 5 mrem per year average dose at the plant perimeter.

ALVIN WEINBERG: I would like to put the following question to the whole Panel or at least the radiobiologists. Looking at the data and considering the fact that it's such a very difficult situation with low doses, poor statistics and so on, can one rule out the possibility that the effect, say, below 5 rems or maybe below 1 rem, is zero? And if one cannot rule this out, then can anyone on the Panel explain why in the BEIR reports the lower limit is always set at different from zero?

WEBSTER: Let me take a first whack at that one. It's a good question because it strikes at the heart of this extrapolation process which we've been talking about.

There is one area of evidence where it is claimed that human effects have been observed at about the 1 rem level. This is a rather controversial study but nevertheless it's been repeated by other people and duplicated. This is the area of damage to the fetus due to x-ray examination during pregnancy, and it is claimed that at that level the incidence of leukemia and other cancer is elevated above nonexposed people.

WEINBERG: Could I just respond to that one because I think, as you say, that is really at the heart of the issue.

Dr. Thornberg's decision to evacuate pregnant women was based on the Stewart/Kneale data which say that fetuses are that sensitive. And I guess I therefore am a little bit puzzled that you said a factor of 5 between fetuses and grownups. My figure is more like a factor of 100. But you neglected to point out that this was a retrospective study and that there are, indeed, profound methodological difficulties with this Stewart and Kneale analysis.

WEBSTER: Well, I did say it was controversial. There is some evidence on the other side, particularly from the Nagasaki experience, in relation to women who were pregnant during the explosions: their children, followed for the next 10 years, did not show cancer.

WEINBERG: Actually, it's controversial not only because of Nagasaki but because the methodology of Stewart and Kneale itself is suspect.

MORGAN: [As Dr. Morgan did not have the opportunity to comment at this point during the Forum, he has asked that the following be entered into the record.]

Dr. Weinberg put his question to the whole Panel and so I must respond because I strongly disagree with the implication that the results of the study should not be taken seriously because it was a retrospective study and "the methodology of Stewart and Kneale itself is suspect." As I see it, nothing could be further from the truth than such an implication. I consider this study showing a 50 percent increase in cancer risk due to in utero exposure to diagnostic x-rays the most important study ever conducted on the effects of low-level ionizing radiation on man.

In the early period when the results of this Oxford study were first published (1956), there were many skeptics and publications by critics. But during the past two decades as the criticisms have been adequately addressed by many scientists and a large mass of additional data has been collected, the Oxford findings have been generally accepted throughout the scientific community; even some of the early critics, e.g., Mole, have later published papers agreeing to the validity of the study and its conclusions. MacMahon in a survey of a number of similar in utero studies (in 1963) gave strong support to the Oxford findings. No one questions that it is a retrospective study, but several papers have shown that there are fewer biases in this study than in some of the prospective studies, and recent prospective studies now strongly support the Oxford conclusions.

Regarding the divergence of these data from the finding of only one cancer in the ABCC in utero exposed children, one must concede that there are many reasons why the fetuses of mothers who were subjected to fire, blast, deprivation, loss of loved ones, etc. did not survive to die of cancer unless they were unusually healthy babies. This in turn meant they were free of in utero stages of cancer developments, so this ABCC prospective study was of a select population that could be expected to have a low cancer incidence among children. There have been many publications showing the Oxford data are more reliable than the ABCC in utero data. These same authors also have answered the various early criticisms; for example they have shown that it is not the "sick" fetus or cancer-prone fetus that is likely to receive a pelvimetry x-ray exposure but the large healthy fetus for whom a cesarean delivery is anticipated and often required. Even the age at which leukemia appears in the children of the Oxford study helps to identify the radiation-induced malignancies as distinguished from those of other etiology.

KOSHLAND: This is an important subject and we could debate it, too; I think you've done a very clever job at picking the advocate of low-level calculations to defend a higher level calculation.

We now have a sort of a ballpark estimate, as I said, in these figures, about 1 percent to 1/2 percent of the natural level with a factor of 10 which would make it 10 percent, or a factor of almost down to zero. I now would like the Panel to discuss the second question which is: What criteria can be used by society to establish an acceptable risk policy in the area of nuclear radiation for the workers in the plant and for the population as a whole? There are two types of risks, it seems to us, as far as society is concerned: the levels we make for the occupational workers who choose to work in a plant, and the level of risk we make for the environment in case of an accident of some sort.

Let us start out with Dr. Vagelos on this one.

P. ROY VAGELOS
President
Merck Sharp & Dohme Research Laboratories

The energy of the world, as we know it today, is limited to several sources -- oil, hydroelectric power, natural gas, solar energy, coal, which the United States has in great abundance, and nuclear energy. Oil is limited as a source in the U. S. as well as the rest of the world. The two sources that are most important for the generation of electricity in the immediate future are coal and nuclear energy. Hopefully, some day in the more distant future, solar energy will also play a major role; but for the moment, solar energy is not cost-efficient. Let us therefore compare the risks associated with the two presently available alternatives for the generation of electricity, nuclear energy and coal.

As I am not an expert in this field, I have gone to several good sources for expert information. One source that has yielded much information is Nuclear Power Issues and Choices, the report of a study sponsored by the Ford Foundation in 1977. Health risks from nuclear energy include those from uranium-mining accidents, which would cause about 0.2 to 0.5 cancer deaths per gigawatt-year of nuclear electricity generated (equivalent to 1 million killowatts generated for a year). Occupational exposure, which includes about two-thirds coming from reactor operations and repair and one-third coming from mining and milling, would account for about 0.2 to 0.3 delayed deaths per gigawatt-year. Thus total occupational fatalities would be in the range of 0.4 to 0.8 deaths per gigawatt-year.

Public health consequences that result from nuclear electricity generation are more difficult to quantify with any degree of certainty. The population exposure during normal operations is due primarily to radon emissions in mining and milling and also to routine effluent emissions of carbon-14, tritium, and krypton-85. A population dose commitment of 1,000 man-rem would correspond to approximately 0.2 latent deaths per gigawatt year. Thus the total health risk for workers and public is about 0.6 to 1.0 expected deaths per gigawatt-year.

If reprocessing and plutonium recycle were added to the present uranium fuel cycle, the distribution and magnitude of health risks would be changed a little, with a probable reduction in occupational deaths in mining and milling and a decrease in population exposure from radon and its daughters, but with the introduction of new risks that are very uncertain. The GESMO figures for reprocessing and recycle suggest that a net effect would be the reduction in occupational deaths of about 0.04 per gigawatt-year and an increase of 0.07 deaths in the general population.

