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PROCEEDINGS OF THE SECOND INTERDISCIPLINARY CONFERENCE ON SELECTED EFFECTS OF A GENERAL WAR

JULY 1969

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Proceedings:
SECOND INTERDISCIPLINARY CONFERENCE
ON SELECTED EFFECTS OF A GENERAL WAR

VOLUME II

This Conference was sponsored by the Defense Atomic Support Agency (Contract DASA 01-67-C-0024, NWER Subtask DB003) through the auspices of the New York Academy of Sciences Interdisciplinary Communications Program. It was held at Princeton, New Jersey, during 4-7 October 1967.

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SUMMARY

The Second Interdisciplinary Conference on Selected Effects of a General War was held at Princeton, New Jersey from 4-7 October 1967, under the auspices of the New York Academy of Sciences Interdisciplinary Communications Program, with the support of the Defense Atomic Support Agency. The first of this series of conferences was held from 18-21 January 1967 and dealt chiefly with the effects of the atomic bombing of Hiroshima and Nagasaki.

This second conference was concerned mainly with the effects of fallout or other release of radioactive materials from subsequent tests or accidents involving nuclear weapons. The specific effects discussed extensively included the effects of the 1954 H-bomb test in the Pacific ocean which resulted in radioactive fallout contamination of Marshall Island natives and of the Japanese fishermen on the vessel Fukuryu Maru (Lucky Dragon); the ecological effects of bomb tests in the Pacific ocean test regions; and the effects of the "Spanish incident," which involved the accidental dropping of four nuclear weapons, without detonation but with release of radioactive material (plutonium) onto Spanish soil as a result of accidental destruction of an airborne bomber.

Representatives of many disciplines engaged in vigorous and free-wheeling discussion and debate of all aspects of these incidents. The disciplines represented included, among others, physics, weapons technology, military science, ecology, epidemiology, radiation biology, toxicology, pathology, psychiatry, genetics, other biologic and medical specialties, and pertinent administrative and cultural specialties.

In addition to discussion of the physical characteristics and extent of the radioactive contamination, the radiation doses, the monitoring and decontamination procedures, the biological, medical, psychological and sociological effects of the radioactive contamination upon the people and locales immediately involved, the discussions extended to broader and farther reaching psychosocial aspects, i.e., to the chains

of circumstances and events leading from these localized incidents through the news media and diplomatic channels to the reactions of the more complex social structures, such as the economic, political, and diplomatic repercussions of national and international scope.

There was much discussion of possible reasons for differences in reaction to incidents of these kinds among different nations; the importance of seeking answers to such questions in the differences in culture, as well as in politics, was stressed.

On the basis of the discussion of the specific incidents and their consequences, the conferees roamed the whole field of psychosocial and biomedical implications of nuclear warfare in an attempt to project the consequences of nuclear warfare under a variety of conditions with respect to magnitude of the warfare, anticipation of onset, preparedness, and civil and military defense policies. Interest was focussed upon policies and means which might help to prevent or to mitigate nuclear warfare, upon the nature, scope and consequences of nuclear warfare should it occur, and upon the problems of national recovery after nuclear warfare.

The participants of this conference included Dr. Frank Fremont-Smith, director of the New York Academy of Sciences Interdisciplinary Communications Program; the two co-chairmen of the conference, Dr. Austin M. Brues, and Dr. Arthur C. Upton; the discussion initiators for the five major subjects on the agenda, Dr. Charles L. Dunham (the 1954 thermonuclear test), Dr. Robert A. Conard (the effects of fallout on populations), Dr. Lauren R. Donaldson (ecological aspects of weapon testing), Dr. Wright H. Langham (the Spanish incident), and Dr. Merril Eisenbud (discussion of psychosocial reactions); and others listed on the following pages.

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TABLE OF CONTENTS

SUMMARY	iii
PARTICIPANTS	v
LIST OF ILLUSTRATIONS	ix
LIST OF TABLES	xiii
SESSION I	
INTRODUCTORY SESSION	
OPENING REMARKS	1
SELF-INTRODUCTION	9
SESSION II	
THE 1954 THERMONUCLEAR TEST	
INTRODUCTION	35
RADIOACTIVE FALLOUT AND RADIATION EXPOSURE	38
THE FUKURYU MARU (LUCKY DRAGON) AND THE PROBLEMS IN JAPAN	56
SESSION III	
THE 1954 THERMONUCLEAR TEST (Continued)	
THE MARSHALL ISLANDS PROBLEM	95
IMPLICATIONS FOR DEFENSE POLICIES	157
SESSION IV	
ECOLOGICAL ASPECTS OF WEAPON TESTING	
INTRODUCTION	171
RADIOACTIVE CONTAMINATION OF PACIFIC REGION	173
SESSION V	
THE SPANISH INCIDENT	
INTRODUCTION	233
DESCRIPTION OF THE ACCIDENT	234

TABLE OF
CONTENTS (Continued)

THE SEARCH FOR THE LOST H-BOMBS 237

RADIOACTIVE CONTAMINATION AND DECONTAMINATION 239

PSYCHOSOCIAL, ECONOMIC AND POLITICAL ASPECTS 245

SESSION VI
PSYCHOSOCIAL REACTIONS

INTRODUCTION 277

ATTACK DAMAGE AND PROBLEMS OF POST-ATTACK RECOVERY 303

SESSION VII
PSYCHOSOCIAL REACTIONS (Continued)

PROBLEMS OF POST-ATTACK RECOVERY 345

REFERENCES 397

LIST OF ILLUSTRATIONS

FIGURE NO.	TITLE	PAGE
1	Map of fallout area, Marshall Islands, March 1, 1954. From R. Conard. (Courtesy Annals Int. Med.).	96
2	Rongelap Village as it was in 1954. From R. Conard.	98
3	Rough sketch showing fallout deposition. Wavy areas represent gamma radiation, and stippling represents beta radiation. From R. Conard.	98
4	"Beta burns" of neck (subject No. 39, March 1954). The area over the thyroid was a frequent site of "burns". From R. Conard.	101
5	Marshallese bathing in lagoon at Kwajalein in March 1954, to decontaminate skin and hair after fallout contamination. From R. Conard.	102
6	Rongelap village today. From R. Conard.	103
7	Radiation syndromes (schematic presentation). From R. Conard.	105
8	Schematic graph showing major blood changes and clinical signs for radiation doses where survival is possible (200-600 rads). From R. Conard.	107
9	Numerous superficial "beta burns" of the skin of a young boy who was wearing little clothing at the time of exposure. From R. Conard. (Courtesy Annals Int. Med.).	110
10	"Beta burns" of the feet. From R. Conard.	110
11	Epilation in the temporal area of the scalp of a young girl. From R. Conard. (Courtesy Annals Int. Med.).	111
12	Benign nevi (moles) that developed 8 years later in area of "beta burns". From R. Conard. (Courtesy Annals Int. Med.).	117
13	Scarring of ear from "beta burns". From R. Conard.	117

FIGURE NO.	TITLE	PAGE
14	Permanent alopecia in Japanese fisherman. From R. Conard.	117
15	Comparison of skeletal age and chronological age (1961 & 1962 pooled data). From R. Conard.	119
16	Depression of blood elements in Rongelap people. Percent depression compared with average counts of unexposed people. From R. Conard.	119
17	Benign thyroid nodules at surgery. Arrows point to nodules. From R. Conard.	126
18	Sectioned thyroid gland showing multinodular, cystic and hemorrhagic nature of the gland. The nodules were benign. From R. Conard. (Courtesy New England J. Med.).	126
19	Skeletal age development before and after hormone therapy in two boys showing greatest growth retardation. From R. Conard. (Courtesy Annals Int. Med.).	128
20	At right, a 12-year old boy with greatest growth retardation; at left is his brother, a year younger. From R. Conard.	135
21	Same boy with retarded growth as shown in Figure 20; at left, before thyroid hormone treatment, and at right 6 months after treatment began. From R. Conard.	135
22	Map of the Pacific, showing locations of major study sites for the evaluation of radiological contamination. From L. Donaldson.	172
23	Scale map of the United States superimposed over area of the major test sites in the Pacific Ocean. From L. Donaldson.	174
24	Photograph of portion of outer reef at Eniwetok Atoll, the site of numerous weapons tests. From L. Donaldson.	176
25	Proposed track of the U.S.S. Walton, June 1956. From L. Donaldson.	181
26	Actual cruise track of the U.S.S. Walton, June 11-21, 1956. From L. Donaldson.	181

FIGURE NO.	TITLE	PAGE
27	Atomic explosion, Eniwetok Atoll, Marshall Islands. From L. Donaldson.	185
28	Blast damage at Rigili Island, Eniwetok Atoll. From L. Donaldson.	185
29	Thermal damage to vegetation at Rigili Island, Eniwetok Atoll. From L. Donaldson.	186
30	Bird flight over the reef at Eniwetok Atoll. From L. Donaldson.	186
31	Injured bird on beach at Rigili Island, Eniwetok Atoll. From L. Donaldson.	187
32	Catching injured bird on beach, Rigili Island, Eniwetok Atoll. From L. Donaldson.	187
33	Noddy tern with burned wing and tail feathers, Rigili Island, Eniwetok Atoll. From L. Donaldson.	187
34	Dead terns on sandspit following atomic blasts, Rigili Island, Eniwetok Atoll. From L. Donaldson.	188
35	Distribution of fallout radioisotopes on Rongelap Atoll as determined by a survey in 1961. From L. Donaldson.	191
36	View of islet near a test site, Eniwetok Atoll. From L. Donaldson.	193
37	Photograph and radioautograph of soil showing correspondence of radioactive portions to organic matter. From L. Donaldson.	193
38	Photograph and radioautograph of Messerschmidia stem with radioactivity in the outer, growing layers. From L. Donaldson.	193
39	Radioactivity of plankton in thousands of disintegrations per minute per gram following atomic tests at Bikini and Eniwetok Atolls. From L. Donaldson.	194
40	Shadow-shield whole-body counter (gamma spectroscopy). From R. Conard.	206
41	Typical gamma spectrograph of Marshallese subject before (March 1957) and after (March 1959) moving back to Rongelap Island. From R. Conard.	208

FIGURE NO.	TITLE	PAGE
42	Estimated body burdens of isotopes in Rongelap people at various times since 1954. Values obtained either by gamma spectroscopy or by radiochemical urine analyses. From R. Conard.	209

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PROCEEDINGS OF THE INTERDISCIPLINARY CONFERENCE ON SELECTED EFFECTS OF A GENERAL WAR (2ND). VOLUME II

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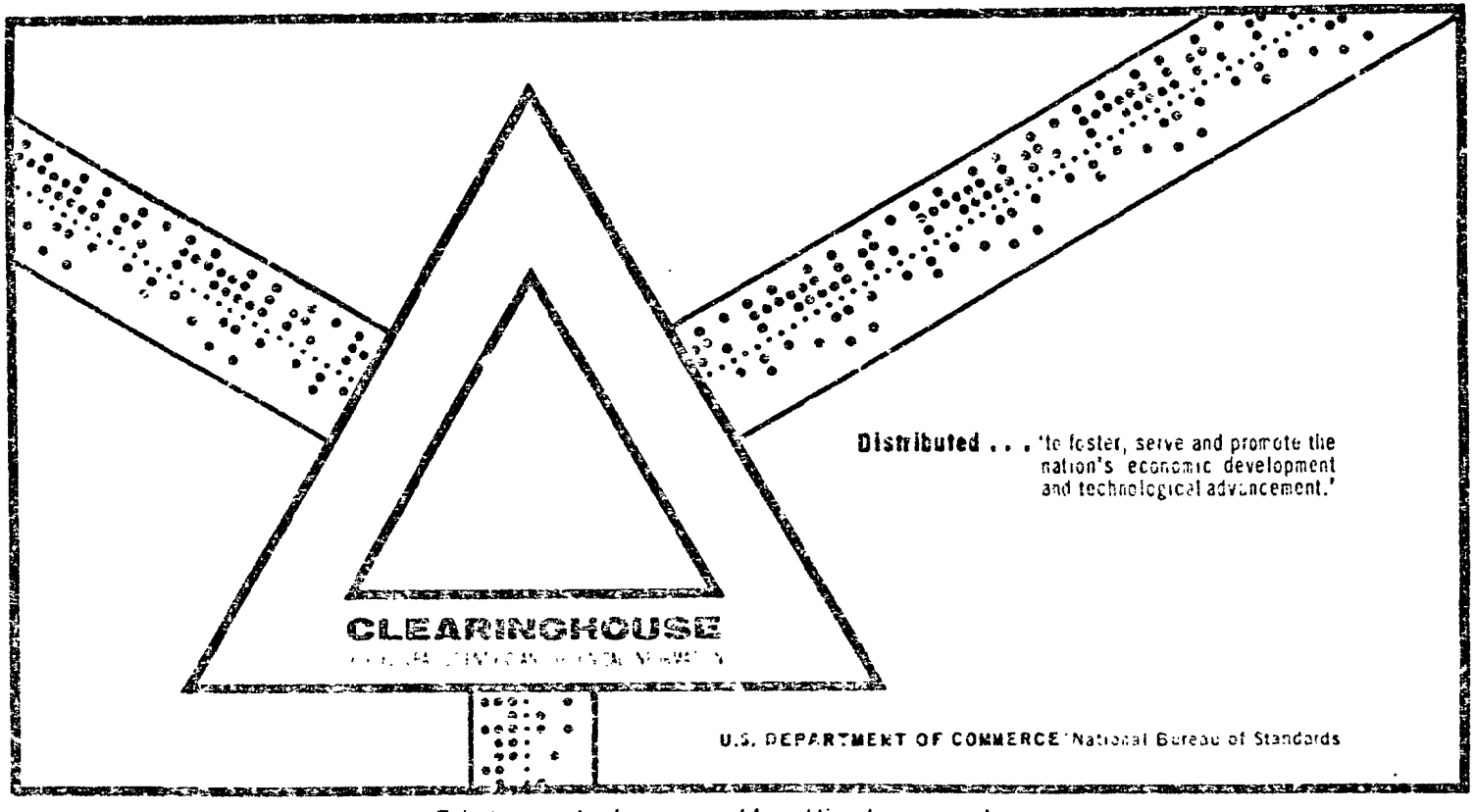

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41446

LIST OF TABLES

TABLE NO.		PAGE
1	Summary of fallout effects (Marshall Islands). From R. Conard.	96
2	Estimated body burden of Rongelap people (μc) From R. Conard.	100
3	Thyroid nodules (including hypothyroidism) in Marshallese populations. From R. Conard.	124

SESSION I
INTRODUCTORY SESSION

SESSION I
INTRODUCTORY SESSION

OPENING REMARKS

BRUES: This is the second of a series of conferences on the long-range biomedical and psychosocial effects of nuclear warfare.

The first conference, the proceedings of which are published in DASA 2019-I, dealt with the situation that occurred following the bombings of Hiroshima and Nagasaki. In continuing the general subject of the series, we have selected for discussion at this second conference certain incidents of a different nature.

We have a wide variety of disciplinary participation in this conference and hope that we may be led into many aspects of nuclear warfare in connection with the subjects under discussion. Dr. Arthur Upton and I are acting as co-chairmen of this series owing to the reluctance of either of us to assume the full responsibility, in view of our frequent unavailability between conferences, at least that is what we have found.

FREMONT-SMITH: And I have found it with respect to both of you! [Laughter]

BRUES: I would like to ask if Dr. Upton wishes to make a few remarks.

UPTON: Thank you, Austin. I really have little to say except to voice my pleasure at being here again and seeing gathered here esteemed and admired friends and colleagues.

When I first heard from Dr. Fremont-Smith and Dr. Brues about this series of meetings and was asked to help as co-chairman, I had very mixed feelings. I felt the issue was one of such overwhelming importance that anything I could do to help deal with this issue would be effort probably well spent. At the same time, the issue is one of such serious importance that I worried about my ability to make a dent in the problem.

I can only say that coming here again today and looking around the room and seeing the people who are also here fills me again with hope that perhaps out of this discussion something may, indeed, be accomplished. I hope, as co-chairman, that I may be able to serve as a little bit of a catalyst in helping the meeting to be a success.

BRUES: I think it is to be anticipated that at this informal meeting both the medical and social aspects of the subject will be under discussion concurrently.

I would like next to hear comments from Colonel Hemler, who actually is the one who developed the idea of having this series of conferences.

HEMLER: Thank you, Dr. Brues. Actually, you attribute too much to me, in view of the efforts of two other members of this conference, Dr. Taylor and Dr. de Boer. As I mentioned last year at the first conference, I was more or less a catalyst between those two men.

The conference idea was actually a series of ideas developed by Dr. de Boer in Albuquerque, and by Dr. Taylor, who was, at that time, the Deputy Director for Science Technology at the Defense Atomic Support Agency in Washington, D.C. Dr. de Boer caught me unaware when I was on a trip in Albuquerque, and approached me with the idea of talking to Dr. Fremont-Smith about his Interdisciplinary Conference Program. At the same time, Dr. Taylor had considered the problem that we are faced with today, a problem which is probably more far-reaching today than at the time of the First Interdisciplinary Conference in February 1967. The problem is that of the vast numbers of people around the world who fall generally into one of two extreme camps: those who believe that the "on the beach" philosophy exists with respect to nuclear warfare, and those who believe that this is simply another weapon on the battlefield that can be used with relatively few long-range aftereffects. If I can steal a little bit of your thunder, Dr. Brues, as I remember, you said at the first conference that the answer probably lies somewhere in between.

The purpose of the conference, then, is to gather some of the people together from the several disciplines in order to exchange some of the specialized knowledge that we all have, in hopes that perhaps we can arrive at some conclusions as to exactly where the

in-between is and, if we cannot arrive at the conclusions, that perhaps we can pin down the questions more definitely.

I won't take much more time here. I would like to mention that we, in DASA, are still firmly for the idea of the conference series. However, I am sure that all of you have been reading in the papers about the budgetary problems that the Government is having at this time.

Even with this type of budgetary problem that faces us right now, we definitely will go for a third conference. It is hoped that we can continue on to complete the series. If not, we would actively support some other Governmental or non-Governmental agency in continuing with the series because we feel that it is extremely desirable and necessary.

The first two conferences serve primarily to acquaint us with the facts that have occurred in the past. The succeeding conferences are designed to try to extrapolate from these facts, plus a few assumptions, and to see how we can make these meld with the questions and the conclusions regarding long-term effects of general nuclear war.

BRUES: I assume from this you are not suggesting that it is appropriate for us to discuss here where the Government should put its money. We would probably have little influence in this matter! [Laughter]

HEMLER: I certainly agree there. We're not even sure that we have any influence! [Laughter] Perhaps I should say we are sure we do not have any influence.