Public health consequences of nuclear plant accidents are much more difficult to quantitate. Probabilities cannot be predicted with certainty. The Ford Foundation study antedated the Three Mile Island accident, and therefore there was no precedent to evaluate at that time. Consequence calculations have been made, however, based on modeling studies. The Reactor Safety Study (WASH-1400) projected average accident consequences of about 0.02 latent (cancer) fatalities per gigawatt-year. This study, however, was flawed by methodological problems. An extreme upper limit, if one is very pessimistic with all uncertainties, would put the risk as high as ten deaths per gigawatt-year.

Another source that I will refer you to is Risks Associated With Nuclear Power: A Critical Review of the Literature, an analysis that was written for the NAS Committee on Science and Public Policy and published in 1979. This paper concluded that the routine operation of nuclear power plants would cause about 0.5 cancer deaths per gigawatt-year of nuclear electricity generated. If one assumes that 40 gigawatts of energy are produced in a year, as occurred in 1975, then the nuclear industry causes 20 cancer deaths per year. For perspective, I should mention that there are about 360,000 cancer deaths in the U.S. each year. This report estimates that the number of deaths from accidents in uranium mines is about 0.4 per gigawatt-year of nuclear-generated electricity. Both of the estimates of delayed (cancer) and accidental mining deaths from nuclear electricity are similar to the numbers projected earlier in the Ford Foundation study. The report, however, emphasizes the many remaining unknowns: the hazards posed by terrorism; not enough is known about the leaks from waste storage areas; and the difficulty in assessing the risks of a major nuclear plant accident.

Let us now turn to a consideration of coal as a source of electricity. There are vast amounts of coal in the earth, and it appears to be the logical alternative to nuclear energy until solar energy can be made cost-effective. For expert opinion I refer again to the Ford Foundation report. Coal mining accidents account for approximately 0.5 fatalities per gigawatt-year of electricity generated. The transportation of coal is very costly in human lives: between 0.55 (WASH-1224, 1974) and 1.3 (Sagan, 1974) deaths are estimated per gigawatt-year. These deaths occur mostly when automobiles are hit by freight trains hauling coal at grade crossings. The construction of generating plants is responsible for about 0.05 deaths per gigawatt-year. Therefore, coal mining and hauling plus plant construction account for about 1.10 to 1.85 deaths per gigawatt-year of electricity generated.

The effect on the general population of electricity generation by coal combustion is difficult to assess with certainty. Coal combustion causes the production of two major air pollutants, sulfur dioxide (SO_2) and, perhaps even more important, suspended sulfates which are the oxidation products of SO_2. Sulfur-related pollution causes five different health effects, according to Finklea (1978): (1) respiratory disease deaths; (2) aggravation of heart and lung disease in the aged; (3) aggravation of asthmas; (4) excess acute lower respiratory disease in children; and (5) excess risk for chronic bronchitis. Deaths caused by coal combustion effluents were estimated in an NAS report in 1975 by using the North-Merkhofer model, which indicated an extreme range of 2 to 100 deaths per gigawatt-year for a plant burning 3 percent sulfur coal. The wide range is due to the great variation that would be expected due to differences in plant site as well as uncertainties in dose-response relations between health effects and sulfur-related pollution. Now, with new plants meeting current new source standards for sulfur emission by burning low-sulfur coal, the estimated deaths would be reduced to 0.4 to 25 per gigawatt-year. The addition of lime scrubbers to reduce sulfur emissions would further drop this range to 0.04 to 10 deaths per year.

Thus new coal-fired plants meeting new source standards would be responsible for about 2 to 25 deaths per gigawatt-year of electricity generated if one factors in the deaths from occupational and public effects of the coal cycle and the effluents of coal combusion. The addition of lime scrubbers would reduce this further.

The general conclusion is that, in spite of great uncertainties in attempting to quantitate health hazards due to the generation of electricity from nuclear power or from coal combustion, in the worst case there would be about a similar number of deaths from nuclear energy and from new coal-fueled power plants meeting new source standards.

KOSHLAND: In the calculation for nuclear power, you calculated the numbers in the plant that would be hurt, but you did not mention transportation. Is that a significant number, because you don't transport as much nuclear reactor waste?

VAGELOS: I assume that's small.

MORGAN: I think in the comparison one must take the full cycle, both of the coal and the nuclear operations. For example, Dr. Victor Archer pointed out in the Senate hearing two years ago that in his study group of the noncarcinomas he already had 170 deaths among the uranium miners from exposure to the daughter products, the radon-222. That certainly should be added in as part of the risks in the nuclear energy cycle.

Potential deaths from accidents in shipping of the fuel, reprocessing and fabrication of fuel assemblies should be added in for both coal and nuclear.

KOSHLAND: Are you saying in terms of public policy that if the reactor industry could come up with a record of safety equivalent to coal, you think that would be an acceptable risk policy?

VAGELOS: Coal is not without risk, and other sources of energy are not without risk. If nuclear power risk, through proper siting of the power plants, through proper safety introduced in the plants can be kept to a number that's equal to or lower than health hazards from other sources, then it certainly would be a good policy to approve it.

KOSHLAND: Mr. Roper, would you comment on that?

ROPER: I think it makes sense that if the risk is the same from coal and nuclear, the standards should be the same. But it seems to me that in order to develop a policy it's incumbent on the scientists, even if they disagree -- and that's natural -- to come up with a ballpark kind of figure of what the risks are, a minimum-maximum kind of thing so that the public can assess that. Are we talking about a 50-50 chance of dying or a 1 out of 2000 chance?

I think that the educational process that coal is more dangerous than nuclear should be initiated. Then if the public will accept that as a reasonable standard, that's fine. But if people have what you might describe as an unreasoned fear of nuclear that they don't have of coal -- coal is something they're used to, they've experienced it, they know the risk or think they know the risk and they're willing to accept it, but they're scared stiff of nuclear -- then I think you've got to establish a different standard for nuclear than for coal. Because one of the purposes of this whole thing is to serve the public interest, and if by establishing the same standard for the two of them you scare the hell out of the public about nuclear, then you're not serving the public interest.

VAGELOS: One other number that puts this in perspective comes from this National Academy Committee on Science and Public Policy report: using the figures of 1/2 death per megawatt year of nuclear electricity and the number 40 megawatts generated per year, that would say that there would be 20 cancer deaths per year in 1975 coming from nuclear energy in a population that would also have 360,000 cancer deaths that year.

KOSHLAND: As I understand, you're saying that the public's fear of nuclear radiation probably could be assuaged in one way as far as the workers in the plant are concerned because we have experience with industrial accident safety. But as far as reactor accidents, that might be a different problem because of the unknown. We haven't really been operating for many years with nuclear reactors. That would suggest that the safety factor in regard to the reactor itself might require a different standard until we have experience, and then it might be altered. Is that right?