BRUES: Dr Fremont-Smith, we would like to hear some discussion from you.

FREMONT-SMITH: Well, I also want to welcome all of you, those of you who have been here before and those of you who are here for the first time.

It is a delight to me to have the opportunity to facilitate our being here together. Some of you may remember that at the previous conference I went through the motions, which I like to do, of removing my coat. These are going to be informal conferences but there should be a bit of formality at the beginning. I usually get a smile out of that.

I think that Arthur said that we would hope that we would make a dent in the problem, but I think that one thing we can be sure of is that the problem will make a dent in us or on us, a big one, and I think it's quite necessary.

It's quite an assignment that we have before us, particularly in view of the fact that there have been so few nuclear wars for us to extrapolate from. However, we ought to be dented, I'm quite sure.

The conference pattern, for those of you who haven't been here before, is based on the kind of conference procedure that was developed in the Josiah Macy, Jr. Foundation, and I have been continuing that first in AIBS and now with the New York Academy of Sciences. This is to provide a forum for what does not ordinarily take place in scientific meetings, that it, a real discussion in depth among a group of people who come from very different backgrounds of training.

We believe we have made a great rediscovery and we are waiting for the Nobel Prize Committee to decide that Nobel Prizes are given for rediscoveries. They haven't shown any signs of doing this yet, but we are very patient. The rediscovery is that people communicate with one another not by speaking at but by conversing with. I am sure that all of you are aware of the fact that at most scientific meetings there is no conversation with the chairman as the discussion starts. At the end of the morning he says he hates to interrupt this fascinating discussion but unfortunately we are twenty minutes behind on our agenda and I must call on Professor so-and-so to make another statement at you. But a good active discussion in the form of bull sessions does take place in the corridors, in people's rooms and in the bar, and often this is the most rewarding part of scientific sessions. So, we thought, why not focus on the rewarding part? This is what we try to do here, that is, to bring the bull session point of view of the corridors and the people's rooms and the bar into the conference room. If anybody feels very impelled to make a speech at our conferences, we send him to the bar! [Laughter] That works not too badly.

Other than that, we believe in conversation and, as all of you are aware, the heart of conversation is interruption. So the mood of our conferences is, don't speak when I'm interrupting. This is the way it will be. I want to warn our discussion initiators that, in spite of the fact that one of our co-chairmen may have invited them to be prepared to talk for not more than half an hour, they will be interrupted.

It may happen at the earliest words that they say, or even before they start to speak, but we hope that they will be prepared for this and know that they can interrupt the interrupters, maybe.

The job of the discussion initiator is not to tell the others what he thinks they ought to know, but rather to evoke from them their questions in terms of what they need to know, because they know better than anyone else what their needs are. This is the essence of what we are after.

We are dealing, as all of you are even more aware than I am, with an information explosion. We are told—and the figures are not very accurate—that there are more than 20,000 articles published in journals in the bio-scientific field per year. The figure on the number of journals is very inaccurate at the moment, but they are showing up fairly rapidly. So, there are many, many journals, and many, many articles, and it's absolutely impossible for anybody to be up even on the narrowest field, or at least be sure he is up-to-date, because he can't be sure what's been going on in Argentina last week or what will be going on in Geneva next week which may bring out some information that he had no idea of at all.

BRUES: All I meant by my warning to the initiators, Frank, was that they should not have more than twenty or thirty minutes worth of gasoline in their tanks! [Laughter]

FREMONT-SMITH: This is absolutely fine, but this gasoline is to be spread over what happens when you're driving in a crowded city. It will be a stop and go situation: bumper to bumper. I think we are twenty-two, maybe twenty-three people here. If each person here shook hands with everybody else once, who can quickly come up with a figure of how many handshakes would have taken place? It won't take you very long. Anybody?

AYRES: One-hundred and ninety if there are twenty people — $\left(\frac{20 \times 19}{2}\right)$.

FREMONT-SMITH: This gives you an idea of the number of possible interactions. If everybody spoke once, there would be many dialogues that would take place, and we hope that several of you will speak more than once. So, this is the opportunity, and it's really quite surprising how many interactions there are. Not all verbal, either.

Now, I think I ought to tell you that I have a secret weapon. If I get caught in an argument and find that I am being worsted—and this does happen sometimes—then I turn to my opponent and I ask him in the nicest voice, "Now, would you mind repeating your basic assumptions?" It's surprising how few of them will remember what their basic assumptions were. So I usually have them at that point. But I ought to confess also that one of my basic assumptions is that nature is all of one piece, and that with the mass of fragments of information pouring in from all the research outposts on the periphery of knowledge, it's going to become increasingly necessary for us to put time and energy into the re-integration of these fragments of information into meaningful wholes, meaningful in the sense that these wholes will have some bearing on the problems that mankind is faced with, whether in the sciences or in the social sphere or in artistic areas, also. This re-integration, to be meaningful, must be multi-disciplined.

I think that I might confess that I believe very deeply that the whole advance of science and of civilization requires an entirely different level of effort, time, and thought on communication, and especially on the multi-disciplined communication, and we're not organized for it. There are a few other conferences run along these lines but they are very few and far between, and it is relatively a small amount of intensive cross-disciplined communication that takes place either in our universities, in our federal agencies, or what have you.

I may have given the following illustration at the last conference, but if you don't mind I'll give it again. At the White House Conference on Education three years ago, I think, John Gardner was the chairman. I think it was just before he was made Secretary of Health, Education and Welfare. In fact, somebody said that he was made Secretary because he did so well at this conference. Anyway, at this conference on education, Dr. Gardner begged the conferees, the several hundred that were there—and I wasn't there—to take, as he put it, big barracuda bites of the problem of education because it was in such a desperate state. The reporter who wrote this up in the Saturday Review said that although some such interactions, vigorous interactions, on the problem may have taken place in the corridors, the community of educators was apparently not organized to enter into this kind of interchange, and that the White House Conference itself seemed to be much more like a game of golf in which each person continued to hit his own ball, as opposed to the game of tennis in which the ball is tossed back and forth across the net.

Well, in our conferences we believe in mixed doubles and we are very glad you are here for that reason. We do toss the ball back and forth across the net, and I think this is the only way in which the necessary communication adjunct to all the computerizing and to the reading can be fulfilled.

BRUES: Where is the net?

FREMONT-SMITH: Well, the net is slightly visionary but it is right here, there's no question about it. It's a kind of a curved net and it's also one that moves around and shifts it's position from time to time.

I will take one more minute to ask you to just consider the difference between a speech and a conversation. Now, I'm the only person who is allowed to make a speech here and even I m being interrupted, thank God for that. I'm glad of it. But in a speech, unless a person is one of those rare birds like George Wald who can just capture a whole group of people and carry them with him, one makes a series of statements which are bound to be misunderstood, or differently understood, by the majority of the people in the audience.

The audience also, if paying attention to what is being said, is bound to have a number of ideas, challenges, doubts. But, since it is not polite to interrupt when someone is making a speech, excepting in my case, all these ideas, thoughts, doubts that come to mind have to be repressed. This is why listening to a speech is so exhausting. You spend all your time and energy repressing every idea you have or else you settle down and doodle and think about something else, which isn't very effective.

Now, in a conversation, of course, you've got something else. You've got a mutually corrective feedback system which keeps the people in the conversation on the same wavelength or lets them know very promptly if they are not on the same wavelength. In the speech there is one person in the room who really gets satisfaction from the speech, and that is the man who is making the speech, because he hears himself saying what he plans to say, the words come out very much as he planned to say them, and there is a tremendous amount of reassurance and satisfaction. I see Jelle is shaking his head.

DE BOER: That's not true. I've tried to make speeches and they never came out the way I planned them! [Laughter] I wish it were true, but go ahead.

FREMONT-SMITH: You are unique, anyway.

I think I've said enough to give you a picture of what we're after. I did want to mention one point, and that is that we try to do a combination of two things at once. We try to have the maximum of freedom of interchange, that is, people speaking perfectly spontaneously and not holding back, and interrupting when they feel like it; and, on the other hand, the interchanges are so exciting that we want a record of them. In order to have a record of them without inhibiting the interchange, we have a stenotype reporter here with us who is taking down everything that we say. But, if you want to say something off the record, if what you are going to say about somebody is so awful that it has to be off the record, you just say "off the record" and you will see that the reporter will raise his hands and not take anything down. However, I beg people to keep it on the record because before publication each person will have an opportunity to delete anything he wishes he hadn't said, or to improve or otherwise modify it. You can't modify anyone else's remarks but your own. Over the years—and I've been at this business for some forty years—we have found that this system works out fairly well.

I would mention one other thing that I believe in, and I think this goes along with something that you said, John, about getting out all the facts that come from the past. I think that the half-life of facts is getting shorter and shorter. I think that this is a very important point to keep in mind, that facts are not very steady. In fact, I often challenge anybody to produce any two facts, or any fact for that matter, that isn't based on at least three unproven assumptions. Therefore, we will perhaps try to get a little bit behind the assumptions.

In a group of this sort, with this kind of interaction, you do get some new agreements and these are quite useful. But even more important is the specification of the nature of the residual disagreements. If you can specify the nature of a disagreement you often are on the track to its resolution. Very often the research that is necessary for its resolution, the new teamwork which is necessary, becomes self evident, or it may be that there is no technique for resolving the issue and you know that you have to put it aside for a while until the technique is developed.

The other thing that happens is that you get to know each other, that you become friends, that some of you even cooperate with each other. I have known people who have met at our conferences who

visit each other, have shared experiences and even have worked together on a common problem from different disciplinary points of view. In many respects this is perhaps the most rewarding aspect of these conferences, that is, the human relations that take place in the room.

Well, I've talked too much already; I thought I would be interrupted again before long, but I wasn't. So now, Austin, I turn it back to you and Arthur.

BRUES: I think there are some other questions that need not be discussed.

FREMONT-SMITH: Do you want to name them?

BRUES: Such as what is the consequence of deleting the question and including the answer or deleting the answer and including the question?

FREMONT-SMITH: The editor's responsibility is to deal with that terrible difficulty.

SELF-INTRODUCTION

BRUES: We come next to the self-introduction of participants. We feel that it's useful for everyone here to hear a description of each of the other participants in his own terms. I would suggest, in order to simplify this, that we start at one place and proceed around the table. May I ask Dr. Bustad to make his self-introduction and then we will proceed counterclockwise.

FREMONT-SMITH: We would like you to tell us a little bit about who you are and how you got that way.

BUSTAD: My name is Leo Bustad, and I am a Professor of Radiation Biology at the University of California at Davis in the Physiological Sciences Department. I am also Director of the Radiobiology Laboratory. Prior to coming there two years and thirty-four days ago I spent sixteen years at the GE Company, Hanford Laboratories, which was taken over by the Battelle Northwest.

SPEAR: There's a certain risk of being repetitious to those who were here before and also to those who were not and who found

themselves unable to resist curling up with the little brochure that was sent out by Colonel Hemler's group. But I will run through it briefly. Among the largely irrelevant endeavors that have resulted in my being here, I was an English major at Yale in 1932, a very poor year for English or any other majors; this was followed by two quiet years of sitting alone and reading on an old abandoned farm. Then I went to Syracuse where I took my Master's degree in public administration. Then there were about nine years of working in the public welfare field, followed by a brief hitch in the land-locked Navy. Then I went in Federal Government where first I was in a very improbable office, the Foreign Relations Service of the Veterans' A ation. I first ran into Staff Warren and Chuck Dunham in 1 when I went with the Federal Civil Defense Administration.

I beat my head against a stone wall in one way or another until a few years ago when I moved out of government and went with Public Administration Service, which is a consulting service that works only for government. My involvements there have been largely irrelevant to our discussion here. I've been concerned with such things as local building codes, the education of the deaf, federal grants-in-aid to states, the use of data processing machinery in vocational educational schools and things of that sort. But at one time in government I was with the Planning Board of the National Security Council and engaged in an enterprise of which this conference reminds me very much: the human effects of nuclear weapons development. In this, two of my colleagues were Frank Fremont-Smith and Staff Warren, and I'm sure that my involvement here is somehow or other an outgrowth of that.

I would say that my pleasure at being here at the second conference is heightened by the realization that George Casarett will be editing this series! [Laughter] So, my responsibility ends with the discussion.

WARREN: Well, I'll try to make mine brief. It's a little difficult because I am the oldest one here, of course, and I've been in the radiation biology field. . .

FREMONT-SMITH: You're not quite as old as I am, I don't think.

WARREN: You're just a young squirt in my mind! Indestructible is what you are, Frank! [Laughter]

I've been in radiation biology and similar aspects since 1918. I recall the conference that Ralph talked about because Frank was the chairman and we had delicious arguments, and I think it's probably one of the early conferences in which you used this discussion technique. Of all things, we had a poet there. How a poet. . .

SPEAR: A philosopher, too.

WARREN: Maybe a philosopher.

FREMONT-SMITH: T.V. Smith.

SPEAR: Yes.

WARREN: Well, I got into the Manhattan District through devious channels, from my standpoint, and became the medical officer during the war. I had to develop safety around the operations in the plants and in the mining industry that was connected with it, and also I had to set up the medical programs in the so-called "secret cities." I met this gentleman on my right, Wright Langham, at Los Alamos, and, of course, at Bikini later Dr. Conard over there was in the right spot. I looked at the movies here a couple of months ago and you were quite young and vigorous looking at that time! [Laughter]

FREMONT-SMITH: Isn't he nice!

UPTON: You are indestructible, too, Bob.

WARREN: Well, then I had the bad judgment to become a Dean at UCLA with the idea that I was going to put research on a legitimate basis in the medical school, which I think I did. Expensive space for biomedical research is taken for granted now; it wasn't in that day at the University of California.

Now I'm in a very favorable position: I'm emeritus and I'm an unregistered graduate student in paleo-radiation-biology. I've reviewed my math and physics and a little bit of this and that. It reminds me of a story which I think maybe some of you have heard, and it's part of my paleo-radiobiology.

Talking about Noah, after he got all the animals safely ashore, he encouraged them at the last conference to go now and reproduce and multiply. They all filed out and a few months later he went around

looking and there was due multiplication everywhere except among a pair of snakes who were specialists. They were adders, and he encouraged them but they had not done anything. So, he was kind of sad, went away, came back about four or five months later and saw that they had multiplied. He said, "Well, isn't that wonderful?" And they said, "Yes. You know, we're adders, but we found out about logarithms, that you can multiply!" [Laughter]

LANGHAM: I'm Wright Langham of the Los Alamos Scientific Laboratory where I've been now for some twenty-four years. After a year or so with Seaborg at Chicago I was transferred to Los Alamos where I've been ever since and where I migrated into the biological-medical aspects of radiation. I've never had a chance to admit publicly before that one of the principal things I'm proud of is that I consider myself a protégé of Dr. Warren sitting on my left.

WARREN: Was I responsible for that! [Laughter]

LANGHAM: He taught me many things. This does not mean I always agreed with him; in fact, I just noted here that the first time I disagreed with him was on February 12, 1945. I am happy to acknowledge at this time that I am proud to be his protégé.

WARREN: I learned a great deal from him. You can't imagine the vacuum, the intellectual vacuum, that existed when those of us responsible for the safety of the community gathered together in Alamogordo with really no information on what was going to happen. The guesses and the facts put together by Wright Langham, Jim Nolan, Louis Hempelman, Jack Hubbard, Joe Hirshfelder, and the late Paul Abersold, a most unreconstructed group who, as General Farrell said, must have used a divining rod to guess the weather, came out right; a homogeneous group of neometeorologists.

The picture that was created by this group, it seemed to me, was pretty close to the facts that happened later, and they worked out very well. I have to thank both Wright Langham and Bob Conard for their contributions at Bikini. The chairman, Frank Fremont-Smith, of course, is notorious because he collected radioactive sodium seawater 72 miles downwind from the test site. He got there just in time, to the horror of the captain of the ship.

FREMONT-SMITH: The rainwater.

WARREN: Well, it came down in the rain but it was uplifted from the lagoon. In retrospect it was very interesting but at the time it was pretty rugged.

CASARETT: I'm George Casarett. After undergraduate studies at the University of Toronto I came back to Rochester, which is my home town, and went to work in Staff Warren's bio-physics division in the Radiology Department at the medical school there. I also did my graduate studies in the medical school there. I was one of those who was infected by Dr. Warren's enthusiasm for research. Then I worked in the Manhattan Project Laboratory at Rochester and subsequently in the AEC Atomic Energy Project which became the Department of Radiation Biology and then later the Department of Radiation Biology and Biophysics, in which I am a Professor. I have a section of radiation pathology in that department and also in the Radiation Therapy Division of the Radiology Department.

MILLER: I am Robert Miller, and I arrived in Rochester after Dr. Warren had left, too late to benefit from his presence.

I am a pediatrician by birth, and upon completion of my training in that discipline, I spent a post-doctoral year sponsored by the Atomic Energy Commission, which was trying to fill doctors quickly with knowledge about radiation medicine. I then had to enter the Army, which did not know what to do with me. So they assigned me to AFSWP, the Armed Forces Special Weapons Project, which I understand is now DASA.

Subsequently, I went to the Atomic Bomb Casualty Commission in pediatrics and later in a study of the effects of inbreeding on child health. That experience led me to epidemiology, whatever that means, and to the National Cancer Institute where I am in charge of the Epidemiology Branch.

FREMONT-SMITH: Jelle, you tell us a little bit about how you got involved in the very beginning of this, too.

DE BOER: I don't know where to start. Jelle is the name. It comes from "yellow," I have been told. I have never been at the Nevada Test Site. These past couple of months, I have reviewed what happened at the Bikini tests, and read quite a lot about what occurred during the "Lucky Dragon" incident. This is not why I am here. I am here because I am working with a group of young people who are instrumental

in making better delivery systems for nuclear weapons and better fusing systems to detonate them, and in devising better strategy and tactics for their use. Most of these young people—and not all are that young—have not had the privilege of seeing what these weapons can do. Yet, there seems to be an increasingly enthusiastic mood about using these newly designed and packaged weapons, which are of great variety, design, and yield.

This is what concerns me. This is why I am here. I may not be the oldest here but having had the experience of being bombed during World War II, I am of the opinion that one major war during anyone's lifetime is just about enough. Now, I know some of you here already may have gone through two wars and you sure won't ask for a third one. Yet, I am convinced by all that I see, hear, and read that unless we as "scientists" of different feathers start looking into this, we may just get that war that none of us wants.

Most scientists become increasingly specialized while losing, to an ever-increasing extent, contact with the world in which they live. In applied research—research I am engaged in at the Air Force Weapons Laboratory—things often seem to be the reverse of what I just said. Here as a student of reproductive physiology and endocrinology, I have not had a chance to do any work in this field of science, but rather spend my time studying the effects of nuclear weapons on integrated biological systems. We are to determine what really does happen after a weapon has been used. This particularly in terms of when, and for how long, where, and over what range, and how, i.e., how quickly does a man get killed, and if he doesn't get killed, what has been done to him in terms of his potential as a soldier? To put it in other words, can he still be used? If not killed, and having a chance to live, what are his chances, and are they good enough to send him to the medics who "supposedly" know what to do with him in terms of treatment.

Gentlemen, the facts are that the medical profession, in general, knows next to nothing about the treatment and care of radiation damaged biological systems. Granted, many medical doctors working in research do know. Dr. Conard's work is an excellent example. He and his team have gained tremendous knowledge about what happened and can happen from their studies on Nagasaki and Hiroshima. Yet, somehow or other this information does not enter into the medical school curriculum and our young M.D.'s graduating from these institutions know little or nothing about it. Seemingly to them and those who teach them, there are more important things to know about

and specialization in the more profitable branches of medicine often enters into consideration.

So from where I am sitting, it appears very important that we not only look at and evaluate the long-range effects of nuclear weapons. After all, this has already been done and is recorded in the publications of Dr. Robert Conard and many others. But, and perhaps more important, we should look very seriously at the chances of using similar weapons again. Many think at this time, more than 20 years after the bombing of Japan, that we should be able to put before those who are charged with the responsibility of the uses of such weapons, what the consequences of their actions might be. Gentlemen, I am afraid we are not prepared to do this as yet. Moreover, I feel that this can't be done by the biological or medical profession alone. I think this has to come from a reasonable consensus of opinion expressed by the scientific community, i.e., a community which includes the physicist, engineer, social and natural scientists, both military and civilian. Why? Because now these scientists and engineers compete with each other for the dollar which supports their individual interest rather than cooperate with each other on an apparently less profitable and more complicated venture which would be in the common interest. In doing so they are leaving our exposed flanks increasingly vulnerable.

How can this interdisciplinary scientific community attain their goals? I think these conferences in which we participate today could be a beginning. The end-product of these conferences has the potential of showing clearly the urgency of the problem as well as containing some basic suggestions as to how the problem could be solved. I know that this is an urgent matter and one which will not be solved when discord and distrust prevail in the scientific community. We, as scientists, cannot afford much longer to put self-interests before the Nation's interests. We must grow mature enough to admit that sometimes we may have been wrong; that in science there are no short cuts; that we need each other; and as a group, we must be able to relate to those who govern us what the lack of certain knowledge may have as consequences. Then, and only then, can those who are now in power make intelligent judgments in assignment of priorities. I hope we are not too late.

Dr. Frank Fremont-Smith urged me to think of clear-cut justifications for holding these conferences and to be sure to know my assumptions. Well, gentlemen, these are my assumptions:

1. We have a lot of misinformation.

2. We don't know enough about the effects of nuclear weapons on man.

3. We do not have an infinite amount of time and money to do something about this lack of knowledge.

4. We need priorities, not only in the biological or medical sciences, but priorities in terms of all scientific pursuit.

5. We do not live in a vacuum, nor can we. We need each other.

These interdisciplinary study groups, with follow-up research suggestions, may be the beginning in providing some answers and may eventually prevent a holocaust. It is not an impossible task, at least I like to think it is not.

TAYLOR: I'm Ted Taylor. I spent most of the war going to Cal Tech as a Navy apprentice seaman. After the war was over I went to the University of California, and in 1949 I went to Los Alamos and spent seven or eight years there working on the design of nuclear weapons. From there I went to General Atomic in San Diego at about the time it was formed and spent six or seven years there, mostly promoting a scary project that many people didn't like: propelling big space vehicles by a series of nuclear explosions. From General Atomic I went to the Pentagon and worked two years in DASA. It was at that point that somehow several people simultaneously got on to the idea that a collection of people more or less like this group here was a good collection to get together, probably several times over quite a long time.

The main sorts of things that I had in mind in thinking about this had to do with possible things that the Defense Department might do that it was not doing and that would be the result of more detailed understanding of what the long-range effects of various types of nuclear war might be. I still believe that there are some things that would be rational things to do that are not now being done and that are difficult to do without knowing more about what the long-range effects might be. The kinds of things I have in mind are such things as changing the way in which missiles are targeted so that, for example, instead of mostly going off on the ground, they would go off in the air in such a way as to produce essentially negligible local fallout but would disperse the fission products all over the atmosphere. I don't know of anyone who has said that he thinks it's clear which type

of fallout is worse for humanity as a whole. Obviously, it's not a simple question even to phrase, let alone to answer. But there are some things that could be done to change the character of the strategic forces that we have, and that might be sensible things to do if we knew more about what these long-range effects might be.

Parenthetically, I want to point out that in some cases a threat of retaliation against a country for a surprise attack or an accidental start of war is likely to be something which, if carried out, would do severe damage to countries that border on the country attacked. If we do as I believe is now planned, such side effects probably could be avoided. I think they should at least be understood as well as possible. I want to emphasize this because DASA's interest at that time—and I think this continues—is to some extent connected with what might actually be done that is not now being done. Such action might flow out of a rationale that comes out of these meetings.

I left the Pentagon in September, 1966, and went to Vienna, essentially on my own, because of very strong compulsions to at least try to understand what was going on in an attempt to control nuclear energy on an international scale. It seemed to me that the best thing to do was to go and find out what was being done by the agency that at least nominally had the job to do this. So I've been there for about a year, originally as a consultant to the AEC on a day-to-day basis, and for the last six months as a member of a company that has one full-time technical employee.

I am working essentially entirely on what people call safeguards, that is, the formal attempt to try to establish some kind of control over plutonium or U^{235} to prevent it from getting in the hands of people who might use it for purposes other than those for which it was originally intended. Some of what has developed in Vienna in the last year, at least that I've been exposed to, has some bearing on what I think is happening here, in the sense that the likelihood of nuclear explosions being detonated for destructive purposes I think is increasing with time, not decreasing. There is at least one reason for this, and that is that the material out of which these things can be made is being dispersed all over the world in a way which is not at the moment under sufficient control to prevent any determined effort to use this material for making nuclear explosives.

The thing I've been concerned about most is not the proliferation, in the usual sense, of military stockpiles of weapons, but the use of

nuclear explosives by people that don't need to identify themselves to have their purposes served. The reason I think this is extremely important, and even has some bearing on our meeting here, is that one way in which deterrence—which I think all of us would agree has produced the stability that has existed since 1945 with respect to general nuclear war—might fail is that the attacker be unknown. As soon as the attacker is not known, the whole concept of deterrence disappears. One way for attacker not to be known is for the attack to be based on purposes that are well served if the attacker doesn't have to identify himself.

I have followed to some extent in the footsteps of Fritz Zwicky in using what are called morphological outlines to try to sum up the number of ways in which a threat might be made without the threatening organization being identified, and my outline at the moment is capable of generating 10^{35} possibilities. So I'm somewhat dismayed about any notion of control that requires understanding all of the things that people might do.

I think we have to understand what the end effects may mean. I think this is part of what this conference is about, and I think we also have to find a way to keep the basic material out of the hands of people who could use it for their own purposes. That's what I'm up to.

AYRES: I would be delighted to pass up my opportunity to talk and hear you explain this factor of 10^{35}.

TAYLOR: Well, that can be done at some point. Do you want me to explain it now?

AYRES: Maybe not.

My name is Robert Ayres. I suppose I ought to start at the beginning. I thought of starting at the end. I studied mathematics and physics despite the fact that I can't multiply. I spent several years jumping from university to private industry and back to university. I went to the Hudson Institute in 1962 and spent four years there spending most of that time worrying about long-range effects of nuclear war. This was work supported by the Office of Civilian Defense.

Parenthetically, I might add that the only reason I knew about this proposed series of conferences and, as far as I know, the only information that DASA had about the OCD (Office of Civil Defense), came

about because I happened to walk into Dr. Taylor's office to ask him on behalf of the Committee on Emergency Planning of the National Academy of Sciences what research DASA was doing that was pertinent to emergency planning. I was also asking many other agencies of the government the same question. So he told me what they were planning, which I found very interesting.

I've spent some years thinking about these problems, but not to the depth that I hope some of you will be able to carry the discussion. I didn't specialize in any of the subjects which one needs to know. I was a physicist but I didn't specialize in weapons design. I'm not a biologist nor a doctor. So I feel very much a lightweight in many of these discussions.

Perhaps the major purpose of the study we did was to identify questions. I'm not sure how successful even that was. Since our last conference I have left Hudson Institute—and this outline is now out of date. I occasionally share an elevator with Spear. I work with Resources for the Future, and I suppose the most accurate description of what I'm doing at the present time is studying environmental pollution.

HEMLER: I'm John Hemler. I suppose, Dr. Warren, you can call me a kissing type poet! [Laughter] However, at the time that you were detonating the first weapon over Alamogordo I was delivering newspapers in Carlsbad just across the mountains over there, and I might say that was my start in nuclear weapons then.

WARREN: Pretty good contact.

HEMLER: Really indirect contact. Shortly after the war I went to West Point, graduated from there in the early fifties, and the Army absorbed me into its overwhelming regimentation for the next few years. In 1958 they decided to release me for a while, and I went to graduate school at the University of Arizona where I obtained my master's degree in physics and mathematics. Then the Army pulled me back into its fold again. I worked with the Nike-X program for three years under the Army jurisdiction, primarily in the warhead development area. I was loaned then to the Atomic Support Agency in 1961 and 1962 for, they called it then, "a thirty day" period, to participate in the 1961 and 1962 test series in Nevada and the Pacific. This began in late January of 1962 and the "thirty-day" period was over in late November of that same year, but I did get a chance to see many of the tests that went on.

From there I went to Korea and continued my work in weapons, only at this particular time I was concerned with the targeting aspects of nuclear weapons. I returned from that and I'm now with the Atomic Support Agency in the Office of the Deputy for Science and Technology. As someone mentioned a few minutes ago, this is the new name for the old AFSWP, or Armed Forces Special Weapons Project which divided out of the Manhattan Project back in 1947.

We are concerned with the overall management of the Department of Defense Effects Research, laboratory research and test programs that are going on in Nevada and in the various laboratories all over the country, both governmental and private. We work with the Atomic Energy Commission. We also work with many private contractors.

WYCKOFF: I'm Harold Wyckoff, presently at the Armed Forces Radiobiology Research Institute at DASA. I suppose when one gets to my age, one can look back and wonder why one's interests have taken a particular direction. Major changes of interest may often be traced to a particular environment or acquaintance. When I attended high school, I became interested in electrical engineering. This interest was aroused by the presence of a small radio broadcasting station belonging to the school and built, maintained, and operated by the students. My participation in this fascinating activity did much to shape my future. Electrical engineering continued to hold my interest through my first year at college. At that time the broader horizons and close contact with graduate students in the "purer" sciences influenced my planning for the future. During this time, I considered both chemistry and physics as possible majors and ultimately chose the latter.

The subject for my thesis was in the area then called "modern physics." The high voltage source required for it was only available at one of the local cancer clinics. Hence, I became interested in what is now called radiological physics. When I graduated it was a natural step to the National Bureau of Standards (NBS) where the radiation standards for such work were being developed under the direction of Lauriston Taylor.

This more or less orderly progression was interrupted for about two years during World War II. The NBS standards activity was slowed down and the manpower was diverted to other activities. I spent those two years as a member of Laury Taylor's operational research group at the 9th Air Force.

When I returned to NBS the radiation standards requirements had expanded considerably. Radiation shielding requirements also became a more important economic force. Both of these areas occupied my interest for the next few years. From here it was natural to drift into the area of shielding design and thus into committee activities dealing with this area. Many of the persons involved in this activity were also interested in the biological bases for radiation hazards. Close contact with them aroused my interest in the subject. My transfer to the Armed Forces Radiobiology Research Institute about a year ago was a result of this interest.

DUNHAM: I am Chuck Dunham. I haven't practiced for eighteen years. I was at the University of Chicago, but I had nothing to do with the Manhattan Engineer District, though people often assume that I did. I was in Los Angeles and Hawaii while these things were going on, in the Army, doing regular Medical Corps duty. I joined the Atomic Energy Commission in 1949, a Johnny-come-lately like Dr. Taylor, and within a couple of years I met half of the people in this room who are the real old hands at this game, and I've been learning from them ever since.

I eventually became Director of the Division of Biology and Medicine of the AEC, and my principal function was to serve as go-between between these distinguished gentlemen and the Commission, the Bureau of the Budget and the Congress in trying to see that they were supported in the style to which they had not only become accustomed, but to which they were entitled.

I don't consider myself an expert in any of these matters. I did have the privilege of being out at Eniwetok for a month at the time of Operation Greenhouse and I was with the medical team at Kwajalein following Bravo shot. Dr. Conard, of course, was there. So I've had some contact with these things, but mainly I've been in administration.

Recently I have retired from the Commission and have taken up a job as Chairman of the Division of Medical Sciences of the National Research Council, and I bring you perhaps a different perspective from that standpoint.

I am looking forward with a great deal of anticipation to this conference and I'm particularly delighted that the theme of it is not strictly scientific because it's been one of my pet hobbies that the basic

problems of mankind are probably not going to be solved in the laboratory; that the basic problem is how people can get along, how they can live with each other. There are laboratories of psychology and the whole world is a laboratory in one sense. But in the ordinary test tube laboratory I think you can only go so far, and with that I will step aside and let John Wolfe pick up. He came a little later than I did in this game, but not much.

WOLFE: The same day you took over as Director, I think.

DUNHAM: Yes, sure.

WOLFE: I don't have such an illustrious history as these other distinguished gentlemen. I was a professor of botany at Ohio State University in 1955.

FREMONT-SMITH: Were you sort of born a professor of botany?

WOLFE: Well, I worked up to it, Doctor. For fifteen years I worked to the place where I could have two classes a year with graduate students and then I went to Washington for two years. I came to the Commission at the same time that Chuck took over as Director and I've had a decade of happy years in the ecological program there, I think mostly because of his broad view that science is not going to solve everything in this vale of tears. I've never heard him express it that way before, but have had a feeling from time to time that he did look beyond the little fences of science. Whereas, I suppose, I was supposed to be a scientist at the university, I'm pretty sure I stepped across the fence from time to time both in class and in research. At any rate, it was a considerable jump from the classroom to Washington. I hope, before I retire, to find out what the hell Washington is all about, but it's coming slowly! [Laughter] I was always a slow learner and maybe in another ten years I'll make it.

We're interested in the ecological aspects of nuclear energy in whatever form and through whatever use. I think I have never said it before in public but I have never claimed that ecology is a sophisticated science, like you fellows that carry things out to six or eight decimal places. You even have got me talking about my thirty-second wedding anniversary as 2^5! [Laughter] But it's a discipline or a point of view or an attitude that mankind had better face up to in the next decade, or maybe he's got a quarter of a century, but he's in a hell of a mess now and I think what we're doing in ecology, in the

Atomic Energy Commission, and what they're doing in NSF and a few of the other agencies might contribute to some of the answers. I don't know whether they will solve the problem but they might contribute to the solution.

Lauren!

DONALDSON: Why don't you keep right on, John? I was enjoying that! [Laughter]

Like Mr. Spear, I'm well aware that the year 1932 was not really a vintage year! [Laughter] I was one of three people added to the staff of the University of Washington that year, and if you are associated with a university at this time, you are well aware of the difference between the depression years and the present, when university appointments are made almost every day. I have been at the University for many years, with a few detours such as working with Dr. Warren on many of the projects and programs that he initiated in the field of atomic energy. We started our work together in 1943, with the objective of developing an evaluation program to measure the impact of the operations of the Hanford Works on the Columbia River. Of course, as a "fish farmer" I was particularly interested in the Columbia River, for this gigantic stream was our best fish "farm." Five species of Pacific salmon use the stream during their early life span and then after a sojourn in the sea, come back to its cool water to spawn and reproduce. You can imagine my consternation when the early discussions projected estimates that billions of calories of heat and thousands of curies of radiation would be added to the river. To make matters worse, Dr. Warren insisted that we should have all of the answers that normally would take a lifetime to get, with luck, but he wanted them "right now."

WARREN: You almost did it on time, too! [Laughter]

DONALDSON: During the war years we were able to pretty well establish dose measurements of acute radiation for a variety of aquatic forms. With the termination of the war, we assumed that we could go back to teaching school and "farming" fish. Again Dr. Warren had other ideas and invited us to take part in Operation Crossroads, with the nice easy assignment of evaluating the radiation from the weapons tests as it drifted over a major part of the Pacific Ocean. Dr. Conard was a member of our little group operating with LCVPs and other small landing craft as we attempted to plot the radiation field in Bikini lagoon.

The following year, 1947, we went back to Bikini to evaluate the biological impact of the radioactivity on all of the living forms there and returned again following subsequent weapons tests in 1948, 1950, 1952, 1954, 1956, and 1958. In 1962 we went to Christmas Island and studied the effects of the fallout in that area.

One of the most exhausting and traumatic experiences of any of our Pacific experiences was in 1954, following the Bravo shot. We were asked by the State Department to go to Japan to try to quiet the fears of the Japanese people, which resulted from the Fukuryu Maru incident, and to help to persuade them to resume fishing and, more important, to continue eating fish.

During this time there were a few interludes with trips to the Nevada testing ground, where again we had some interesting assignments. One was to collect rattlesnakes so that we might measure their uptake of radioactivity. On one such assignment I was really "holding the bag" for one of our co-workers who was to put a live rattlesnake into it. This would not have been such an unpleasant task except that he was holding the snake by the tail, and as he waved it around, my hands provided a fine target to strike at.

DOBSON: Did you do that experiment?

DONALDSON: Yes.

DOBSON: What happened?

DONALDSON: To the rattlesnake?

ROOT: To the bag?

DONALDSON: I decided I should hold him by the tail and let somebody else hold the bag.

FREMONT-SMITH: What happened to the snakes?

DONALDSON: Nothing. As so often is the case, there was no observable effect.

FREMONT-SMITH: Nothing noticeable?

DONALDSON: No, nothing at all.

Later Dr. Warren left for greener pastures and Dr. Dunham and Dr. Wolfe directed our activities. They also wanted answers—yesterday—and we have continued to try to provide them as best we can. A few months ago I decided that it would be wise to change direction a bit and give up some administrative responsibilities. Thus, in the next few years I hope to be able to write down some of the observations and conclusions in the way of answers to the questions which have been asked over the years.