ROPER: Yes, I think that's right. As far as the workers are concerned, it seems to me there that the way to implement a policy is to have a completely open, frank exposition of the risks the worker runs if he takes the job -- again, with minimum and maximum kinds of limits -- and let him decide. But the dangers can't be glossed over or kept quiet on the grounds that since nothing will ever happen to him, why scare him. I think he has got to have the full disclosure and then make the decision himself.

KOSHLAND: Are there any other comments from any members of the Panel?

WEBSTER: I agree that the hazards of working with radiation should be explained to the worker, particularly since they are not as obvious as the risks in some other industries, such as mining and construction. Standards for radiation control should be set so that the risks are comparable with those experienced in other relatively nonhazardous industries. There must be some risk since radiation levels cannot be reduced to zero, but I don't believe that radiation workers should be sequestered into a very low risk category, or, in other words, receive a preferred treatment relative to other workers. I believe a case can be made for examining the long-term health experience of all radiation workers nationwide: only this way will the large low-dose population that Dr. Land emphasized be gathered together. This experience could then provide the basis for future changes in the occupational standards.

LAND: Even if the expected number of deaths each year attributable to air pollution, say, from coal-fired electric power plants is many times greater than the expected number attributable to nuclear power, mainly from the possibility of a catastrophic accident, there may be reasons for preferring the former alternative. The deaths caused by a catastrophic nuclear accident may tend to occur at younger ages than those caused by an increased level of air pollution, for example. Perhaps more important, though, is the consideration that increased air pollution is very unlikely to cause several deaths in a short period of time in a given group of persons, like a family, while a nuclear accident, in the (admittedly unlikely) event that one should occur, might well wipe out whole families. That is, for an individual, the nuclear alternative may offer the better chance of survival, but from the group point of view, like that of a family, the chances of survival may be better under the nonnuclear alternative. Much of human behavior is directed toward the welfare of groups at the expense of that of individuals, and the "irrational" public fear of nuclear hazards could be another example of this.

KOSHLAND: Dr. Morgan, I know you have strong feelings about medical x-rays. Would you put those in perspective in terms of a sort of common fear that we've been with for many years as compared to a new one?

MORGAN: For many years I've felt very strongly that the population has an unwarranted fear and phobia of radiation from the nuclear industry. If such people were really honest with themselves and concerned about the radiation problem, they would realize that most of the problem is from unnecessary medical diagnostic procedures and take heroic measures to reduce it. One can show very easily that were you to reduce the unnecessary portion of medical radiation (i.e., the unnecessary portion that's been alluded to by the Bureau of Radiologic Health and other groups) by 1 or 2 percent this would reduce the man-rem dose and the total population dose of the United States more than the complete elimination of the nuclear industry. Unless people, in their ignorance, feel there's a difference in the photons from medical procedures and from nuclear energy, then I think that some of this hysteria is not completely justified on a rational basis.

So the only way I think one can rationalize the situation is perhaps that some people fear the spectacular event, the Three Mile Island event. As I see it, the only reason why people would be so disproportionately concerned about nuclear rads compared to medical rads is that their knowledge is limited and that they distrust the nuclear industry -- and, I might add, with considerable justification.

VAGELOS: I want to put in perspective the projected cancer deaths or cancer incidence that will occur because of the Three Mile Island accident. Would someone like to give us that number?

WEBSTER: Well, it depends on what you take as being the population exposure when that occurred. The biggest number that I have heard and would have the biggest effect was 5000 man-rems; that means that if you put it on the basis of 5000 people, they got a rem apiece, so if you put it on the basis of 50,000 people they got a tenth of a rem apiece, and it's the product of the people and the dose that we're talking about. That's a measure of the whole population exposure.

On the basis of 5000 man-rems, and if we take my number, which is a linear extrapolation number, 100 cancer deaths eventually per million people for 1 rem, which is the same as saying 1 death in 10,000 people getting 1 rem. In other words, you get 1 death from 10,000 man-rems, and with 5000 man-rems, which is what we got at Three Mile Island, there would be a half a death from cancer. And if we multiply that by 2 or 3 to account for incidence, we'd get possibly 1 to 2 cases of cancer over the long term; that is, when all those people are followed up to the ends of their lives. Now, if my number is wrong and Dr. Morgan's number is right, then we might be talking about 5 times as many. So our 1 or 2 cases of cancer will go up to 5 to 10 cases of cancer. But I can't see it going any higher.

KOSHLAND: It seems to me that the public worries about another scenario. Three Mile Island was an accident that was contained, even though for a while it looked like it might not be. But if it had happened, say, in a Con Ed plant near New York City and the wind was in the wrong direction, it could be a totally different scenario.

Supposing it is decided that to guard against a disaster you put these plants relatively far away and minimize the danger to the general public. But in order to make them economically viable you therefore have to lower the safety standards within the plant. That's a tradeoff, and therefore, you lower the radiation levels within the plant. Is that an acceptable policy? In other words, at what point do you trade off hazard to the workers against the hazard to the population as a whole? Essentially that's what we're doing with coal at the moment. The workers in the plants have a quite high hazard in the mines, and the rest of us have very little hazard.

ROPER: I think you probably would have to go with the public standard and that would just put all the more emphasis on the disclosure problem with the employees and the freedom of choice and pay and everything else.

I would also suggest that moving the nuclear plants farther out might be, to some extent, an actual improvement in the safety factor and, in another respect, might deal with the psychological fear. You don't see the darned thing, it's miles away. So such an arrangement might have a two-edged effect as far as the public is concerned.

KOSHLAND: Let us now have a few questions from the audience.

ANNA GYORGY: I'd like to just comment on something that Dr. Morgan said and then ask a question of Dr. Vagelos. It's obviously true that in terms of quantities of medical x-rays, they are very much abused and overused. But I would submit that the difference there and the reason that people are so upset about the nuclear fuel cycle is that you can refuse to take x-rays, but you cannot refuse to be a victim of the nuclear industry.

Also, it is not just plants, but it is the entire fuel cycle that leads me to the question and points I have about some of the statistics you gave, Dr. Vagelos, which I think, from what I've read, have a lot wrong with them. For example, do the figures for miners' deaths that you gave from the Ford study, I presume, include the point that was raised in the Jordan memorandum through the NRC I believe in 1977 that said that the NRC's analysis of health effects of the mining for miners is off by a factor of 100,000. That's one question; is that included?

Secondly, you mentioned WASH-1400. That is the Rasmussen study which, after more than 3 million dollars spent, has been repudiated by the NRC. I don't think that you can give us figures from that study without mentioning the fact that it is under quite a pall these days.

And thirdly, it's really quite clear that the whole comparison with coal is illegitimate anyway because miners' deaths in coal mines can largely be removed or helped to a great extent by increased safety measures such as venting of mines. This is a political and social problem, something that cannot be quantified. Therefore, any comparison between the two, although it's been done for years, really is illegitimate. And I would submit that our unreasoned fear is not so unreasoned after all, and I'd like to hear your comments on that.