ROOT: I'm Lin Root and I come to the Interdisciplinary Conference by an undisciplined and circuitous route. I started as a biochemist, did most of my work at the College of Physicians and Surgeons of Columbia University, took a New York State examination and found myself in charge of research for the Psychiatric Institute of the Manhattan State Hospitals. Biochemistry at the time was both primitive and cloistered—no far-flung conferences in exotic places. After several years of quantitative studies of the arterial blood of dementia praecox patients and schizophrenics, to the amused bafflement of my psychiatric colleagues, I felt the need to get away from my ivory tower and to go where the action was. So I joined Will Beebe's expedition to the Galapagos Islands and the Sargasso Sea, with the aim of making comparative studies on the blood of birds and fish. After this I became Science and Medical editor of Time magazine and then moved into feature writing for national magazines.

In the early fifties when atomic reactors were still very hush-hush in the U.S., a series of lucky breaks gave me several scoops on foreign atomic energy stories—a sort of chain reaction that culminated in the inside story of the Soviet nuclear situation just before the first Geneva Atoms-for-Peace Conference in 1955.

This was after the 1953 Soviet thermonuclear detonation, and our big 1954 one at Bikini. In the last few years I have been systematically studying the long, or I should say intermediate, range effects of the 1954 test in every country I've been in. You can trace them in international relations, in political alignments, in the attitudes of youth, in the credibility gap, in practically every aspect of life. People are not aware of how much our whole culture has been affected by this event. I think a careful study of this precusor should provide important clues to the long-range biomedical and psychological effects of nuclear war.

MILLET: My name is Jack Millet. I am a psychiatrist and a psychoanalyst. If you've been thinking that I must be English, you are not quite right. I was born there and brought up there but my parents were Yankees. My father fought in the Civil War and heard Lincoln give the Gettysburg Address; so, you see, I think I have the claim to say that perhaps I am the father of the family here today. I am very much pleased to have the opportunity to come and learn something. The only people I feel I can learn anything from now are young people, you see, and this is a great privilege for me to be here.

I've been engaged in a variety of things in my profession. The last years have been devoted mostly to the education of residents and the establishment of schools for graduate training in psychiatry and psychoanalysis. I now still do a little supervision for advanced studies for Columbia, but most of my time is spent quietly in my own office doing consultation work and carrying a few patients of my own. I am also presently Director of the Ruth M. Knight Counseling Service of the Manhattan School of Music.

The last thing that I have undertaken is to establish a counseling and referral service for the Manhattan School of Music. I have always been interested in community psychiatry. I've been responsible for establishing two or three services in different states in the course of my life. I've been retired twice, I think, now, and I am about to be retired again against my will from the United States Committee for the World Federation for Mental Health, with which I've been identified with Frank here for so many years.

FREMONT-SMITH: Jack, you didn't say anything about the group for the advancement of psychiatry which is somewhat related to some interest here.

MILLET: Yes. In our committee, which has the rather pretentious title of Committee on International Relations of APA, we are digging into what we can come up with in understanding how it came to be that we are in such a hell of a mess in Vietnam; how and why we ever got into it and how it got escalated to the point where it is, and so forth, all the motivations and the conflicts that went into it and the changing viewpoints of each successive president. I was at one time Chairman of the Committee on International Relations of the APA, at which time we had a series of roundtables on transcultural psychiatry, the first one being an attempt to get some opinion on the reactions of the community to the threat of nuclear war. That was a very interesting

roundtable. That brings me as close to your interest, I think, as anything that I have personally experienced.

BRUES: Austin Brues. I majored in philosophy at Harvard College. I concentrated in internal medicine with excursions into some of the other medical specialties in medical school. I then went into cancer research and clinical hematology.

During World War II, probably not because of any particular competence but because I appeared to have guessed what was going on, I got drawn into the Manhattan Project where I could be kept quiet! [Laughter] I joined the Argonne Laboratory and the staff at the University of Chicago and was maneuvered into the position of directing biological and medical research at Argonne which I continued to do, for fifteen years, having promised to do it for one year; learning that that sort of job, like most, became more time consuming rather than less time consuming, with the passage of time. I retired from that responsibility about five years ago and have gone back into research in carcinogenesis.

FREMONT-SMITH: I'm Frank Fremont-Smith. Before I talk about myself, I want to make a couple of comments. First, that I think it's quite remarkable and wonderful that DASA has made it possible for us to hold these meetings and has put no restrictions upon us. They have told us to see what we can come up with. We could select the people to come and talk about what we as a group felt we should talk about on this topic. And they didn't say, "Now, don't touch that and don't do this." They just said, "Go to it!"

I think this was, in the first place, a very nice compliment to this kind of process, to the human interaction process, and also that it was a compliment to DASA that it could have the freedom of action to give us such freedom of action. I want to express my appreciation on that score.

I was especially pleased that Dr. Dunham, Chuck Dunham, if I may say, emphasized some of the limitations of science in terms of the human problems that the world faces. I think that this is a matter of utmost significance, and in a world in which computers and those who know how to feed computers are taking over, it may be worthwhile to remember that the most precious things in the world can't be quantitated and can't be put into computers. So, we are going to have to have some aspects of human judgment outside of quantitation enter in if we are going to have any opportunity to survive.

I graduated from Harvard Medical School. I never did get through either Harvard College or MIT, both of which I sort of indulged in. Then I was trained in neurology. While working at the Massachusetts General Hospital in neurology, I needed some extra typing to be done and my chemist, Mary Dailey, now Mrs. Rod Irvine, said she would get somebody that would be willing to do some typing to come in the next morning at nine o'clock. So the next morning I went to my laboratory at nine o'clock and, to be sure, there was a young woman there but, unfortunately, you practically couldn't see her because she was completely enveloped in the arms of a 6'4" young man, and this was my introduction to Staff Warren and his lovely wife, Vi, who hadn't seen him for three days and found herself in his arms! [Laughter] So, this was a very special occasion and it's been a continuing association of great joy.

WARREN: And it's marked us both ever since! [Laughter]

FREMONT-SMITH: Then, in neurology I became interested in psychosomatic problems, emotional factors influencing physiological behavior of human beings and animals. In 1936 I joined the Josiah B. Macy, Jr. Foundation and became what they call a philanthropoid —and this was Fred Koeppel's term. Fred Koeppel was the former president, the late president, of the Carnegie Corporation. He said that he felt that Foundation executives, poor lonely creatures that they are, deserve some special form of appellation and suggested they should be called philanthropoids. Why? Because, he said, they acted like philanthropists but with somebody else's money! [Laughter] So I acted like a philanthropist with the Macy Foundation money for some twenty-four years and altogether had a very interesting time in that relationship.

A couple of things bore on the present situation. I did find myself in the most unusual positions. In one of them, as Staff mentioned, I was out at Bikini, and after I came back from Bikini, through an absolute fluke which I won't have time to explain, the Macy Foundation, which already had been holding conferences in which I was very much interested, was asked by the Department of State, the Department of State no less, to hold a series of conferences on human relations insights coming from psychiatry, psychology, sociology, and cultural anthropology, and on how these might be used by the Department of State. This was a very interesting process and led nowhere in the long run because of the McCarthy era which followed not long afterwards.

Then I became very much interested in the World Federation for Mental Health that Jack Millet mentioned. This was or is a non-governmental international voluntary agency which relates all the branches of science that deal with human relations in a conjoint way, tries to bring these to bear upon the problems facing humanity and works very closely with the Economic and Social Council, with the World Health Organization and with UNESCO. At the time that the International Atomic Agency was established in Vienna, it was drawn to our attention that there were emotional problems, mental health problems, associated with the peaceful use of atomic energy in terms of panic reactions, in terms of accidents, in terms of protection against accidents and social behavior in response. The World Federation for Mental Health established an international committee which made a report which went to the Agency, where it probably could not be found, and also to the World Health Organization, which dealt with some of the human relations problems associated with the establishment of peaceful use of atomic energy on a worldwide scale.

Well, this, plus my very deep interest in conferences, in dialogue and in the essential need of human beings to relate themselves to one another, has been the basis of my special interest which led me here, and I'm very pleased that you, Jelle, got going and stirred this thing up with Dr. Taylor and John and made it possible for the whole thing to come about.

UPTON: I'm Art Upton. I did my undergraduate work at the University of Michigan, and went on there to complete medicine. After graduating from medical school, I took a residency in pathology and, as I was winding up in residency, received an invitation from Jacob Furth to join him at Oak Ridge to help him with some studies on the pathologic effects of radiation on experimental animals. I went down there assuming that it would be for a couple of years, and arrived there about the same time that a big batch of animals came in from Eniwetok, where they had been exposed to an experimental nuclear detonation. It turned out that Jake left Oak Ridge before the experiment was completed. I stayed on to finish it up, meanwhile started other things, and I've been at it ever since. I suppose that's why I'm here today.

DOBSON: I am Lowry Dobson. I have always felt that I would have benefited very much from having gone to Harvard College or having studied at Harvard. I didn't; but I was conceived in Cambridge, Massachusetts! [Laughter] I was born in Peking, China, and studied medicine at the University of California.

FREMONT-SMITH: You are a Harvard alumnus then! [Laughter]

DOBSON: After hospital training in internal medicine I went back to the Berkeley campus, to the Radiation Laboratory, in early 1946 which was not so long after Alamogordo, Hiroshima and Nagasaki. I did studies in biophysics, research and teaching in radiobiology, and was associated with John Lawrence in clinical radioisotope work. In Ernest Lawrence's Radiation Laboratory, and in the University more generally, I had responsibilities in medical physics and radiation protection. Then after some twelve years I went to Geneva, Switzerland, to work for a year or two—and stayed for ten—with the World Health Organization in radiation health, radiation medicine, and human genetics. I have just returned from California at the Lawrence Radiation Laboratory in Livermore, concerned with laboratory research once again, having spent ample time for the present in international administration which is extremely interesting but often a bit removed from animals and test tubes.

I suspect we all feel that potential energy is just as real as kinetic energy. We hope that potential nuclear war has no such reality, and that it will never convert. But the compelling interest and importance of the various related questions are, I think, the reasons many of us are h[illegible] at these discussions, whatever the details may be.

SCHULL: Jack Schull or, as the clerk at the desk informs me, "Shewell!" Department of Human Genetics, University of Michigan.

As the twenty-first man on this twenty-three man totem pole, I have wondered what remarks I might make which would enter into the spirit of "can you top this" that prevails! And it seemed to me that if there existed an opportunity, it had to be in the introductions which others made. And thus as Leo Bustad was introducing himself I wept for there but for a single letter, "M" instead of "B," might be the inheritor of a Norwegian fish hook fortune; and, as Stafford Warren was describing the social responsibilities that rested on his shoulders in 1943, I could commiserate because that year saw me on Guadacanal and subsequently on Bougainville with a gun and much the same feeling of social responsibility. When Bob Miller introduced himself I rejoiced in the shared memories of some very happy years in Japan; and finally as Frank Fremont-Smith spoke of the dent that this conference would make, I thought, in view of the way we are fed and beveraged, that a bulge rather than a dent was the more likely outcome. But there was an alternative to this play on the introductions

of others, and that was to take advantage of possibly the only opportunity that I shall ever have to introduce a rear admiral, Bob Conard!

FREMONT-SMITH: That's very nice. Lovely.

CONARD: Thank you.

When I finished medical school in 1942 I went into the Navy and spent several years on a cruiser in the South Pacific, and on my return went out on the Bikini operation and had the very pleasant and stimulating experience of working under Staff Warren and made some very valuable lifetime friendships with Frank Fremont-Smith, Wright Langham and Lauren Donaldson and many others.

During the Bikini operation we realized, as Staff pointed out, that there were many aspects concerning radioactivity about which we were ignorant. Because of this we were ultraconservative. I remember very well the bundlesome types of respirators that we forced the men to wear in offloading ammunition and the strict precautions to protect them. Later, in Hawaii when the target ships from the operation were returning, I was forced to order the sinking of many beautiful small boats such as Captain Gigs because radioactive contamination was considered too high.

Later on, I had an interesting experience when the Nevada, the target ship, was sunk by the Navy to test out some weaponry. I was asked to go along as the radiological safety officer and they insisted that I go on board with the two gunners mates to get these 1,500-lb. bombs wired up, and I protested saying I would like to see why a doctor really had to go along on an operation like this. But they insisted anyway and I had to hold the wires while they stuffed the gun cotton into these big shells and, of course, in the meantime the destroyer escort went off about thirty miles and stayed off until we finished the operation. And then later when I saw this ship blow up, I thanked my lucky stars that I had gotten off that safely.

I participated in other atomic tests including Operation Greenhouse in the Pacific and the Nevada test. I spent several years as Project Officer at USNRDL and had a fruitful year doing radiobiological research with Harvey Patt and Austin Brues at Argonne. In 1954 the unfortunate fallout accident occurred in the Marshall Islands while I was still in the Navy and I was a member of the original medical team under Dr. Cronkite along with Chuck Dunham that carried out the original examination of these people.

After that I joined the staff at Brookhaven National Laboratory, where I have been since, and have continued to head up the annual medical surveys of these Marshallese people; every year I take out the medical team, experts under the auspices of the AEC and Brookhaven, to examine these people.

Looking back over the years it seems to me we have come a long way since the early days of Bikini and have learned a great deal more. The Marshallese experience has provided us with important information about the effects of fallout on human beings.

So I think this conference may serve a most useful function in culling such information and examining it in the light of interdisciplinary facets.

EISENBUD: I'm Merril Eisenbud. I envy you fellows and Miss Root because you all know what you are. You are biochemists or experimental pathologists, veterinarian pathologists, physicians. I'm not sure what I am. I started out in pre-medicine; I switched to physics in my last year and then was persuaded that one couldn't make a living in physics; so I switched again to electronics engineering and took my first and my only degree in electrical engineering.

I was interested in biology and tried to find a degree program to bridge the gaps. I went from door to door; I actually attended school as a registrant for a graduate degree at three of the major colleges in the East, but couldn't break down the tight disciplinary barriers that existed at that time. As I look back on it now, it was probably a good thing.

I'm probably the last of the less educated professors. There aren't many of us left. I think of myself as a sort of a quasi-intellectual mugwump. I like to straddle the fences between the disciplines and I'm never sure which side of the fence my face is on or my rump is on; and sometimes I'm not sure which fence I'm on! [Laughter]

Shortly after I got out of school I heard that there was a job open in an insurance company for somebody with an interest in biology and a knowledge of electronics. I couldn't understand why one would want that kind of background to sell insurance. So I applied for it, and found that this insurance company, which turned out to be a very large casualty insurance company, was setting up a small laboratory to look at occupational disease problems that existed in those days; I was dispatched to some plants in Pennsylvania to work out some methods

of separating dust electrostatically. In the kingdom of the blind where a one-eyed man is king, I was teamed up with a fellow whom many of you knew, and who joined the group about the same time. I refer to the late Charles Williams, a petrographic geologist of considerable talent who taught me some useful techniques for identifying dust particles by their optical properties. At the age of twenty-three I couldn't differentiate a silicotic nodule from a sarcoma but I could look at a thin section through a microscope and tell whether a man was exposed to silica dust; the physical side of the pneumoconioses was fascinating and I became a dustologist.

When the war came, although I had never heard of the Manhattan District, I was assigned to parts of it and was actually working on a problem of beryllium poisoning in the latter part of the war, and I had also some exposure to radium poisoning. When the war ended, I guess the punch cards were in my favor. Actually, this isn't generally known except to a few close friends, but I decided at the end of the war to go to medical school and was admitted to Tufts, and was supposed to be the oldest man of the class of fifty. Then in 1946 the Commission offered me a job, which I couldn't turn down, and I took it and worked in a position which was extremely interesting, during which time I was one of Chuck Dunham's people and got involved in a number of things which will be the subject of the discussion this week.

Finally, like many of you, I had to make a decision because as time went on I found I was getting away from where the work was being done. I had just about given up hope of ever being where the fun was. I thought I would have to spend the rest of my time in administration, when things began to stir in the field of environmental health in 1958 and I found myself with three offers from three universities. I selected the one that I had then been associated with in a part-time capacity for, at that time, almost fifteen years, now almost twenty-five. So I went to the New York University as their professor of environmental medicine. When I got there I tried to dignify the title of "Mister." As I say, my only degree was in electrical engineering. I had no problem at all except in the parking lot. They had a protocol in which if you were a doctor you were in the front, and if you were not a doctor you went to the back. I would drive in and the fellow would say, "Are you a doctor?" and I would say, "No," and he would send me around the back.

Well I think word got out that something had to be done about this, and I armed myself with an honorary doctor of sciences degree, which

permitted me the front row in the parking lot! [Laughter] Well, it's been a random walk, and when I think about it, I would not have wanted it to be different. The only way I can explain it is the way Polly Adler explained it, if you ever read her book, you old timers. She was a well-known madam who wrote her autobiography. She used to tell the story that her biggest problem was with the college boys because after it was all over they wanted to know how she got into that business, and the only thing she could say was "Lucky, I guess!" [Laughter]

BUSTAD: I'm glad I was first!

BRUES: This takes us all around. Let's not start again! [Laughter]

SESSION II
THE 1954 THERMONUCLEAR TEST

INTRODUCTION

BRUES: To introduce the subject which will occupy us today we have asked Dr. Dunham to say something about the 1954 thermonuclear test, its background and nature and anything else he wishes to say.

DUNHAM: My guidance has been rather loose, I would say, and not having attended the previous meeting, you are going to have to put up with my playing it very much by ear. I have taken our leaders literally in that I haven't prepared a half-hour lecture on any particular topic and I gather that my function is that of an initiator in the sense that one talks about initiators in atomic weapons; the problem is whether I can generate enough neutrons to produce a chain reaction with this, our critical assembly here. [Laughter]

FREMONT-SMITH: Critical mass.

DUNHAM: Critical mass. I've been thinking about this off and on ever since Austin persuaded me to take this assignment last June, and I'm still having very great difficulty in trying to relate this event to the avowed purposes of these meetings, which are to consider the long-range effects, psychological and biomedical, of a nuclear war. The more I think about it the more difficult I find this, other than the medical. You will find that Dr. Conard and Dr. Donaldson will have a great deal to say on what the fallout aftermath is for plants, animals and people in a hypothetical or real nuclear war.

To relate the way people behave—and this is one of the more fascinating things about this whole story—to the way people might behave or react during a war, I find very, very difficult, and I think of a proposed experiment that was concocted back around 1949, in relation to the old NEPA Project, to find out how pilots would behave if they realized, when they were flying a plane near where a nuclear device let go, that they had received a lethal dose of radiation. This flight project

was to take a bunch of Air Force personnel to the reactor at Oak Ridge and have them visit it, and while they were within the building and looking at the outside of the reactor, a lot of lights would flash and bells would ring and so on and so forth, and the loudspeaker would go on the air, "Evacuate the building immediately. Everybody has received a lethal dose of radiation." Then a group of psychologists would stand around and see how these people behaved. Of course, it was absolutely unrealistic in terms of the person who was motivated.

FREMONT-SMITH: This was just an idea? It was never done?

DUNHAM: It was never done, but it was very seriously proposed.

FREMONT-SMITH: Especially that "You have just received a lethal dose." Therefore, you may be used in any way we see fit.

DUNHAM: Right. Anyway, I though it might be useful to try to review the context within which these events took place. I think one has to go back to the fact that there was a war, that two atomic bombs were dropped on Japan, and that the Japanese were the only people who have ever experienced bona fide mass effects of nuclear weapons, admittedly small ones. One also has to object. . .

FREMONT-SMITH: It was not bona fide in Spain?

DUNHAM: That's a little different. Wright will tell you what his definition of the effects there is, I'm sure, later on.

FREMONT-SMITH: I just had to throw that in.

DUNHAM: Yes. Anyway, in 1949 the U.S.S.R. did detonate an A-bomb, and I can remember a meeting called hurriedly about getting on with our program. Shortly after that there was this tremendous debate, which is all available on the public record, a large part of it in the Oppenheimer hearings, as to whether or not we should develop the H-bomb. As you recall, both Oppenheimer and Conant had looked at what had happened at Hiroshima and Nagasaki; they then imagined what an H-bomb would do and they were totally incapable of doing anything except sort of turning the other way and saying "We must have nothing to do with it," and "Well, maybe the Russians will build one, but hopefully they won't use it." You know the decision was made to go ahead with the H-bomb program and at the Ivy Mike shot there was the first detonation of a thermonuclear device. It wasn't a weapon but it showed that the whole thing was a reality and possible, and

information that this thing was happening became more or less public around the world. So, when on March 1st there was a detonation at Bikini of something of the order of 10 to 15 megatons, the stage was really set for people to react. People had begun to be aware that there is such a thing as fallout, but they didn't have any real feel for it, and I don't think the military did either. Certainly I didn't.

In the first edition of The Effects of Atomic Weapons (Reference 1), fallout is discussed and not badly, actually, but still I don't think it meant very much to anybody because nobody could really see the problem.

I think one should keep in mind the kinds of people one is dealing with in this episode. On the one hand, one is dealing with Marshall Islanders, a small group of native people who are quite literate but who weren't well educated, and I think this is the distinction to make. They had been a possession first of the Germans, then the Japanese, and then the United States. I think they do not really love the United States. Bob may contradict me on this, but I think he would agree that their attitude had been, "Well, somebody is always going to be poking his nose into our business. We're going to be wards of somebody. The U.S. has been pretty good." So, when something had to be done and they were moved, they took it all very quietly and were totally cooperative. I never ran into a group of people who tried to be more helpful. Just to give you an idea of the kind of people they are—I don't have any slides because I think slides tend to slow up discussions—I'll pass around some pictures of the natives, and you can take a look at them.

In contrast, of course, are the Japanese, a highly sophisticated people, just as sophisticated as we, who had this extra sensitivity to the whole phenomenon of radiation, and who had been a beaten people who were very worried about their relations with the United States and with the world as a whole, but who were just beginning to sort of feel their oats a little bit.

It was within this general framework that these events occurred. I think that one way to set the stage here is simply to read the preface from a special issuance (Reference 2) of the Institute of Chemical Research at Kyoto, which came out in November 1954, six months after the event, and which shows how they set the stage as far as they were concerned. This is all physics and chemistry. There is no medical business in this report because none of the fishermen actually got to Kyoto, but much of the material did.

"On March 1, 1954, at three-forty a.m., twenty-three Japanese fishermen on board the fishing boat No. 5, Fukuryu Maru, were engaged in fishing in the Middle Pacific about ninety miles northeast of Bikini Atoll when a reddish-white flash was seen on the horizon in a west-southwesterly direction, and seven or eight minutes later a loud explosion was heard. Afterwards it was learned that the flash and explosion had been caused by the hydrogen bomb test at Bikini Atoll.

"About three hours after the explosion, fine dust began to fall on the boat. The falling of dust lasted for several hours and ceased towards noon. The boat as well as the fishermen and the fishes caught by them were covered with a white sheet of fine dust. After a two weeks' voyage, on March 14 the No. 5 Fukuryu Maru, contaminated by radioactive dust, returned to Yaizu Harbor, Japan."

It was at this point that the world really began to learn what had happened, although the U.S. had announced that there had been a test on the first of March, and that 236 residents of the Marshall Islands had been exposed to radiation and evacuated to Kwajalein.

Just to give you a visual picture . . .

FREMONT-SMITH: Had there been a sort of a warning to ships and so forth?

RADIOACTIVE FALLOUT AND RADIATION EXPOSURE

DUNHAM: There had been an exclusion zone within which ships were warned not to come, and there has been argument back and forth as to whether the Fukuryu Maru was within that zone. As you recall, the U.S. officials insisted that it must have been within it. It's obvious that it didn't have to be, because in Rongelap, which is way outside the exclusion zone, the doses on the northern part of the atoll were even higher than anything on the ship, and they would have been fatal.

Bikini is about eighty or ninety miles away from Rongelap; the Fukuryu Maru was up to the north, the other side of the lethal zone. At Rongerik, there were fifty air-weather personnel, and 300 miles from Bikini is Utirik. The doses here were roughly 10r-plus.

UPTON: Excuse me, Chuck. What do you mean by 10r? Is this over infinity or a week or a day?

DUNHAM: Infinity dose.

UPTON: Is this a surface air beta primarily?

DUNHAM: No, air gamma.

EISENBUD: Wasn't this up to the time of evacuation, Chuck? I think it was fifty-six hours actually.

DUNHAM: Here, yes. You're perfectly right. These are doses up to the time of evacuation. I'm sorry. The 800r line is an infinity dose. Thank you, Merril. These are estimates of actual doses received.

The air-weather people at Rongerik got 50. These are external. The dose for Rongelap was 150, and some of the Rongelap people who were on the small atoll fishing probably got about 75.

UPTON: Would this be whole-body or to the skin? What sort of penetration?

DUNHAM: This is an estimate of the whole-body dose. It's no better than an estimate, but a great deal has been based on this in terms of what the human blood response to ionizing radiation is.

As you know, there is a great deal of argument centered around that point, which I think is not particularly germane to the discussion today.

BUSTAD: Of course, on your exclusion zone, Chuck, isn't it true that this was related somewhat to the predicted wind direction, and that the wind direction did change so that Rongelap really appeared in the preliminary stages to have been safer than it was because of the wind shift?

DUNHAM: I think the following happened. The original exclusion zone for the test site didn't include Bikini. It went about two-thirds of the way between Eniwetok and Bikini. When they began testing at Bikini, they extended it beyond Bikini. But only what looks like about 50 miles. The exclusion zone was not big enough for what happened.

EISENBUD: Chuck, could I say something relative to this? In fact, might this be a good time to augment some of the background that you have given, which I think might be helpful in setting the stage?

DUNHAM: Yes.

EISENBUD: First let me say with respect to Leo's comment, in which he tacitly assumed that there was a windshift, I'm not sure of that.

DUNHAM: I believe the wind was already changing.

EISENBUD: This is a matter that hasn't yet been documented. It's a strange business.

I was then Director of the Health and Safety Laboratory and was in direct communication with one of our teams stationed in the Marshall Islands. The only wind information I have ever seen came in an official dispatch, at H - 6 hours, which arrived in New York just a few hours before shot time. From my recollection I would say that it would not have required a wind shift to dump the fallout on Rongelap. Unfortunately, the situation has never been documented in a manner that would make it available to many of us who were interested in the exact meteorological circumstances.

DUNHAM: But your comments are predicated on the only hazard being on those two atolls. It had nothing to do with ships out of the exclusion zone.

EISENBUD: That's right, yes.

For many of us, our first exposure to the possibility of massive fallout came in 1951 with two Nevada explosions of the Jangle series. One small surface explosion and one underground explosion took place in the fall of that year. Prior to that time the military doctrine as it was translated to us on the civilian side was that there would never be any point in exploding bombs close enough to the ground so as to get fallout; they wanted to maximize blast, as was done at Hiroshima and Nagasaki. So, only the airburst needed to be considered. Of course, obvious questions were raised, like "Well, suppose one drops to the surface inadvertantly and explodes on the ground, what kind of fallout are you going to get?" or, "Why not put it on the ground if you can make a big crater?"

I suppose that within the military there must have already been a discussion of a military demand for surface and underground shots. Until Jangle we had not really thought about the consequences of a surface or underground explosion. It was widely recognized that the Jangle explosions would produce more radioactive dust than any of the previous detonations including the Tower Shot during World War II. However, it was thought to be unnecessary to monitor the radioactivity beyond 50 miles from the explosion. HASL arranged to make measurements in the annulus of 50 to 500 miles, despite the fact that people thought we would be wasting time. To the contrary, we obtained a good deal of useful information and, in fact, we found that even as far away as Salt Lake City doses were higher than 100 mr. This was certainly revealing, considering that the two Jangle devices were very small. Following these tests several groups took the Jangle data and extrapolated to the multi-megaton device which was then being planned for Eniwetok.

FREMONT-SMITH. What is Jangle?

EISENBUD: Jangle was the Nevada test. It was a code name. This was in November 1951, and a year later they were planning to explode the first large thermonuclear device at Eniwetok.

There was an Air Force officer known to most of you who came up with a rather pessimistic estimate of what the fallout would be like, and he, I think, was probably the first to have predicted that there might be hundreds or maybe thousands of rads hundreds of miles away. Our group in New York came up with somewhat the same conclusion although not quite so pessimistic. However, it certainly did seem that much more extensive monitoring in the Pacific would be necessary than was then being contemplated. The task force saw no need to monitor beyond the atoll of Eniwetok, where Mike, the first large thermonuclear detonation, was to take place. A fallout research program was included as part of the test program but it was limited to about 50 miles from Ground Zero. The AEC, however, did agree that a monitoring program beyond Eniwetok proper could be mounted if support could be found outside the task force. We succeeded in convincing CINCPAC, Commander-in-Chief of the Pacific, who had responsibility for security of the natives in the Marshall Islands, that the fallout should be tracked throughout the Southwest Pacific Ocean.

Then we were given the job of doing it, and after the Mike shot, found there was no fallout. As we reconstructed it later on, based

on water samples, we realized that there was fallout that went into the ocean. The probabilities of hitting those atolls are pretty small. They were a very small fraction of the total water surface exposed.

Well, there was about two years of wrangling over what should be done to Castle, the series we're concerned with here. There was a very, very influential group of people, both among the military and civilians, who insisted that there never was any Mike fallout, that it all went up into the stratosphere and that probably most of it was in outer space, and there even were calculations to prove it. But once again we felt that this had to be looked into. However, because of a very low probability that there would be fallout on these atolls, since they were so small, and a greater probability that it would all go into the ocean, we began to devise schemes for laying artificial islands. This has never been reported, largely because the information got lost in what happened afterwards, but on the day of the shot we actually were off the Florida coast in a Navy-supported operation, in which drums of viscous oil were being dumped from aircraft in such a way that it was hoped that an oil raft would lay on the surface long enough so that fallout would lay on the top and then a plane with suitable instrumentation could swoop down and make measurements.

This worked. The test fallout material was some iron filings that were irradiated in the Brookhaven reactor and dropped on these oil rafts. Plans were under way for shipping large amounts of oil out to the Pacific to lay down these rafts so that we could find out whether or not there was fallout. The idea was to wait until the shot was fired, find out the direction in which the fallout was likely to occur, send aircraft out to drop the oil rafts, then wait a few hours and send the aircraft in again with instruments to see if there was anything on them.

Well, actually, in parallel with that there were instruments put on that island, but those . . .

UPTON: That island?

EISENBUD: On those islands. The nearest one to Rongelap was the instrument put on Rongerik. I think this is revealing because it simply serves to illustrate the tremendous tenacity with which certain people just refuse to accept facts.

DUNHAM: I think that one of the problems is that you see people around Bikini all the time. They stayed there even when the thing was

detonated, and yet in one sense they were exposed to more or less lethal radiation.

EISENBUD: Yes. I think one of the things, in retrospect, was. . .

DUNHAM: Of course, they were in bunkers and that sort of thing.

EISENBUD: But in the Mike Shot the whole task force was exposed and we could have lost 10,000 men. It could have been awful.

DUNHAM: I think that the fact that we were there gave a sense of security. You see, if you looked at the original weapons handbook at a pattern of fallout, and, as Admiral Schyler used to say, "Scale it up," why, you had something. But I don't think anybody took it as seriously as it should have been.

CONARD: I remember that during the Greenhouse Operation, we actually did have quite a substantial fallout.

TAYLOR: Also, after the first shot, the Dog Shot. That is one I've never understood. There was serious enough fallout so that people got a few r, at least.

CONARD: Yes.

TAYLOR: And this was known to a lot of people but somehow it never seemed to have had much of an effect on what happened at Castle. They were tower shots, I guess. At least the Dog Shot was a tower shot. And the fact that that produced quite heavy local fallout was certainly a material indication of what would happen later.

BUSTAD: But isn't it true that the March 1st shot was considerably larger than predicted?

EISENBUD: Well, it's true in part but I don't think the difference is significant.

BUSTAD: Isn't it a factor of two or three or four?

EISENBUD: I think my recollection is that it was considerably less than two. Let me make the point I wanted to make, which was that the instrument on Rongerik, which was an automatic instrument, went off scale at H plus seven hours. This was an instrument which was not part of the Task Force. It was being operated by what was

basically a CINCPAC-supported civilian organization based with the Task Force but not operating as part of it. When the instrument went off scale, the operating procedure called for the aerial confirmation of this and there was not enough interest in the Task Force to authorize sending a plane over the island to see if, in fact, the instrument was working properly. As I recall it, this was delayed about 36 hours. No information beyond the initial dispatches came into the States for about two days. In other words, there was just a complete breakdown as far as information was concerned, in taking the steps that were necessary in order to evaluate the situation, and to take the necessary palliative measures.

UPTON: You say it was delayed?

EISENBUD: I cite this simply to illustrate that right up to the last minute, with the fallout lying on the ground, the people just didn't go up to investigate.

UPTON: You say 36 hours, Merril? Was something done then, and if so, why?

EISENBUD: This is also interesting. The Commission had recommended an evacuation capability up there and this was denied on the basis that it wasn't necessary, that there would not be any fallout; that there just couldn't be enough fallout to warrant keeping ships on station so that they could evacuate natives on short notice. Finally, a plane went up. I was never clear as to why it went up there, but it was up there with a radiation instrument; it flew over Rongerik and found that the radiation levels were high. It was a PBM-1, of that series. It put down into the lagoon and took the American personnel off and then sent information back to headquarters which resulted in an LST, I believe, being dispatched to Rongelap to take natives off of Rongelap, so that the natives were there, I think, 56 hours.

DUNHAM: Fifty-two hours.

CONARD: A plane evacuated 16 older people from Rongelap at 50 hours and the remaining 48 people were evacuated by ship at 51 hours.

EISENBUD: I thought I would give this as background, because it illustrates the incredible disbelief of the subject of fallout that persisted not only up to this point but later on, as you will probably see.

DUNHAM: This was an analogous situation to what was seen in the Army with malaria. They had little malaria units. Every military group had a team, but the commanding officers had had no experience with malaria. They didn't see anything and this poor little malaria unit would cool its heels until they had a great many cases of malaria. Then they would be told to scurry around. I think it's just human nature.

Langham, you seem to be restless there. Would you have anything to add? You're the authority on Dog Shot, by the way, because some of your dogs were there, weren't they?

LANGHAM: Yes, they were. Merril's story to me is almost incredible.

FREMONT-SMITH: That's like life! [Laughter]

LANGHAM: Fallout was predicted for the Trinity test in 1944 by the bomb phenologists, Hershfelder and McGee. Stafford Warren mounted evacuation teams and monitoring teams to cover the potential fallout area. We didn't have to evacuate anybody; we almost did. The arbitrary limit chosen for evacuation was an infinite life-time dose of 50 r. One family approached this limit, and there was much debate as to whether we should evacuate them or not. They weren't evacuated.

WYCKOFF: What happened to the cattle?

LANGHAM: Cattle were burned by fallout at Trinity, and we had experience with fallout at Bikini where there was fallout on ships. I can't imagine anyone thinking that there wouldn't be fallout involved with weapons tests. I still to this day want to attribute the 1954 accident to just a little bit of misconception on the part of the meteorologists. I can't imagine at that time that one would think there wouldn't be a fallout problem with that device if a populated area was downwind from the detonation. So they had trouble, and I can't understand why anyone would have expected otherwise.

FREMONT-SMITH: You know what happens on misunderstanding. It seems to me this is one of the things we have to face. I will give you a little episode. During World War I we had shell shock, a considerable amount of it. It was so reported, and anybody who studied the thing at all knew that we were going to have some kind of equivalent

to this in World War II. So as soon as the first report came out in the Lancet by Sargeant and Slater of the war neuroses of the men evacuated from across the Channel, I came down to Washington to see Lou Weed of the National Research Council about what we were doing in anticipation of the emotional problems we would be facing when we got into this war. He sent me over to the Army Surgeon General's office where I was met by a colonel who said, "Now, Doctor, what are you worried about?" I said, "Well, I'm worried about what preparations we are going to make because we're going to be in this war and we'll probably have a considerable number of emotional problems as a result of the war, and we know from World War I what happened. In World War II, the British have already had it." And he said, "Doctor, you don't need to worry; we'll have no neuroses in the U.S. Army!" [Laughter]

Now, I just want you to know that this is the kind of extraordinary aspect of human nature one has to face, and I suspect that the true story really didn't come out that it wasn't a radiological but a human factor that went wrong. But maybe I'm wrong.

EISENBUD: I can understand why you feel that way. The fact of the matter is that Joe Herschfelder by then was probably back in Wisconsin.

WARREN: Jim Cooney was my deputy at Bikini. Jim, like many others, was not convinced that there was anything to do. He would leave at four o'clock and go to the BOQ and have a beer just about the time the boys were returning with contaminated clothes and hands on the gangplank, and then about dark the algae would begin to rise and we would have troubles with radiation through the hull all night. He thought it was unimportant. He thought we were foolish for staying up all night wondering where the stuff was going in the deep part of the lagoon. When Frank came back with this radioactive sodium, there was a big haw-haw on his part and they almost court-martialed me for exposing Frank's ships to this radiation hazard. And, yet, on the other hand, Jim pooh-poohed the whole operation and thought it foolish to send a destroyer on this crazy downwind trip in the hope of getting some rain-out.