WEBSTER: I would like to point out that the figure from the WASH-1400 report was .02 and the number Dr. Vagelos used, under pessimistic circumstances, was 10, which is 500 times greater, so he wasn't trying to hide under the WASH-1400 report. He said that that's an underestimate, and it could be 500 times greater.

And as to the comparison between coal and nuclear, it's a very good one because in both cases the exposure is of the public. The major one we're talking about for coal is air pollution, not mining. Both radiation and air pollution are involuntary exposures. You can't turn off the radiation you get, and you can't turn off air pollution either.

VAGELOS: Thank you, Dr. Webster. The third question that you asked, or the comment, was that certainly there can be an improvement. I think I mentioned the improvements -- siting the coal, the place where one burns the coal, using low-sulfur coal, using scrubbers. One can also improve, in the same ways, the nuclear plants. One can site them better, one can put in and use safety procedures. I think those can be done in parallel.

And, the numbers I quoted were the numbers for the new plants with optimal operation for coal.

MORGAN: You commented that in the case of medical procedures you may have a choice, but if you live near a nuclear plant, you may have no choice. I would like, though, to emphasize the fact that with proper education you do have some choice of medical diagnostic procedures. When I was at Oak Ridge National Laboratory, our facility gave an average dose to the skin of the chest of 15 mr for a chest x-ray; whereas, we made some surveys in some not-too-far-away areas where the average was up between 2000 and 3000 mr per chest x-ray. We got far more useful medical information from the technique we were using at Oak Ridge.

I could point to situations in the city of Atlanta where I live and other places where some general medical procedures are giving less than a half rem for a given procedure; whereas, others are giving more than 10 rems for the same information, or in attempting to get the same information. Usually, the higher doses in these cases give less useful information. So by education you do have some choice in reducing your dose from medical procedures.

DAVID BALTIMORE: The title of this meeting is "Nuclear Radiation: How Dangerous Is It?" and the major discussion concerns nuclear power plants and how dangerous are they. I think Dr. Morgan is right that from everything we've heard, nuclear radiation has its danger. But its danger is probably as a medical procedure much more than as a part of the nuclear power plant industry. The danger that we've heard about nuclear power plants that everybody seems to agree is a real one is a meltdown or explosion or some catastrophic event. It seems to me we should be calculating that and its effects in terms of power plants, not radiation. Because if I understand Dr. Webster's calculations correctly, even in the case of a meltdown the problem is hardly a problem of cancer; the problem is the intense radiation locally, and the lethal effects of that. Those things can be calculated, and we know those numbers quite well.

So the really hard number to calculate -- in fact it's obviously an impossible number -- is the number that relates time to the probability that any given nuclear reactor is going to go. But I think we could talk about that a little more than we have.

UPTON: I think that Dr. Baltimore is correct, as I perceive it. The major concern of the public is the possibility of the worst case scenario -- the China Syndrome -- and what that would entail.

In the Ford Foundation study, there was the estimate adapted from the WASH-1400 study of some 3000 early fatalities, 45,000 radiation sickness victims who did not die, 45,000 cancer deaths over the remaining lifetime of the population, 240,000 thyroid nodules which were nonfatal for the most part, 30,000 genetic disorders of various kinds, and an area of about 3200 square miles rendered unhabitable for some period.

The estimates, of course, were predicated on worst case assumptions about population density, weather conditions, and inability to evacuate within a distance of 10 to 20 miles. If one takes more optimistic predictions, then the early deaths go down substantially, but the late deaths, the cancers, the genetic disorders, may not change.

As I recall, the WASH-1400 calculation assumes a risk of about 1 such catastrophic meltdown and release -- breach of containment and release -- for every 200 million reactor years. I think the Ford Foundation group looked at the probabilities that went into that calculation and thought that they could indeed be too low by a factor of 500. And it's that factor of 500 that makes the difference between the .02 fatalities per 100 megawatt year electric versus the 10. The 10 is based on the supposition that one will, in fact, have such a catastrophic incident from time to time, and one must average out all the resulting deaths over the remaining period of operation of all the reactors that are online.

LAND: It seems to me that we should consider the question of whether in order to enjoy the benefits of nuclear power we have to put up with a risk of catastrophic accidents that is as high as that quoted by Dr. Upton. Are there, for example, reactor designs that offer significantly less risk of catastrophic accidents, or less risk from terrorist actions, than the designs currently being used in this country? I have heard, for example, that the Canadians use a reactor design that is much less susceptible to meltdown than the pressurized-water reactors in use in the United States.

KOSHLAND: I should put in a commercial here. Part of the reason we've limited the discussion is that we are going to have subsequent Forums on the nuclear waste disposal problem and on reactor safety. Your question should be fully covered when we come to reactors.

NORMAN MILLERON: I'd like to speak about a subject that has not been raised that emphasizes the difference between coal and a large-scale nuclear industry.

First of all, when you have a large-scale nuclear industry you develop a lot of waste radioactivity. And unfortunately, this places a weapon in the hands of a terrorist, the like of which you can't imagine. I'm not speaking about a nuclear weapon; I'm speaking about what the government of the United States has chosen to speak about this past year; that is, the idea of releasing some high levels in a large population area. The terrorist would have a weapon, the like of which we couldn't imagine its effect. If you tried to do that with coal you'd have a little harder time.

Now, the consequences of a large nuclear industry are vastly different than having a few reactors. Dr. Vagelos mentioned an energy study. It's a curious fact that as far as I know, the energy study that the National Academy carried out and still has not released does not mention the temperature gradient in the freshwater reservoirs and lakes of the United States. A paper appeared in the January 12 issue of Science pointing out that large lakes like Lake Mead and Shasta Lake in California would generate 3 to 4 times the electric power that they presently generate. This is due simply to the thermal energy represented in a gradient in the water. Although this is an engineering reality that we need not go into, when you quote the energy potential of the U.S., you can't neglect things like this. As far as I know, there is no work going on in Washington or any of the states to develop this energy source, and yet we have a fairly large program in the ocean thermal gradient, which is a much harder technological problem.

KOSHLAND: Thank you. Although the comment is very good, I would like to concentrate in this Forum on the biological facts. We're not trying to decide these other issues; in fact, I underestimated our program. At some stage I think we will be considering ultimate sources, their potentials and hazards, also.

I will take one more question.

ALBERT BATES: What is the mechanism for assessing the acceptability of, let us say, 20 to 40 deaths per year from routine releases to unidentified and basically uncompensated victims who are citizens both in present and future eras without the opportunity to either consent or to resist the exposure? And is this process scientific or political, and is it democratic? Considering it is spanning more than one generation, is it fair and ethical?

KOSHLAND: That's a great question. It almost leads perfectly into the third and final question of our program, so maybe if the speakers don't answer it at the end, I'll call on you again.