If I may just continue. He was the RADEF for the preceding operation and was the adviser to the Army, and many of the times that I described in the last session, when I was up before the Fleet for explanation in a pseudo-court-martial, they couldn't taste it, they couldn't hear it, they couldn't see it, they couldn't feel it. There were

just these RADEF boys, with their instruments which showed something or other, who claimed it was hazardous and that they were losing their ships and equipment and their gear and their laundry and their possessions. You could understand some of the objections. It was a lot of trouble and it was costly. How do you get a station to stand out in the ocean in the right place? The waves come along in a little while and the fallout which hits the water is gone. Even the SARAR left an awful lot of oil when she sank, and this went on over the reef. It was traced downwind about 60 miles but in ten hours it was gone and anybody going out there then could show that there wasn't anything there and could ask why you were worrying. It was costing an awful lot of money and time. The meteorology was expensive, too, to cover this vast area where there wasn't anything to sit on, and it was very chancy. But they didn't really have the concept of how vast this phenomenon was and what the quantities were. You'll find people, not all of them in the military, who were unwilling to face what might have happened at Alamogordo. Oppy protested our surveys after the war until the white-backed cattle appeared in the Albuquerque slaughterhouse. It took a lot to overcome the resistance to our purchasing of cattle. I don't know if Dunham remembers this because it was partly before his time.

Such antagonism to the concept of the meteorological mechanisms and the vastness of the fallout problems, together with all of the expense and trouble and manpower required for instrumentation and the many safeguards like evacuation plans and public relations complications from excluding ships from this vast area, all combined to make this episode possible.

Then I feel that this was a very fortunate thing to have happened with so little real tragedy involved because actually nobody was really hurt seriously by the fallout.

DUNHAM: I think the most dramatic thing of all is where that 800-rad line landed.

WARREN: Yes.

DUNHAM: It was squarely between the Japanese fishing boat and the Rongelap people.

WARREN: If you had planned it that way you couldn't have gotten it better.

DUNHAM: If it had happened on their own home island they probably would have had a lethal exposure within the 48 hours between the time of the fallout and the time they were evacuated. These were studies that were made by Pete Scoville, * I think (see Dunning, Reference 3), who was one of the principal people involved in actually taking the measurements. They went in there at 36 and 48 hours; they took readings at different places on different parts of the atoll, then went back later, took more readings, and then extrapolated back along the K-constants, and so forth, as to what it would be originally and what the infinity dose would be.

Merril, do you want to comment on this?

EISENBUD: I think it was very difficult to estimate the doses, obviously.

DUNHAM: Yes.

EISENBUD: I've often had a feeling that the doses may have been very much higher than had been estimated, particularly in the case of the Japanese ship.

DUNHAM: Of course, that's a different proposition, because nobody measured them until two weeks later.

EISENBUD: That's right.

DUNHAM: And the ship had been hosed some.

EISENBUD: That's right. I saw that ship March 22, 22 days later, and by that time it was still reading generally about 110 mr per hour, and the Japanese and our own people had had enough of the debris. We knew what the decay-characteristics were, and if we extrapolated from that 150 mr per hour to H plus four hours, the integrated dose was something better than 100 r.

DUNHAM: Yes.

EISENBUD: By this time the ship had been hosed, as you say, and scrubbed and people had gone on with vacuum cleaners to take off as

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* Scoville, H., Jr. At that time Scientific Director, Armed Forces Special Weapons Project.

much of the dust as possible because they wanted the dust for study. So it could very easily have been in excess of 500 or maybe even 1,000 r.

DUNHAM: So it's possible, with this line that I've drawn—and you called my attention to it this morning—on the map, that I've come much closer to the ship than is indicated there; the 800-r line might have been quite close, not 20 miles away.

EISENBUD: The fallout on the ship was estimated to have been 50 curies per square meter, which is going to make some of you wince, but I think it's a pretty good estimate. It was made by the Japanese in a very interesting way. They took surfaces and sprinkled sugar on the surfaces and then asked the fishermen, independently of each other, to pick a surface which looked like the ship at the time of the fallout. The opinions clustered around a certain couple of slabs, and since they had samples of the fallout, they could estimate what the activity was. The best estimate is around 50 curies per square meter, which is quite a heavy dose.

BUSTAD: Wasn't one of the difficulties that some of the crew members swept up the fallout and put it under their pillow?

EISENBUD: I don't know that.

DUNHAM: One of them put some in his pocket, I believe, to take home as a souvenir.

MILLET: Thus far, we have heard that those in charge thought they knew, but they did not. Whether or not the fault lay with meteorologists, admirals, generals or scientists may not be important except to those who want to define history in its greatest detail.

No information reached the United States for 36 hours. There was incredible disbelief that the event had occurred. And disbelief was true not only for this episode, but as Dr. Dunham has mentioned, for malaria, and as Dr. Fremont-Smith said, it was true also for psychoneuroses. It happened subsequently with respect to radiation exposure, as we will hear later in this meeting.

I wonder if there are not really two kinds of psychological features with which we should be concerned: one is the fear of radiation effects among exposed persons, and the other pertains to the psychology that

leads to underestimation or miscalculation of the magnitude of the nuclear event and its psychosocial consequences.

BRUES: This is because we've been brought up to have a two-valued way of looking at things, isn't it? That either we're frightened or we're not frightened. Actually, there are degrees of being frightened.

MILLET: I think one of the very interesting things is what motivates so many people to deny the facts when they are so readily demonstrable. If the data are clear and are presented and they are denied by intelligent people, otherwise intelligent people, there must be some motivation known to them or unknown to them which makes it impossible for them to change their position. This brings us to the question of when is a delusion not a delusion.

FREMONT-SMITH: Right.

LANGHAM: I think it's a matter of biased values. There isn't a man in the field that isn't anxious to get on with his part of the job, and in dealing with these people you find that to them the highest priority, consciously or subconsciously, is to get on with the job; isn't that right, Dr. Warren?

WARREN: Yes.

LANGHAM: Invariably you'll find this conflict. The protection man is obstinate in his way. He wants to do a job right, too. And this is a conflict that's brought about by the bias. The bias is brought about by the position in which the man finds himself.

MILLET: One wonders if there isn't something in our national culture which makes us prefer getting on and moving rather than waiting and listening and finding out. I heard a comment last night from my neighbor here that the American psychiatrists don't bother to read foreign literature, for example.

LANGHAM: We have hawks and doves right now. I think probably insofar as radiation protection and nuclear devices are concerned I might be classified as a hawk. I still think one has to make haste, but with caution. I think in some cases people who want to be cautious may lose and in some cases they may win. At Greenhouse we had a trick played on us which may amuse you. During Dog Shot, at which

we were recovering animals from the shot island, we dressed in complete protective clothing including respirators. We looked like men from Mars. We invaded the shot island to get our animals, and the plan was that when we came back to our home island with the animals we would strip off all our clothes and throw them into a box on the beach and walk up to the quarters in the nude. On the shot island, we could hardly get a meter reading anywhere. In the meantime, a sheer in the wind had brought the fallout right over our home island. When we returned to base camp with our animals, we took off all our clothes and walked in the nude through a hundred times as much radioactivity as occurred on the shot island! [Laughter]

FREMONT-SMITH: That's a wonderful story.

TAYLOR: I would like to interject something that you challenged, Staff. You said a moment ago, you can't hear it. Apropos of the Dog Shot, fallout was clearly audible. There were little beads of steel from the tower that condensed, and one heard this constant tinkle, tinkle of steel from the tower hitting the aluminum roofs and then rolling down the gutters and piling up in little piles on the ground. The thing which I've never understood, which has some psychological significance, I suppose, is that the radiation monitoring teams, pairs of people with a Zeuss meter, would find one of these little piles and you just heard from them lots of expressions of various kinds of bad language about 10 r per hour, 40 r per hour, a few r per hour and a sort of disbelief. The upshot was that everybody kept wandering around. According to a Zeuss meter that Herb York* had set up in one of the buildings just to have people file past to see what their reading was, my own hair was reading 2 r per hour after a shower. Well, I got worried, along with a number of other people. But somehow there was an air of unreality about the whole thing. There was a big discussion about whether we would have a movie that night or not, and somehow they, and no one seemed to know who "they" were, had decided that the movie was all right.

Somehow I've never understood how that could have happened, in view of all the literature that was available for years before Greenhouse on fallout and on how large areas could be covered with very intense radiation. No one seemed to want to believe what was happening.

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* Herbert F. York, then at the University of California.

FREMONT-SMITH: Isn't there a lesson for the whole purpose and goal of these series of conferences in this discussion that's taking place this morning? Human nature is not going to change that fast and we're going to have a variety of conflicts and attitudes, and hawks and doves, with respect to a, b, c, and d, in preparation for the possibility of atomic war. Also, if there ever is a nuclear war, there will be this same kind of confusion and reaction all over the world. So it seems to me that this aspect of human nature, which we're probably going to have to face in one way or another as long as there's human nature around, is one of the central lessons for this whole business. If we're going to get anything out of this, part of it is going to come by the fact that human nature is this way and that there are conflicts in authority, the highest level of authority. You're going to get denial of facts, as Jack brought out; clearly evident facts will be denied up and down and proved not to be so by other authority.

I attended a conference that the Civil Defense put on in which the problem faced by the group in this 3-day meeting was that a bomb has been dropped. This was the assumption, and we were to focus attention on two counties in northern New York State bordering on the of the Great Lakes. According to the assumption, the wind has blown the fallout over these counties and the question is what do you do? Well, the report of this meeting was never published, not, I think, so much because it was classified, but because it was unbearable to have a group of intelligent people about as confused as we were. We ended up with a terrible wrangle as to who was to milk the cows! [Laughter]

So, I think that among the lessons is that there's a lack of a logical approach to the realities of the problem that can be counted upon no matter where we stand. I would throw in one little touch, and that is that we are all aware of the fact that the weather every once in a while turns out differently from what is predicted.

LANGHAM: I would like to refer back again to the conflict of interest on the part of scientists trying to work together. Each man's ego is tied up with his job.

FREMONT-SMITH: You are right. Our multi-disciplined conferences are bringing this out every day.

LANGHAM: I have a rather amusing story that illustrates this. I don't know whether I ever told Dr. Warren, but he kept getting messages from the colonel on Eniwetok who was in charge of putting the droned B-17s through the clouds at Bikini.

WARREN: Yes.

LANGHAM: Under remote control these B-17s had been flown through the bomb cloud. They were not destroyed but were slightly radioactive. The colonel wanted to take the remote control equipment out and use the B-17s to fly his crew back to Honolulu. He asked Staff to send a man over to clear the planes as radiologically safe.

Staff sent over two people and before the monitor would get back, this man would be on the radio again asking Staff for a decision. Dr. Warren finally came to me and said, "I don't know what's happening with that guy. I sent two men over and he's still bothering me. Will you go over and find out what's bothering him and get this thing straightened out?"

I went, and as I came down the ramp at Eniwetok, standing at the bottom of the stairs was the young colonel who looked about 25 years old; he wasn't as old as I by 10 years or so. When I came down the stairs, these were his words, "Are you that radiological man?" When I said I was, he pointed to the B-17s and continued with, "Well, sonny, they're there. Don't give me any of this crap about milliroentgens. Do I fly them home, or do I push them in the ocean?"

The highest readings were in the cockpit where there were several radium dials and on the engine intake and exhaust manifolds. I came back to the colonel and, in my most efficient manner, announced, "Fly them home." With that he said, "Come with me. We're closing out the club." I stayed there four days and wasn't sober a minute! [Laughter] It never cost me a dime!

Here is a specialist, good at his job. So you've got a psychological conflict right here that, I'm sure, stems back to the ego and the fact that the man doing the job satisfies that ego by filling it well.

FREMONT-SMITH: And you satisfy yours and therefore went to the club! [Laughter]

LANGHAM: That's right.

WARREN: After 20 years I've got an explanation why he was so long gone! [Laughter] This is why I made such a tremendous effort to save the Independence. The Navy had towed her to Mare Island. She was seriously contaminated by the underwater blast. The Navy

had been unable to clean her enough to get the radiation down below our 24-hour level. Twice I went to the 12th Naval District where she was berthed to persuade the commanding officer to delay her sinking temporarily. She was a fine example of general contamination inside and out, and would have been a fine training resource.

The first time an inexperienced person walks into a situation where he's surrounded by contamination and the meters show it, he can hear the buzz on the Geiger counter, he realizes he's in a hazardous situation and he's either prepared or not prepared to deal with it. But he should be prepared and he can be prepared to deal with it and conduct himself with some safety. We needed a place like that, a real situation as this ship represented. But they finally took it out and sank it. I think part of it was to get it out of sight, out of mind.

FREMONT-SMITH: "Let's forget about it."

WARREN: It was a hazard they wanted to forget.

DUNHAM: Maybe we should move on from this background as to why the Task people behaved as they did. They behaved in some ways very much like the Command in Hawaii, when the little fellow running the radar at the ack-ack installation at Pearl Harbor reported he saw some planes coming in.

FREMONT-SMITH: Exactly.

DUNHAM: I think as far as the Rongelap people go—and if anybody wants to disagree, they can take this up right here—that until one comes to the end of the line almost, there's no particular psychological problem. They were dealt with, I think, well. They were put in good barracks and taken care of. They were probably given too much to eat and had good medical care and there was very little protesting. Isn't this generally the situation, Bob, as far as the people are concerned? They were not enthusiastic about having to leave their atoll but they bore with it. They were not having any aberrant psychological responses.

CONARD: This is generally true. There were a few psychological reactions resulting from the fallout situation on Rongelap after they were moved back to the island. I will refer to these later.

DUNHAM: They still didn't really know what happened. They were told that something happened. They were told that they had to

have their hair washed and that they had to stay away from home for a while.

LANGHAM: How did they respond to this?

DUNHAM: This is all second-hand from talking with them. One of them, the "doctor," what was his name?

CONARD: Jabwe.

DUNHAM: Jabwe, the "doctor," who had some training, decided the water maybe was getting contaminated, and I think he forbade them to drink water after the first few hours.

CONARD: But they did anyway.

DUNHAM: They did anyway. Some of them went swimming to get the stuff off. Again I don't think it was a panic reaction. There was nobody to tell them this was radioactivity; there was nobody to get them excited, and it had happened. I think one of them who had been in Japan somewhere along the time of the Japanese occupation, recollected that it looked like snow but, of course, wasn't cold. I don't want to steal your thunder for your afternoon session, Bob.

CONARD: They had seen previous shots.

DUNHAM: They had seen the light.

CONARD: And this was nothing unusual except it was much larger than anything they had previously seen, and they described it as the sun rising in the West, I think.

EISENBUD: They wouldn't have seen the Eniwetok shot in 1952.

CONARD: They saw others.

EISENBUD: Yes. It was my recollection that the Eniwetok shot certainly was about the same size as Bravo, wasn't it?

DUNHAM: In 1952?

DONALDSON: No, no. A little less than one-fifth.

THE FUKURYU MARU (LUCKY DRAGON) AND THE PROBLEMS IN JAPAN

DUNHAM: I think we should go on to the Japanese fishing boat. Ralph Lapp, you know, has written a book (Reference 4) on this subject and there are some pictures in it of the boat and the crew. I'll pass this around for anybody who hasn't seen it. It was an old tub, not up to modern Japanese fishing boat standards, but I think it did have a radio aboard and that the radio was in constant communication with Japan throughout this whole two-week period. It's not at all clear that anything was ever said about this episode in conversing back and forth.

FREMONT-SMITH: You mean they didn't report it to Japan at all?

DUNHAM: No.

FREMONT-SMITH: Not until they got in?

DUNHAM: Not until they got in. Anyway, the Japanese fishermen actually developed skin lesions, which Bob will describe quite vividly for you with pictures, as appeared in the Rongelap people, perhaps a little more severe and the distribution somewhat different, particularly along the belt line because they were all wearing trousers and apparently collected a lot of the stuff right where the trousers were tied. The people are described as looking black, and you can almost sense—Ralph tells a good story of this part of it—how the almost panic situation developed over a period of 48 hours.

FREMONT-SMITH: After they got the fallout.

DUNHAM: After they got the fallout.

UPTON: Were they unaware until then that they had been exposed? Is it clear from the log when they first became aware that they had been exposed?

DUNHAM: They saw the flash. They had the fallout.

UPTON: Did they know at the time?

DUNHAM: Yes. There was no question that they had a general idea exactly what the whole story was and they hot-footed it straight home. They made a bee line home, which in itself is significant.

FREMONT-SMITH: Did they know they were in danger?

DUNHAM: I don't know if they knew how much danger. There were various degrees of concern, and what they were thinking at that time, I don't think we know. Ralph interviewed a lot before he wrote the book and he was there three years or two years later, which is an after-the-fact recollection.

UPTON: You speak of panic, you mean among the crew or among everyone concerned?

DUNHAM: No. This was a broad panic almost involving Japan as a whole. I want Merril to make a real contribution now because he was right there. When they monitored the ship, they found radioactivity. They found that the fish, at least the top fish on the catch, were contaminated. They began throwing the fish away. Then the next thing anybody knew was that within a week or so they had thrown away a million tons of fish; almost anything that came from anywhere. They would monitor the run and they would say, "Oh, boy, it's reading," and right into the sea it went. Merril, you were right there and you saw what happened.

EISENBUD: This whole story has the same element of the Rongelap fallout. For example, there's no official report of it, which is surprising. I don't think there is one of the Rongelap fallout; at least I've never seen an over-all comprehensive report covering the thing from beginning to end.

FREMONT-SMITH: This is extraordinary, isn't it?

EISENBUD: Yes.

DUNHAM: What kind of a report do you mean?

EISENBUD: Well, I mean that ordinarily you would expect that an incident of this magnitude would involve setting up an investigating team and putting out a report which would be available to the people who are involved. For example, I never wrote a report on my own experience in Japan beyond the first two weeks because I just waited and waited, presuming I was going to be able to fit it into some sort of over-all report.

DUNHAM: You mean a report on the episode, how and why?

DUNHAM: This document here is an after-the-fact one.

EISENBUD: That's right. Normally you would expect, for example, that the meteorology would be described, including the development of wind patterns starting a day or two before and running right up to shot time. This is not available. I asked for it before I came down here and it's still classified. So I couldn't bring it with me.

FREMONT-SMITH: You mean it's available but classified?

EISENBUD: Yes, right. This would simply mean that nobody has taken the time to declassify it, which takes work.

DUNHAM: I think Merril has a feel for the way this thing built up in the Japanese press that nobody else in this room can have. I hope that he will just devote a few minutes to this, starting with, say, throwing away the fish from the Fukuryu Maru.

I have a few more visual aids which I will pass around. You can look at them at your leisure. There is a record by Holmes & Narver* of the repatriation of the Rongelap people, and it has nice pictures of them and their habitats.

The only thing really wrong about it is that the pictures of the original houses were taken after two years of total neglect and they are not nice, well-kept-up homes such as Bob Conard and Cronkite put in their report, which were pictures taken immediately after the event. But otherwise I think you'll find these interesting.

The other things I want to pass around are pictures of Mr. Eisenbud and some of his Japanese friends. This is the July 17, 1954 issue of the Saturday Evening Post, with an article (Reference 5) entitled "The Grim Facts of the H-bomb Accident." This was out at about the height of the fever both in this country and in Japan. It starts: "Shortly before noon of a sunny day last January began the most famous voyage any Japanese ship has made since the battleship YAMOTO undertook the dramatic suicidal sortie from the Inland Sea." It shows pictures of Dr. John Morton examining the fisherman. It shows pictures of Merril wandering around on the deck of the Fukuryu Maru. Please treat it gently because it's my only copy.

*The Holmes & Narver Co. was contractor to the Joint Task Force and rehabilitated the islands of Rongelap and Eniwetok on the Rongelap Atoll. The document referred to was never published.

It may not be apparent from articles like this or from Ralph Lapp's book how much rapport developed between the Japanese scientists and people like Merril, John Harley, Lauren Donaldson and others who worked closely with them and tried to help them sort facts from fiction. It was a very close working relationship, and as evidence of this, in the special issuance (Reference 2) of the Institute of Chemical Research at Kyoto, which is a special issue on the dosimetry, radiochemistry, and so forth, it says, "Furthermore, we should like to acknowledge with deep appreciation the kindness of Dr. John H. Harley, Chief of the Analytical Branch, Health and Safety Division, New York Operations Office, U.S. Atomic Energy Commission, who provided us with much valid literature concerning the metabolism and internal dose determination of fission products." Many of their articles have a similar acknowledgement at the end of the article. I think this is important to keep in mind; in spite of all the public panic, hoopla, newspaper reporting, personal accusations and unpleasant things that may have occurred on the streets, there was, among the disciplined, thinking scientific community, a great deal of wholesome and constructive exchange.

With that as sort of an introduction, I'm going to ask Merril first to tell us a little about his experiences in the development of the problem over there. Then Lauren can tell us something of his experiences. He was sent over at the request of the Japanese as an expert on fisheries and radiation. Finally, I hope we will have time for a little bit from two people, Dr. Schull and Dr. Miller, who were at the time with ABCC, which was peripherally involved, and that they will give us a little picture of how they got dragged into the thing.

Keep to the same ground rules. Everybody interrupt, if you want to.

WOLFE: Before you start I would like to know just what the date was that the U.S. society found out about this fishing vessel.

EISENBUD: Well, it's a good place for me to start. They found out the way the world found out, when the ship put into port.

WOLFE: That was two weeks after?

EISENBUD: Yes. It was the 18th, I think.

DONALDSON: The 17th.

EISENBUD: The 17th here, the 18th there, I think.

DUNHAM: March 14th, precisely two weeks.

EISENBUD: Then I'm wrong.

WOLFE: You mean our people didn't know that ship was out there?

EISENBUD: That's right. If you've ever been on any of these sweeps, you could understand why. It's a big ocean and the radar isn't very effective on a small wooden vessel.

DUNHAM: Remember how long it took to find Eddie Rickenbacher.

EISENBUD: Yes.

WOLFE: He had the winds blow in two directions.

EISENBUD: The boat put in, I thought it was the 17th but you say it was the 14th, and I think the first newspaper accounts were on the 16th, as I recall.

DUNHAM: Right.

EISENBUD: Now it comes back to me.

FREMONT-SMITH: The Japanese?

EISENBUD: The Japanese newspaper accounts were, of course, picked up all over the world. Consistent with the pattern right from H plus 7 hours, the initial reaction here was disbelief, that this was just a propaganda stunt, that there would be nothing to it. Dr. John Morton, who was then director of ABCC, was dispatched pronto up to Tokyo to help out and telephoned me in the middle of the night.

FREMONT-SMITH: Where were you at this point?

EISENBUD: In New York. He told me that he would need somebody who could evaluate the physical facts. There was no one there at the time. I tried to catch John Harley, who had just left Japan, but I couldn't intercept him, and it was finally decided that I should go there myself.

FREMONT-SMITH: How long did it take you to get there?

EISENBUD: Well, apparently I'm not too good on the dates. I flew straight through. In those days it was about 40 hours. I think I got there around the 19th or 20th, 48 hours later. There was a lot of confusion everywhere. You've got to remember that 1954 was the end of a very bad time for the Japanese. It was nine years post-war but the upturn really hadn't begun. They were two years past the Peace Treaty. The scientific community wasn't organized. The Japanese had no instruments, not even Geiger counters. Also, there was a lot of jockeying for position among the Japanese.

Well, I went very innocently myself. Actually I was all packed for going into Eniwetok anyway, and within an hour I changed my plans and left for Japan and had no contact with anybody until I got there. When I got there, there must have been a thousand people with signs at the airport, and I wondered who the big shot aboard was; I found out it was I! [Laughter] Somehow or other, through this telegram, they had word that I was coming and were picketing. Some American MPs had been permitted to come to escort me into a limousine, which was right at the foot of the ramp.

Well, this of itself was very bad. A number of Japanese had come out to the airport to meet me, some of whom I knew quite well, but I wasn't permitted to see them. They had waited for hours, and I was put into the limousine and whisked out to the Embassy so that I could brief the staff. So that was the beginning.

The Japanese had no way of getting the basic information that they needed. They knew nothing about bombs; there was no way in which they could get, for example, information on the fission products that you would expect, the debris, and what kind of activation products would be present. On the other hand, the next morning one of the first people I saw was Doctor Kimura, who was one of the first radiochemists who actually had been a student of radioactivity, and who in 1945 was the one who had taken soil samples from Nagasaki and Hiroshima and concluded that there was plutonium in the Nagasaki bomb, based on his analysis and what he read in the newspapers.

By the time I talked with Kimura the next morning, he had already analyzed the debris and had detected uranium-237, which led him to the conclusion that there must have been an n2n reaction which involved the fast fission of uranium-238. I mention this because at that time this was a very sensitive fact in our weaponeering and here I was sitting with a man who had deduced something in a couple of days that was known to very few people in the United States. So you

see the situation I was in, trying to be helpful and at the same time trying to protect information that other people thought should be held secure.

I think that at that particular point in time the whole difficulty with the Japanese, as far as the public relations problem was concerned, could have been solved. The main thing that the Japanese wanted was a statement that our government was sorry.

DUNHAM: Didn't one of the fellows get involved with the accusation as to whether or not they were within the exclusion area, so that it was a long time before the powers in Washington would agree that it was perfectly possible that it wasn't within the exclusion area?

EISENBUD: That's right. I think it was clear, and this was reported, that they really couldn't tell, and that the navigation equipment they had wasn't very sophisticated. The log looked authentic but they could have been five or ten miles on one side or the other.

One thing that impressed me through this stage, which I've often remembered as other crises developed and as I think about our people that were participating, is how tired you get. I flew straight through in 40 hours in a very excited condition wondering what it was going to be like when I got there. I arrived at two o'clock in the morning of, I guess, the end of the second day. I was whisked to the Embassy at two in the morning and stayed in conference for about 2 hours. I went home and got into bed for the first time in 3 nights; I had 2 hours sleep and then went off for the first conference with the Japanese, and met all day. I made a point of getting to bed early that night, but with the 12-hour difference in time, John Bugher was just about ready to telephone me along about ten o'clock at night, and this pattern kept up for 4 or 5 days. I was really at the verge of exhaustion, but I had to make a decision.

FREMONT-SMITH: Yes, which is very difficult to do in that state.

EISENBUD: Yes. And I don't know whether or not I made the right decision; I mean, somebody else would have to evaluate this. But when I think of the Cuban crisis and the Berlin crisis, and of the very few people who were at the center of this thing and who had to think despite the fact that they couldn't get their rest. I think it's a problem that someday the government is going to have to deal with. Chuck, you may have been in the middle of this many times.

DUNHAM: It's not uncommon.

EISENBUD: The relationships with the Japanese were . . .

FREMONT-SMITH: The safety of all the world can rest upon the judgment of somebody who is exhausted, who has to made a decision.

EISENBUD: Yes. There were some obvious snafus of a very minor nature which seem amusing, but might not be. Maybe there are some that I don't know about that were not so amusing. For example, on the third night Tsuzuki who was down at Yaizu—there were a few fisherman down there—passed word, through one of the others, that I should call on him immediately on his arrival that night. He was coming in at eleven o'clock that night. This seemed like a strange time to be asking me to call on him, but I checked with this fellow who seemed to speak good English and he said, "No, Dr. Tsuzuki wants you to call on him at his home." So the Embassy provided a car and at eleven o'clock I was up at Tsuzuki's house and, of course, he came to the door in pajamas. He was expecting a telephone call! [Laughter] This illustrates another problem, that is, that the fact that a man thinks he can speak English can be very dangerous. The difference between "call Dr. Tsuzuki," which I would take as meaning that he wants me to telephone him, and "call on him," which means that you visit, is a subtle one which you can't expect all Japanese to understand. So I emphasize this as another thing that complicates a situation which is already complicated. He was very gracious; he had a bottle of Scotch and we sat up and had a fine chat.

I would say that the political situation was stalemated by the fact that the Japanese Government was very anxious to settle the thing amicably and were willing to cooperate in any way. They were willing to enter into an official agreement with the United States that would relieve us of any further financial responsibility. But they insisted that we had to say we were sorry. So while this was going on, John Morton and I were concerned with the more technical aspects, and it is commonly said that we weren't allowed to see the fishermen. This is not so. They didn't want the American doctors to examine the fishermen, primarily because of what was being said in the American press and by some Americans in Japan, including a couple of Congressmen, to the effect that there was nothing wrong with these fishermen and that it was all a hoax. There were two members of the Joint Committee on Atomic Energy that came through Japan, saw these men a few days after they arrived, saw the burns, decided that these were superficial and made a public statement to the effect

that the whole episode was being exaggerated, despite the fact that at that time the blood counts were dropping at an alarming rate.

So, the Japanese understandably were reluctant to have Americans publicly come in and check up on what they were doing. On the other hand, John Morton and his staff were given every courtesy. They looked at the blood; they stood there while the blood was being sampled. They could poke the fishermen and talk with them. I myself got involved in this in a peculiar way. I think you'll find it on the front page of that Saturday Evening Post article (Reference 5) where it says that I wasn't allowed to see the fishermen because I wasn't a doctor. Quite the reverse is true. I went to Yaizu to see the ship and had no idea of seeing the fishermen because it was almost an impossible situation. I had been told that the hospital was a small hospital, that the patients were sitting on mats on the first floor, that there were hundreds of people milling around and that there must have been 40 or 50 reporters, and I didn't see how it could be useful for me to go to see the fishermen even on a courtesy basis, although I was anxious to make some physical measurements on them.

Well, at lunch that day the Mayor of Yaizu indicated quite strongly that the fishermen would be hurt, knowing I was in town, if I didn't come to see them. So, I did go there and I made enough very superficial measurements to ascertain that their thyroids were very hot. I took samples of their hair and asked for some skin scrapings, which I took with me. These were sent to New York and analyzed subsequently.

DUNHAM: I'm interested in what you said about the relations with Morton and yourself because a lot of Americans got very upset with the idea that the Japanese didn't invite you to take over.

EISENBUD. That's right.

DUNHAM: You weren't invited: there's no reason why you should have been. As George Le Roy* said, how would we feel if the situation had been reversed and a couple of so-called experts from Japan came over and were to demand total access and taking over of the

* Dr. George LeRoy was then on the faculty of the University of Chicago and was consultant to the AEC and to the medical team that was responsible for the care of the Rongelapese.

treatment, and so forth. But this was the way it was played up in the press.

EISENBUD: I got samples of urine and blood, for example.

DUNHAM: Surely.

EISENBUD: Well, we made a considerable amount of progress in the first week. I had set up a sort of formal organization for investigating this. There was a Japanese committee established and Morton and I were invited to all the meetings, and then something happened which was heartbreaking and which is a matter of public record. Of course, the American press at that time was very much involved. There was a furor at home. So, it was decided that the President would go on television and make a statement to the public. He did this with Admiral Strauss and there were two things in that statement which were very offensive to the Japanese and that caused things to deteriorate so far as Morton and myself were concerned. One was the statement that the burns that the men had—if I'm not giving this in correct context, Chuck, say so—were not due to radiation but were due to lye produced when the coral was calcined in the fireball and then fell out on the fishermen.

DUNHAM: I can remember when this hit us. We were at Kwajalein. I could see the expression on Cronkite's* face when he read this.

EISENBUD: Yes. This hit the Japanese papers with the full knowledge . . .

FREMONT-SMITH: Where did the idea come from?

EISENBUD: It certainly didn't come from me, but everybody else thought it did.

CONARD: The fallout material was indeed caustic, though this did not cause the "beta burns" that later developed.

FREMONT-SMITH: You just made a nice excuse.

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* Commander Eugene P. Cronkite, of the Naval Medical Research Institute in Bethesda, Maryland, was in charge of the medical team.

EISENBUD: And I was completely discredited because it was generally known that I was sending daily reports and State Department telegrams as to the technical facts and they had every right to assume that this idea came from me. The other statement was that the Japanese were presumably inside the danger area.

Well, this, coming straight from the horse's mouth, so to speak, widely publicized, nationally televised, and presumably an authoritative statement, made it very difficult for John Morton and me to be effective any longer. I stayed on, I think, for about two weeks after that but it was obvious that very little was going to come of it.

Actually, I stayed on for the two weeks primarily so that I could see some contaminated fish. We worked out a method for monitoring which is not easy to do because there were literally hundreds of thousands of fish piled up on the docks waiting to be shipped.

FREMONT-SMITH: These were all fish from this ship?

EISENBUD: No. The fish on the Fukuryu Maru were confiscated immediately. They were buried and forgotten about.

FREMONT-SMITH: Had they been measured?

EISENBUD: No.

FREMONT-SMITH: They were never measured?

EISENBUD: No. They were dug up and. . . no, they weren't measured.

LANGHAM: I'm sorry, Merril. I can't keep quiet any longer. Again, your story sounds incredible to me. It's not that I don't believe you. I do, because I've been through a similar exercise. It is just that the public reaction to a radiation incident is incredible.

I think that we should be studying the psychology of government relations with governments. Will you please tell me why such a fuss is made over something of this nature? If a G.I. in Japan had accidentally killed two or three people with a carryall, this wouldn't have made any news at all. Why isn't it fashionable to admit a mistake when it involves radiation? Do you mean to tell me the greatest nation in the world can't say, "Okay, we made a mistake"?

FREMONT-SMITH: We can do so anywhere except in radiation. That is holy. That is part of our religion. We are the radiation people and we don't make mistakes in radiation!

LANGHAM: The Air Force every now and then hits a section of apartment houses in an airplane crash. Does that ever get the publicity that this did, and why do we have to worry so much about the American image when I think this country can afford to admit an occasional mistake and not particularly lose face? Yet, I know what Merril is saying is indeed true, and I maintain that what he went through, what the government went through, is indeed true. The question is what's the psychology behind this type of thinking? Why do we feel this much emphasis is necessary when radioactivity is involved?

FREMONT-SMITH: I'm not sure that we did very much better in Spain. We'll come to that later. Maybe there is a tradition here of making this kind of mistake between governments! [Laughter]

LANGHAM: It doesn't make sense.

FREMONT-SMITH: I think past history—and I'm afraid the future history—removes the incredible . . .

LANGHAM: How many accidents have we had in foreign countries before in which the President of the United States felt obligated to make a statement?

MILLET: It's an evidence of power in part.

FREMONT-SMITH: Yes.

LANGHAM: Why? Why is radiation unusual in this case?

MILLET: What about Vietnam?

LANGHAM: Vietnam is a different thing. Let's look at something that's comparable. Wasn't it not so long ago—well, a few years ago—a military plane on takeoff plowed through an inhabited area in Germany and killed several people?

UPTON: Chuck did mention panic developing in Japan at the time.

LANGHAM: Why panic over radiation? That I don't understand.

EISENBUD: Wait a minute, Wright. Everybody knows that a plane can crash into an apartment house and kill people.

LANGHAM: Doesn't anybody know that it's possible that fallout can?

EISENBUD: This was never announced.

DUNHAM: It didn't come out clearly because there was no public announcement about this.

FREMONT-SMITH: At least three things are wrong, or maybe four.

UPTON: I don't think there's any need necessarily to defer discussion to Saturday if it's pertinent now. Isn't that right? This is a free-wheeling kind of a meeting.

EISENBUD: Let me finish the Japanese story.

LANGHAM: Let me clear up one thing. My saying that Merril's story is incredible doesn't mean that I think Merril is incredible! [Laughter]

FREMONT-SMITH: We think he's incredible! [Laughter]

DONALDSON: Merril, at this point may I inject a comment about the fate of the fish?

EISENBUD: Yes.

DONALDSON: The fish from the Fukuryu Maru were buried at Yaizu and subsequently were dug up and sent to various laboratories.

EISENBUD: I'm glad to know that. I was unaware of it.

DONALDSON: Pieces of these fishes have been drawn and quartered and analyzed and reanalyzed, again and again. So there is at least a great fund of evaluations by individual Japanese of the contamination of these Fukuryu Maru fish.

EISENBUD: Good. I'm glad to know that.

FREMONT-SMITH: And they were contaminated?

DONALDSON: As Merril said, some were. It was not uniform and it was the type of contamination which we had never encountered and have not encountered in all the years working in the Pacific. It was not absorbed, but adsorbed radiation, which came from dragging the fish across the deck. This external superficial contamination or surface contamination was easy to measure with the usual radiation instruments, while the internal selectively absorbed radionuclides, so characteristic in the subsequent samples of the March 1, 1954 test, were not found in the tissues of these tuna. You have two types of problems as far as radiation contamination is concerned.

TAYLOR: With these fishes?

DONALDSON: They stopped fishing and began picking up their lines. Therefore, you don't know just how much radioactivity came from contamination in the water and how much was from actual fall-out on the deck.

BUSTAD: With regard to your second statement relative to the crew being in the wrong position, in Lapp's book he states that the crew felt they had been detected by the American authorities. I assume he obtained this information from the crew, didn't he? I mean, this feeling?

EISENBUD: Yes. Well, they thought they were probably going to end up in jail again. You see, they had been in jail probably two months or so.

DUNHAM: They had been in jail in Indonesia.

EISENBUD: Yes, for poaching.

Well, what happened next? Maybe, Lauren, you have better information than I do on this. It's my recollection that the American shipping companies took the position that they would not accept any fish for transport to the United States that was not certified by the American Government as being acceptable for entry into the port when it arrived on the West Coast, and this is what caused the great tuna panic of 1954.