The third question is: Is the public's perception of radiation risks accurate, and if not, what should be done to correct it, and how much should it influence policy? I'll start that off by asking Mr. Roper to comment.

BURNS W. ROPER
Chairman of the Board
The Roper Organization, Inc.

I think at least as far as nuclear power is concerned, we have to start with a little background. All kinds of surveys, including our own, have shown that people do not accept the fact that there's an energy shortage, but at the same time, they accept the fact that there's an energy problem. In a recent study we gave people some 10 different national problems and asked them which ones required major governmental effort. Number 2 on the list, right behind inflation, was trying to develop new energy sources and find better ways to conserve fuel.

Last March -- and the first day of interviewing was the day that Three Mile Island became a real problem -- we asked people whether they thought the likelihood of a severe energy shortage such as we had in 1974 was very likely, somewhat likely, somewhat unlikely, or very unlikely. We got the highest percentage we've ever gotten saying that another recurrence of an energy shortage is very likely. Half said very likely and another 29 percent said somewhat likely.

So I think the people's views of nuclear energy have to be looked at in the context of their concern over energy.

We posed a dozen different things that President Carter said in his July 15 energy crisis speech to a national sample and asked them for each one whether they agreed with what he said or didn't agree. They agreed with practically everything he said; there were a couple of things they didn't. But specifically, 61 percent agreed and only 25 percent disagreed that we must continue to rely on nuclear power plants to supply part of the nation's energy needs.

A survey in March -- the one that I mentioned before when Three Mile Island had just broken; we did not get the full effects of it in this -- showed 57 percent saying we should go into a greatly increased program to develop nuclear energy; only 30 percent opposed to it. That was down a little from what it had been a couple years earlier, but not much. My point is that because of energy, people place a strong reliance on nuclear.

Now, we did a rather extensive study five weeks after Three Mile Island had occurred, been solved, quieted down or whatever you want to call it. We asked people in that survey whether they approved or disapproved of using nuclear energy to produce electric power, and it was approved by a 5 to 3 ratio.

Early in that same study, we gave people a whole list of things that were in the news and asked them how closely they'd been following each. Three Mile Island was the second most closely followed story; the number one being inflation. Despite the lack of any deaths at Three Mile Island, it attracted a lot more interest than the floods in the southern states or than the tornadoes and deaths in Texas and Oklahoma.

Before Three Mile Island, five weeks after it, and again later this summer, we asked people to evaluate a number of different risks. These were not just in the energy field, they were a broad range: smoking cigarettes, flying in an airplane, riding in an automobile, living in a hurricane or tornado area, living in an earthquake area. We asked people for each of these whether they regard it as a high risk, a moderate risk, or a minor risk. Cigarette smoking came in first as high risk, with 61 percent saying it was a high risk. Living near a nuclear power plant was second, both before Three Mile Island and after it. It was more cited as a high risk after Three Mile Island than before, which is not surprising.

What was surprising to me was that so few more cited it as a high risk. It rose from 37 percent saying it was a high risk six months prior to Three Mile Island to 45 percent saying it was a high risk a month and a half after Three Mile Island. It was second, but it was well down from smoking cigarettes, which suggests that people took a somewhat calm view of it. On the other hand, it got a substantially higher high-risk factor than flying in a plane or riding in an automobile, both of which produced noticeably more deaths in the last year or two. So people are worried about it beyond its incidence of death to date, but they don't put it way up at the head of the ladder in terms of risk.

We specifically brought up the Three Mile Island subject and postulated that some people felt safer because with everything that went wrong, nothing really that serious happened, and that was reassuring to them. To other people it must have seemed that we came awfully close and were lucky that the whole thing didn't blow apart. I indicated that people were reasonably strong for nuclear power. That does not mean they are comfortable with it. When we asked did you feel reassured or worried, 52 percent were more worried by Three Mile Island and only 13 percent felt more secure.

We have just received the results of a new study in which we asked people to tell us how concerned they were about various sources of radiation, and this was not just nuclear energy. We had about six different things ranging from TV sets to microwave ovens to ultrasonic fetal monitoring to nuclear power plants and the storage and disposal of nuclear waste.

Now, there's been some mention of the fact that it's the sort of holocaust aspect of nuclear power that scares people -- the meltdown. These data surprised me and cast some doubt on whether that is the public's chief concern because the first thing that people were "very concerned" about was storage and disposal of nuclear waste, which I don't think has quite the meltdown kind of qualities that a nuclear power plant does. Some 55 percent expressed themselves as very concerned about storage and disposal, another 21 percent moderately concerned, for a total of over three-quarters.

Only 38 percent said they were very concerned about nuclear power plants and another 27 percent moderately. It was well below the storage and disposal of nuclear waste, in terms of concern.

In view of what Dr. Morgan said, I think it is interesting that medical x-rays were way down; they were in third place, but with less than half as many feeling very concerned about them. Ultrasonic fetal monitoring was next at 12 percent, dental x-rays 11 percent, microwave ovens at 10 percent and TV sets at 5 percent.

I think that the reason for these disparities is probably several things. One is people have lived with television sets and haven't seen very many of their friends drop in front of the tube, and they've all had dental x-rays and not too many of their friends have expired from them. But the nuclear power plant and the nuclear waste disposal is much more an unknown quantity.

We also proposed several different things that could be done -- this was in the Three Mile Island context -- to reduce the risk of nuclear power plants, and we asked people whether they favored or opposed each of these things. We started with the mildest things, the ones we thought practically everyone would favor, and then worked up to the most drastic ones. The first item we asked about was stationing federal inspectors in all nuclear power plants on an around the clock, 24-hour basis. The ones that followed covered: letting a nuclear power plant be built within 50 miles of a heavily populated area; letting all residents within 50 miles of a proposed site vote on whether to have the plant or not; closing all existing nuclear power plants until all equipment and procedures can be reviewed and improved; then, getting more and more severe, not permitting any more new nuclear plants to be built ever; and finally, closing all existing nuclear plants permanently.

It was very interesting. The one I thought would get the strongest vote, the stationing of federal inspectors, didn't; it came in third. There was strong sentiment for not building a plant within 50 miles of a heavily populated area: 79 percent favored that, only 15 percent opposed it. Close behind was letting the residents within 50 miles of a proposed site vote: that was 75 percent to 18 percent. Stationing the federal inspectors was a 3 to 1 thing: 69 percent yes, 23 percent no. Closing all existing nuclear power plants temporarily until procedures and systems could be reviewed and improved: 50 percent to 40 percent.

Now, here are the two interesting ones to me. Five weeks after Three Mile Island not letting any more new nuclear power plants be built was rejected 2 to 1, and closing all existing nuclear power plants permanently was rejected 5 to 1. Clearly they want something done, as shown by the strong sentiment for the first three or four, but short of stopping it. This is for the majority; I don't mean that everyone agrees on that.

We have another question that we asked on what things the public should decide and what things expert groups should decide. We pointed out that the public votes on the kind of zoning laws they're going to have in a community, but they don't vote on what the safety regulations for commercial airlines will be. That's the Civil Aeronautics Board. Then we went to a number of other things. And the public was highly selective. They thought, for example, that they should decide who the judges would be and what should be taught in schools, and where atomic power plants should be built, by a narrow margin; 52 percent thought the public should decide that, 41 percent the experts.

They reversed that on how atomic waste should be disposed of. That came out 2 to 1 that the experts should decide it. Incidentally, somewhat to my surprise they decided that the experts should decide what highway speed laws should be, not the voting public.

People's knowledge of nuclear is, I would say, fair. We asked people to their knowledge, what percent of the nation's electric power is supplied by nuclear. Forty-six percent said they didn't have any idea; the other 54 percent came up with a median figure of 20.5 percent, which is above what it is but not substantially above what it is.

In another question that again did not focus on nuclear but dealt with it, we asked people whether they thought they understood each of the subjects we asked about pretty well, understood some but not all that much, or didn't understand much about the pros and cons of nuclear power: 21 percent feel they understand it pretty well; a third feel they have some understanding; more than a third feel they don't understand anything about it; and then 6 percent never think about it.

In this next and last one, we gave people a whole list of things that ran all over the lot like the SALT-II Treaty, what's going on in Iran, which products are safe or unsafe to use, the 1980 presidential election, the gasoline shortage, the activities of President Carter's family and so forth. We asked: Are there any things on this list that you'd like to hear less about than you now hear? On a political note, you may be interested to know what heads the list. The activities of President Carter's family is first, with 56 percent wanting to hear less about it; the activities of Governor Jerry Brown is second; the activities of Senator Edward Kennedy is third. Then we dropped down into other things -- nuclear power, only 13 percent want to hear less about it than they're hearing. We then took the same list and said: Are there any that you'd like to hear more about than you're hearing? Amy Carter got 4 percent, Jerry Brown got 5 percent, Senator Kennedy got 13 percent, nuclear power got 33 percent. So there is a feeling of inadequate knowledge about nuclear power and a desire for more of it. That sort of sums up where the public sits at the moment.

KOSHLAND: Would any member of the Panel like to comment?

WEBSTER: I wish you had included in the nuclear question about whether you want to live near a nuclear power station a similar question about coal-fired stations. How many people would feel they wouldn't want to live close to such a station? Maybe you did ask that.

ROPER: We did ask about chemical manufacturing plants, and it didn't come out as badly as nuclear, but it came out pretty badly. I don't have that particular figure with me. We used the full list prior to Three Mile Island and then immediately afterward. But in the repeat we did this summer we just took about half a dozen of these things, and it did not include that one.

MORGAN: I was interested that so many were concerned about radioactive waste disposal. I would have put that much lower on my own list had I gotten one of these questionnaires. But I'm looking at a report from Dr. Medvedev, and in reference to the accident in the southern Urals. I don't know how accurate it is, but it indicates that this accident evolved from an explosion from nuclear waste that was stored underground; that thousands of miles were badly contaminated apparently; and that several hundred people died of radiation sickness. Now, I'm not sure when the accident occurred, but if this many did die, then one would anticipate that several thousand would die from radiation induced cancer integrating over a long time. So I think we would all be very interested to hear more about this accident in the Soviet Union. If the National Academy of Sciences or any other group has the ability to get this information, I would sincerely hope they would do all they can to make it public so we can profit by the one accident of this sort that seems to have occurred in association with radioactive waste.

KOSHLAND: My guess is it's all in some CIA photographs of that area, but whether we can unleash these I don't know.

Do I understand that the figures you gave, Mr. Roper, indicate that the public is quite sophisticated in terms of assessing sort of the balance; that is, they're worried, they sort of feel that we have to go ahead with nuclear power, they think the hazards are severe, they would love somebody else to have the nuclear power plant in their backyard. Is that right?

ROPER: I don't think they'd love it, but they'd prefer it.

KOSHLAND: They'd prefer it to having it themselves, so as long as somebody else --

ROPER: No, I think that's a cynical way of looking at it. I think they don't want it in anybody's backyard.

KOSHLAND: If they could avoid it.

ROPER: Get it away from populated areas.

KOSHLAND: I see. They want it in a nonlocalized areas as far as a lot of people.

ROPER: Well, you can't get a place where nobody lives, but you can get it where darned few people live is their thesis.

KOSHLAND: Well, that's rather reasonable I think. Now, the figures that we've had so far indicate, as we said -- and those levels were fairly high -- Dr. Webster was using the exposure of a million workers to 10 rems; the Nuclear Regulatory Commission limit is 5 rems. Is that right at the moment?

WEBSTER: This was over a 10-year period; my 10 rems. It was 1 rem per year, which is the average level of worker exposure, roughly.

KOSHLAND: So you were taking the average level of worker exposure for somebody living 10 years.

WEBSTER: Well, I actually used 10 years at a half a rem and got 5 rems.

KOSHLAND: Now, if that figure is 1 percent of the cancer rate, which I think most people would think is rather low, do you think that's what the public thinks it is, or do you think they think it's much higher than that?

ROPER: I think they think it's higher than that. I think the fact that they're more concerned about nuclear radiation, for example, than medical x-rays, if the figures we've been hearing today are right, they should be the other way around. So I think that's got to mean that they think there's a much higher death rate or potential death rate from nuclear power than from other kinds of radiation. But they don't think it's the most serious risk there is.

WEBSTER: Mr. Roper has shown that most of the public believe smoking to be the greatest of the hazards producing cancer. His data also show that the public perception of low doses of radiation as a cause of cancer is greatly exaggerated relative to the smoking risk. A simple set of statistics will provide a more realistic comparison. Informed medical opinion holds that about one-sixth of all cancer deaths are smoking-related. Of 10,000 persons there will be about 1700 who will die from cancer and therefore of these deaths about 300 will be related to smoking. Using the linear estimate of cancer mortality risk given earlier, 1 rem of radiation will be expected to produce eventually 1 fatal cancer case in these 10,000 persons. Dr. Morgan's figure is about 3 times higher. Thus the average smoking habit carries a risk between 100 and 300 times greater than the risk of fatal cancer from 1 rem of radiation. Using similar methods, automobiles are about 50 times more likely to cause death than 1 rem. I believe that very few members of the public would place the radiation risk anywhere near this low relative to smoking or driving.

KOSHLAND: Now, there's a situation of airplanes perhaps compared to automobiles that might be analogous. That is, if you take the death rate from airplanes per year over the year, it probably is lower than for automobiles in some calculation of miles. But whenever a big plane crash occurs, 500 people get killed or something of this sort. Is the public much more concerned about an accident that gets thousands of people once every 20 years than a steady attrition of a small number of people over the years?

ROPER: I can't document this, but certainly there are not quite twice as many people who described driving an automobile as high risk as described flying in a plane as high risk. Certainly the death rates from the two machines are much greater than a 2 to 1 ratio. I can't prove what I'm about to say, but I think it is a fact that people feel reasonably confident that if they're in an automobile accident they've got a pretty good chance of coming out of it, and if they're in a plane crash they have very little chance of coming out of it. I think it's the sort of "the jig is up" quality to it, and I think there's got to be some of that in the nuclear thing.

KOSHLAND: Okay. Why don't we throw this open to discussion? I'll give priority to people who haven't asked questions before.

ROBERT CHEN: I have a question, I guess, about consistency. That was brought up a lot earlier but it bears on the perception issue. We've heard a lot of figures comparing the cancer incidence rate from radiation with natural cancer rates, and the factors are in the thousands to 1 from what I gather. What I'd like to know is that especially given recent revelations about things like the military conducting tests which released radiations to huge numbers of Americans without their knowledge and presumably without scientists' knowledge, what is the possibility that a high cancer rate 30 years later is, in fact, due to radiation such as the fallout, or due to synergisms between some low level of radiation and the increasing amount of environmental cancers, or chemically caused cancers, such as Dr. Morgan mentioned early on? That's one question.

But it bears on the perception question directly because to me, the whole question of things like the Americans not knowing that they were exposed 30 years ago to Nevada tests or whatever bears on the credibility of scientists and engineers in general. And basically, what would you say to the proposition that people, as was mentioned, have good reason to desire insurance because there are huge uncertainties in the nuclear area, whereas they might be more prone to accept dangers from coal which they've seen, say, for the last 100 years? To me, being a scientist, I would put a big risk factor on the statements of scientists and people with whom they're associated; a lot of the nuclear people were military people originally. How would you react to that sort of uncertainty?

KOSHLAND: Could we have Dr. Morgan answer the question of the bomb tests and the fallout, and maybe Mr. Roper the credibility of scientists.

MORGAN: Well, as all of us know, there are many cases in court claiming damage from this fallout. We have the situation of people in Utah who feel that there's an increased incidence of leukemia and perhaps other malignancies related to the dose they got from weapons fallout. We have the reports of persons present at Test Smokey; I have a preliminary report on this group showing an increase, presumably of statistical significance, in leukemia among those in the high fallout areas. We have cases coming into court now on behalf of the U.S. Marines that were the first to enter Nagasaki and seem to have a high incidence of cancer. So you'll hear much more about this, I'm sure, in the years to come because these cases are going into court where they are demanding settlement.

I feel that the implication of your comments is this: that the public mistrusts the establishment, they mistrust the scientists; I can only agree and add that the mistrust is for good cause. Eisenhower, who was probably more honest than many of our Presidents, is reputed to have indicated in substance that we should keep quiet and not worry the people about the risks of fallout from the tests in Nevada. When things like this go on, I think it's perhaps understandable that even scientists wonder if the risks maybe are 10 times or 100 times more than the published figures indicate.

ROPER: I'll answer the scientists' credibility part, but there was another aspect of what you said that I want to comment on. Scientists are held in about as high regard as any occupational group there is. At the same time, I think that people have had so many surprises over the last 10 years from various sources that their skepticism is heightened for all sources. So while they respect scientists, generally have a high regard for them, and find them credible, I don't think they necessarily blindly buy everything they say any more -- or even less -- than they do other groups.

The other thing I think you mentioned -- the acceptance of the risks of coal which they've been familiar with for 100 years -- is a very strong factor. There may well be unawareness of the extent of problems from medical x-rays even though they're familiar. But I think one of the reasons people don't assign them anything like the risk factor they assign nuclear is because we've all had them. And the thing I was saying earlier applies, very few of your friends died from an x-ray at a doctor's office. Very few of your friends died from a nuclear explosion, too, but that's because that's new and we really don't know that much about it.

LAND: Maybe I misunderstood the question, but I thought the first thing you asked was whether the exposures, these nuclear tests, are responsible for a substantial portion of the cancer rates today. The only cancers that I know of that have really been going up recently over the past few years have been lung cancer and bladder cancer. But we know the reason for that; it's smoking. I don't think there has been such a big increase in cancer that you could --

CHEN: But given that the latency period is not known, where is the high rate, 17 per 100,000, whatever?

LAND: I think it was 17 percent. I think that's just the human condition. It also has to do with living longer.

CHEN: But nobody really has an explanation, I think, for cancer rates. That's what I'm asking. What is due to radiation that nobody has known about? I thought not many people knew about that fallout.

LAND: Certainly the cancer deaths before 1945 weren't due to unknown radiation.

KOSHLAND: Isn't part of the problem that the number of people dying is 100 percent, and over the period of years, as fewer people die of pneumonia, more people are going to die of cancer?

MICAH SOLOMON: To refresh everyone's memory, Dr. Upton was speaking about the maximum or extremely severe nuclear accidents, not the Three Mile Island type accident, but an accident in which 3000 initial deaths would occur, 45,000 latent cancer deaths would occur, as well as thyroid problems and all sorts of things like that. He mentioned that this would only occur, according to the Ford Foundation, once in a very long time per reactor. Now, I went over this with Dr. Upton at intermission, and it turns out that the same report stated that they believe that by the year 2000 the chance of an extremely serious accident with thousands, tens of thousands, of nuclear-related deaths would be 25 percent. I don't know how all you on the Panel can say in light of that that public fears of nuclear power are unresponsible or unreasonable.

UPTON: This is, in fact, a statement that appeared in the Ford Foundation study. The report stated that there was an estimated chance of one such catastrophic accident every 200 million reactor years. Then the Ford Foundation study indicated that there could, in fact, be a 500 times greater risk, if one could not accept the WASH-1400 number as an entirely credible number. In other words, one could conceive of the possibility that the risk might be 500 times larger, and this would mean one such accident every 400,000 reactor years. Hence, if there are enough reactors built and operating around the world, then within a predictable period of time one will expect to observe this estimated event, and as our speaker mentioned a moment ago, the report does cite a 25 percent chance of such a serious accident by the year 2000.

KOSHLAND: If you have one accident that kills 20,000 people every 200 years, then the answer to that question is that the people have a perfect right to say that this is really relatively safe. If that's one every year, that's another factor. Could you relate that to the energy needs of this country? If 50 percent of U.S. energy is provided by nuclear power, how does that relate to the 200 million reactor hours?

UPTON: I think the relevant comparison -- if you want to compare the total health impact from nuclear energy and an alternative energy system such as coal -- was the comparison that Dr. Vagelos gave us. Taking into account such a maximum credible accident and multiplying by 500 its probability as estimated in WASH-1400, the Ford Foundation study inferred that one might get up to 10 deaths per 1000-megawatt-year electric with nuclear power; whereas, with coal, one would be getting in the range of 25 to 100, depending on the system, whether it was a very clean system or not such a clean system.

I guess the issue is whether you accept these deaths distributed over the population at large, and accept the calculation as realistic and the comparison as reasonable, or whether because they're bunched together they now become very much more apparent, serious, less acceptable. That's a value judgment, not a scientific question: it is a philosophical and value judgment one must make.

ROPER: I'd just like to make a comment on the unreasoned fear part of that comment. The point I would make is whether it's a thoroughly reasoned fear or a wholly unreasoned fear; either way, I don't care. It's got to be taken into account in establishing a public policy because it's real whether it's justified or isn't justified.

KOSHLAND: I'm going to ask the young man who asked the question earlier to come forward. Has this discussion drifted along the line you wanted or not?

BATES: No, I'll try and restate it. By way of introduction, I'm the Project Director for the Honicker Petition which is asking this question among others of the Nuclear Regulatory Commission at this time, and will ultimately be asking this question of the Supreme Court. I think there are probably some people representing the Nuclear Regulatory Commission here who would be delighted to hear an answer to this that they could use. The question is: What is the process, what is the mechanism, for assessing the acceptability of risks, and is this process scientific or political, and is it democratic? And considering that you're giving risks to persons in future eras, is this ethical?

KOSHLAND: Who'd like to answer that? Dr. Upton?

UPTON: I'd like to begin an answer and to do so in the context of the question that was asked before. I think for many people, the spectre of a catastrophic event -- even if one can average the consequences of the event and in terms of averaging make the comparison between nuclear and coal look satisfactory -- the possibility of a catastrophic event by itself is abhorrent. One of the things that the Ford Foundation study recommended was that by appropriate siting, one could, in fact, greatly mitigate the catastrophe, the early deaths and the early radiation sickness.

But I think that the issue as to how one determines what is an acceptable situation must be a public issue, and the public must have as much information as possible and as objectively presented as possible, so that it can decide for itself what it will or will not accept.

One of the purposes of this discussion is, in fact, to try to get the issues out and to get information out so that we can see it clearly and understand it.

KOSHLAND: This question has come up in a number of Forums. It is a very important one, but the answer is simple, and I don't think scientists would disagree with it. The ultimate power in a democracy rests with the people, who always will make the final decision. We were discussing previously the question of credibility. I think there are people on one side saying that nuclear power is extremely dangerous and that everything should be stopped immediately; they are getting a lot of publicity. There are people on the other side who say that things are absolutely safe and ask why everybody is fussing. Also, there are people all the way in the middle.

Just because people are extreme doesn't mean they're wrong. If one says 2+2=4 and another says 2+2=5, it does not follow that 2+2=4 1/2. I think that we see so many commercials that we tend to discount everybody. I have great faith that in the long run, the facts will prevail. Mr. Roper was saying that the public seems to be taking sort of a cynical attitude toward the extremes and following a skeptical, slow procedure. I think this will be reflected in the votes of the people eventually. That's a rambling answer to your question, but I think the Nuclear Regulatory Commission cannot impose something that the voters really don't want.

MILTON CHASE: Along the lines of your last comment, my question is directed to Mr. Roper. From the statistics you quote, it seems as though the public at large is taking the problem of nuclear energy not in a relaxed way but not in an hysterical way.

On the other hand, there are people whom I won't characterize as being hysterical but very much concerned, very emotional about it, prepared to get involved in being arrested and doing whatever they think is appropriate in order to stop nuclear activities from going on.

Have you attempted to probe what are the motivations of the people who are so concerned about nuclear energy? What are their concerns? Are they the concerns of safety or are there other matters involved?

ROPER: No, I can't say we have. I can't imagine exactly what they would be if they weren't safety, though. There are a lot of ramifications to safety.

CHASE: The concern with nuclear being tantamount in our industrial society to requiring a change back to a simpler society would be one significant reason that's frequently mentioned.

ROPER: The reason I measure public opinion is because I can't predict it very accurately. I'm wrong about half the time. I said, for example, that I thought the stationing of guards around the clock would come out first and it came out third. So with that caveat, nevertheless, I'd be very surprised if the primary reason weren't safety, but I cannot answer it.

KOSHLAND: One scenario that I thought of is that the people who are very concerned about nuclear disaster in their neighborhood is not that their safety is really involved because the chances of getting a large number of rems are only high if you stay around for a full day and that's unlikely. But they see the following problem: I've lived in this neighborhood all my life, my children have grown up here, my relatives are in the neighborhood. There's a real chance that even if I'm not hurt I will have to abandon my home for 20 years and so forth. That's nothing to do with safety; that's expropriation of property.

ROPER: It does have to do with safety. It's because of the inherent unsafeness of it, or the perception of an inherent unsafeness, that would result in their having to move out. The risk to the unborn is a safety thing. Safety is a big umbrella.

WEBSTER: I would like to say something about the ethical problem, which bothers me. I have concluded that the hazard of the automobile is considerably greater than the hazard of nuclear power, even under the circumstances of these maximum credible accidents with the vast release that Dr. Upton has talked about happening every so many years. We are killing a lot of people now with automobiles, and it seems to me that there's a private ethic involved in this. We buy automobiles, and yet we know there is a definite chance that we will kill somebody with them. You have to put that kind of consideration, a broader consideration of ethics, into the discussion.

BATES: I think the other concomitant that has to go into that equation is that one can withdraw within the society to a point where one is not infringed upon by the automobile. It is possible to do that. It is also possible to lower your risks by not driving an automobile.

KOSHLAND: I think that's a little bit of an analogy. You can always move to a place where there isn't a nuclear reactor.

BATES: I'm not so sure.

KOSHLAND: But a lot of people have to go to their jobs. I can say that 55 miles an hour is imposed on me; I'd prefer it was 25 miles an hour and there would be no risk. I think there's a good analogy there, and we do make laws that everybody has to sort of take equal risk at a certain point.

BATES: The other problem with the analogy is that if 55,000 people a year are going to die in automobile accidents that is certainly a higher rate than we're experiencing for nuclear power. However, can we equate those two? Can we equate the acceptability of automobiles and the acceptability of nuclear power? Can you balance the two equally on a similar scale?

WEBSTER: The missing part of the equation is the benefit that you perceive getting from the use of the automobile on the one hand, and nuclear power on the other. So I think you have to consider the social benefit before you can condemn the modality, before you can condemn the automobile or the reactor. You have to put those two things together.

KOSHLAND: I regret that we must now formally close the proceedings. On behalf of the Academy Forum I wish to thank all of you who have participated.