DONALDSON: That was part of it.

EISENBUD: Part of it? What was the other part?

DONALDSON: Well, it's a rather long story. Maybe we can come back to that later.

EISENBUD: Okay. So when that happened, the Japanese immediately needed guidance as to how they could obtain certification, and we worked out some quick screening procedures that seemed to be all right because, frankly, we didn't find any contaminated fish, at least during the period when I was there. They were, however, dumping fish. Reports were coming in that this or that boat had dumped its load of fish because it was found to be radioactive. We arranged with the Japanese Government that no more fish would be dumped until I had a chance to look at them. I h[illegible] helicopter and could go anywhere. But these reports would co[illegible]n and one by one they proved to be erroneous. The only explanation that seemed credible at the time was based on a knowledge of the tuna people that a certain fraction of the Japanese boats would come in with defective refrigeration gear and the fish would be spoiled. Normally this would be a loss to the company, but now they had an out. If the refrigerator went bad, all they had to do was dump their fish and say that it was radioactive and then make a claim.

Well, this went on for several weeks. But I did not . . .

DUNHAM: Maybe at this point we ought to ask Lauren, because by this time he had been called overseas.

EISENBUD: Yes. When did you get there, Lauren?

DONALDSON: May 24th.

EISENBUD: I left May 19th. So I didn't even know you were there.

DOBSON: May I ask a question about the earlier period, please? You had said, Merril, that the Japanese did not have Geiger counters and measuring equipment. You mean that all during this time they had practically no way themselves to monitor?

EISENBUD: That's right. They had prewar equipment. I had brought with me some scintillation gear and presented it to them and this was the first time that they had actually had a scintillation counter in Japan. Of course, now they make excellent testers, as you all know. But the original measurements were made with very primitive ionization chambers by Nishiwaki and a couple of others. So I was very much surprised by the fact that our own military people had

very little equipment in Japan. This was Korean war time; 1954 was right after the Armistice, I guess, or just before it. But anyway, it was a tense period. I went to Japan on the assumption that there would be fully equipped radiochemical laboratories in our military establishment, but as near as I could find they didn't exist. So I had to send samples all the way to New York to get them analyzed.

DOBSON: So, when one looks at it from the Japanese point of view—and Japan is a busy country with many ports and a great deal of fishing—at least up until the time that you got there, Lauren, fish were coming in off of many boats in many ports and there were very, very few pieces of equipment in Japan that could be used for surveys?

EISENBUD: Yes. We provided the equipment. We had some Geiger counters.

DOBSON: But how many ports could you inspect?

EISENBUD: My recollection is that there were about 16 ports. Is that right?

DONALDSON: I really don't know, Merril.

EISENBUD: It wasn't any larger than this. It might have been 12 or 14, but it wasn't very many. It wasn't a large number of ports.

WARREN: But you couldn't be everywhere with your single equipment.

EISENBUD: No, but we trained the Japanese. We had, I think, some 30 pieces of equipment flown in and they were able to make measurements. Their plan was that when they found radioactive fish they would phone Toyko and I would fly down and take a look at it.

DUNHAM: Maybe we should ask Lauren why he was pulled over there and what he found in the wake of Eisenbud's visits in terms of public relations problems and relations with the scientists.

DONALDSON: Well, maybe we can go back to the beginning which, I guess, was March 1, 1954.

During each of these test operations our group was busy in the Pacific, studying the biological effects of the radioactivity. Quite in contrast to Merril's statement, which I'm sure he didn't mean—he

said there was no fallout—we know it just went into the ocean. This doesn't mean there isn't fallout. I'm sure you didn't mean it, Merril. It just didn't fall out on . . .

EISENBUD: Land.

DONALDSON: Yes. Just to clarify this one point.

The fallout into the ocean in this case presents an entirely different group of spectra as compared to the fallout on the land, except for the Japanese incident—and this is important: the Japanese get about 90 percent of their source of protein food out of the sea, so it doesn't make any difference whether it's tuna fish or clams or oysters or what not. The Japanese are greatly concerned about radiation in any form, that is, with respect to the contamination of any food that they get from the sea. You have this unique, almost hysterical background of the Japanese people regarding radiation from their experiences during the Hiroshima and Nagasaki bombing, and along with that, fear of airborne contamination is almost a mania with the Japanese. One always sees them with a face mask when they have a cold.

The problems of actual measurement of radiation in the sea were further complicated by the question of where it went. Also there was the resistance on the part of the Task Force to understand what we felt, to shape up to their responsibilities, to actually get busy with the measurements. It wasn't until March 26th that we got the first expedition underway, that is, 26 days after the event the first expedition went into Rongelap to actually do some rather thorough surveys. Even this attempt was hampered by Task Force orders calling the destroyer back for patrol duty while we were still on the contaminated islands.

DUNHAM: Lauren, I think you ought to get back to Japan.

DONALDSON: I will in just one minute.

DUNHAM: I don't want to steal your afternoon thunder at this point.

DONALDSON: The levels of radiation were in the order of magnitude of 100 curies per square meter on Ebeye Island on March 26th, so we're talking about appreciable amounts of radiation.

All right, now over to Japan.

EISENBUD: That's 100 curies extrapolated to March 1st.

DONALDSON: That's right. Extrapolated back to March 1st as measured on March 26th.

EISENBUD: That's right.

DONALDSON: The situation in Japan. I was sent there on two assignments. One, my direct responsibility was to help in any way possible to aid the Japanese fishing industry and the people who were responsible for the management of that industry. Two, I was to aid in any way in providing information on actual radiobiological problems. However, as it turned out, about 99 percent of my efforts were devoted to the field of public relations, as Merril has indicated. This was the real problem and one was faced with it day by day. The port of Toyko was in tremendous turmoil because there were mass demonstrations against the Americans. This was true at Nagasaki; it was true at Yaizu. There were banners, and this is a direct quote, "It doesn't take a bullet to kill a fish seller. A bit of Bikini ash will do the job." Well, this seemed a bit out of context at the moment, but in the area we're talking about I think it does make sense.

If we take tuna fish alone, during the spring of the year the Japanese eat about a million pounds of sishimi, or raw tuna fish, a day. It's a delicacy to them, and it's part of the ceremonial tradition of Japan to have sishimi in preparation for the Emperor's birthday on April 29th.

On March 17th, when the news of the Fukuryu Maru incident was publicly announced in Japan, the tuna sales dropped to practically zero throughout Japan. If we take a concrete illustration, there are over 1,000 fish markets in Tokyo alone, retail fish markets. Many of the merchants come on their bicycles, buy a tuna fish in the market and carry it to their shops. A tuna fish then cost about $35 American money. The sale of these fish represented the sole source of livelihood for the small shopkeeper. They didn't sell the tuna fish, so it decayed and they had to bury it. That was a month's pay or their livelihood. This went on for some days and thus their source of income was stopped. This situation for those people was economic disaster. Or maybe you had a boat that went out to sea and had been gone for six weeks or up to three months; you returned with a load of tuna fish that would be sold to pay off the expenses and the fishermen. But the tuna fish wouldn't sell, not because it wasn't fit to eat

but because: (1) the Japanese wouldn't eat tuna because of fear; and, (2) the United States committed an unfortunate faux pas, as Merril indicated, in saying, "We will not import this tuna fish unless it's certified." This was ridiculous on our part but our tuna fish industry was adamant and they were extremely vociferous in reiterating, "We're not going to be subject to the economic ills of Japan. That's their problem," although, of course, our nation was largely responsible for creating the environment that made this problem.

The fishing vessel owners, then, and the crews, were subjected to economic disaster.

I think we were inclined to minimize the overall sociological and emotional impact of this sort of thing upon a people whom we normally should consider our friends after the war. We did not, however, take into consideration the overall impact of this unfortunate event.

Thus, during the first few days we assumed this trauma would disappear, but there were certain other very real problems within Japan, which, I am sure, have never been documented. Merril left about the time it was becoming increasingly evident that the press—always antagonistic—was willing to grab some bit of news and immediately blow it up into a big headline. This was a great problem in Japan. Very carefully planned sessions were held with the American Embassy staff and with the Asiatic section of the Japanese Foreign Office, and very carefully laid plans were developed to handle situations as they arose; we discussed all aspects of the situation. Then there would be big headlines in the Japanese press: "The nara [kelp] is contaminated with radiation." This radiation problem was discussed at the meetings but the levels were not publicized. Surely, you could measure fallout by this time in the onshore drift. It was detected in small amounts; this had been discussed, but it would be blown up to a big headline.

So you have this weird conflict, our failure to face up to what we felt were real responsibilities, to do what Wright suggested, make a forthright statement, "This is what happened—period," which was not done. Mass hysteria spread through Japan, a country where this could happen because of the previous experience of the Japanese; in addition, there was an attempt on the part of some to discredit any move in the way of a solution or to disrupt anything which might contribute to a logical solution. All of these interacting factors tended to

prolong and prolong, indefinitely, this mass hysteria into a very real international problem

CONARD: I would like to add a postscript to what Merril was saying in regard to the examination of the fishermen.

In 1964 I was invited to go to Japan to examine the Japanese fishermen. I think this is the first time since you were there, Merril, that this invitation had been extended. When I arrived there I was surprised also, as you said, with the amount of press coverage, a large number meeting the plane. I was taken to the American Embassy and they wanted to know exactly what it was all about and what we intended to do and say, and so forth. They seemed to be satisfied that everything was all right. And so we proceeded with the examinations at Yaizu. Dr. Kumatori (Reference 6) was the Japanese physician who was in charge of the examinations.

Everything went along fine except that everywhere we went in Japan we were besieged with reporters and television people who made a big to-do over the whole thing. Certainly it was apparent that even at that time, 10 years after the accident, the Japanese were still very sensitive about anything that had to do with radiation and particularly fallout.

ROOT: I think this sensitivity, this continuing sense of outrage, persistently stimulated by the press, and exploited by political parties, stems directly from the 1954 shot and was exacerbated by our handling of it. I was in Japan in 1964. As a journalist I made contacts through fellow journalists with many officials, doctors, and scientists. They were far from reticent in our discussions. They may have been more outspoken with me because I came with their own friends or acquaintances and was not on an official mission or connected with government activity. They told me that the widespread reaction of horror crystallized into anti-American sentiment; channeled into political segments; mobilized women who had never before had any political interest; infuriated the whole country. Many called it the third U.S. atomic attack.

FREMONT-SMITH: This one?

ROOT: Yes, Bravo.

FREMONT-SMITH: More so than Hiroshima?

ROOT: Yes. This had a greater political effect because Hiroshima and Nagasaki were in the context of war—to that extent understandable. This was completely unwarranted—and the U.S. reactions seemed so callous—not even, I was told repeatedly, saying we were sorry, or taking any responsibility.

Furthermore, it played into a tense political situation. The fishermen came back two days before the Diet was to ratify MSA.

DUNHAM: What was the MSA?

ROOT: Mutual Security Agreement—after Korea. It was terribly important that Japan become a responsible member of the organization. The Yoshida cabinet was entirely favorable to the U.S. and it looked as if there would not be too much opposition. Then the fishermen arrived. Demonstrations flared up everywhere. You had the trade unions, three million strong, protesting. The cabinet tried to counteract the anti-American feeling but a tidal wave of anger inundated the country. It was just diminishing when Koboyama died. This was portrayed as a radiation death.

FREMONT-SMITH: This is the fisherman that had the transfusion and the hepatitis?

ROOT: Yes. Japanese doctors give very small blood transfusions, and Koboyama needed a great many.

Timing in Europe was unfortunate, too. At the end of January 1954 Secretary Dulles made his "massive deterrent" speech announcing a radical change in our policy; we had decided that the atomic weapon as a massive deterrent was our shortest cut to peace. In February, Vice-President Nixon stated that we were tired of being dictated to as to time and place, and were going to call our own shots from now on. The NATO countries, Great Britain and the others, were terribly concerned about this. As staging areas they expected any such momentous decisions to be the subject of consultations at least.

To cap the political confusion and dismay in March, came news of the heavy fallout from the "Bravo" Shot. And where did the press get this information? From Tokyo. As you know Tokyo is a very large city. It has representatives from the press of every major country in the world. Suddenly the whole of Europe was flooded with grim headlines—and no explanation from the United States. The first

explanations, when they came, made us look even worse. "The skin injuries might be lye burns—from the unslaked lime of the coral." Dr. Tsuzuki went on the air internationally—a 15-minute speech translated into all Western languages—to describe the injuries. He said it was ironic to tell him that radiation burns might be lye burns, when he had worked all his professional life with radiation, and had been the first to go into Hiroshima. He made a few unpalatable remarks about the ABCC, and about the Americans using the Japanese as guinea pigs.

There was much misconception about the purpose of the ABCC among the Japanese. They did not understand that the ABCC was a research organization and not allowed to treat patients, as that was against Japanese medical policy. For years resentment had been building up because radiation victims would go to the ABCC, be examined and tested for days—and then sent away without consistent treatment. The idea spread, fanned by anti-American interests, that they were being used as guinea pigs to further American science. I was told that this was one reason the fishermen and their doctors refused to permit examination by American radiation experts and doctors.

In England, Prime Minister Churchill was grilled for 7 hours by Parliament with the Members insisting he call the American Government to account, demand an explanation—and the Prime Minister protesting, "I will get only a rebuff. I think we ought to have an explanation but we can't demand it."

The image of the scientist underwent a sad change—and I think this is not simply a literary curiosity. Before 1954, the prototype was Pasteur, Einstein, dedicated men working for human good. Otherwise they were "mad scientists." Simultaneously, as if on cue, after March 1954, scientists became "sorcerer's apprentices" in every European language—English, German, French. "Mad scientists" dropped out of the literature. All scientists are now in league with the devil.

FREMONT-SMITH: They are all mad. Very interesting.

ROOT: I hope I haven't taken too much time. This may be entirely irrelevant.

UPTON: You mentioned earlier, Chuck, that there were a couple of people in the room who were at ABCC then.

DUNHAM: Yes.

UPTON: I wonder if you would like to have them offer comments?

DUNHAM: Yes. Dr. Schull!

SCHULL: I would like to make two observations which I believe are pertinent before I describe the situation in Japan in 1954 as I saw it. First, we should bear in mind that the Japanese are uncommonly health-conscious, and to an extent that some observers feel borders on hypochondria. The face mask, for example, is a ubiquitous part of the winter scene, or at least was in those years.

DUNHAM: They can't outdo us.

SCHULL: Possibly not. The second observation is that there seems to be no history of responsible journalism in Japan. The three large presses, Asahi, Yomiuri, and Mainichi are in a perpetual circulation war and they are generally prepared to take advantage of any situation which might enhance their status vis-a-vis one another. These two factors, when put together, can seriously restrict the relevance of the Japanese experience for a nation with different journalistic traditions.

As to my experiences in 1954, the story begins in the summer of 1953 when there was convened in Ann Arbor a small informal group whose function was to decide whether or not the clinical portion of the genetic studies then under way in Japan should continue. It was our task to determine whether enough additional information could be gained to warrant further investment of manpower and money. The consensus was that this was unlikely; the basis for this conclusion rested largely on the knowledge that many of the exposed individuals were reaching ages at which no further reproduction was to be expected, and hence continued study would merely increase the "control" observations which were already much more numerous than the "experimental." There seemed, therefore, no particularly strong reason to continue the clinical portion of the studies, and I had gone to Japan shortly after the first of the year in 1954 to terminate that segment of the genetics program.

Shortly after I arrived, there was held in Tokyo a review of ABCC's research activities; this meeting was attended by most of ABCC's departmental chiefs and a substantial number of Japanese scientists.

There was still manifested, I believe, some of the hostility which had arisen in certain Japanese scientific circles in the years immediately after the war. Most of the physicians with ABCC, and, in fact, most of the American physicians who went to Japan couldn't communicate effectively with their Japanese colleagues, few of whom spoke English. The language of medicine in Japan has been German, and only recently has English come to play a prominent role in the exchange of medical information. It was not easy under circumstances such as these to establish rapport. The situation with respect to genetics was quite different. This was ascribable to a number of largely fortuitous happenings. First, there was a firmer body of experimental information from which to attempt extrapolations to Hiroshima and Nagasaki, and even to the members of the crew of the Fukuryu Maru. Second, many of the Japanese geneticists of stature at that time had been trained either in the United States or in Europe, and as a consequence we often spoke a common language, namely, English. Japanese geneticists, in general, strongly supported ABCC's genetics program; whereas the endorsement that was being given to medicine, for example, was of a more qualified nature. The absence of a strong endorsement encouraged opportunists and opportunism, and the Fukuryu Maru incident was replete with both.

The emotional climate that was created in Japan when word reached there of the Fukuryu Maru was really a very strange and almost unbelievable one. Rightly or wrongly, I'm inclined to ascribe it in large part to the "devil's brew" to which I have previously referred. The newspapers seized upon the incident and began a drumfire of daily accounts which almost seemed intentionally designed to heighten anxieties, real or fancied. The Japanese government as well as our own had effectively lost control of the situation. The newspapers had "grabbed the ball and were running with it."

I can recall quite vividly some of the headlines which appeared. There was one, for example, in the Osaka English-language Mainichi; the headline said; "WBC counts of fish-eaters rise." It appeared shortly after it had been announced that radioactively contaminated fish had accidentally reached the Osaka market, and that some had been inadvertently sold. A few individuals who had presumably eaten the fish were being studied by local authorities. This headline accompanied a report of their work which, by the way, was unobjectionable. They had carefully indicated that numerous factors could produce a rise in white blood cells, including upper respiratory infections so common at that time of year: they further stated that on this account one could not conclude that the elevation was necessarily due to the

consumption of the fish. This nicety was lost on, or at least ignored by, the writer of the headline. The effect of this article and others like it was far-reaching, however. Shortly after the appearance of the one in question, ABCC was visited by a woman and her daughter who had been in Osaka when the fish were sold. The mother and her child insisted that something had to be done for them. They were really quite concerned, and were certain they had eaten the contaminated fish. We didn't have the vaguest notion, of course, what should or could be done if we assumed that they had, in fact, eaten the fish. If I remember correctly, to ease their apprehensions stool specimens were obtained and examined, and this had the desired palliative effect. At least they left with the belief that someone was interested in their health. This is but one small indication of the near hysteria engendered largely by the newspapers. I'm sure that Bob Miller can add to these experiences.

MILLER: I was too far from the scene and too inexperienced in Japan at that time to be much of a witness as to what was occurring. But I would like to point out that four years later, in 1958, Dr. Schull and I, among others, returned to Japan to make a study (Reference 7) of children who were in grammar school then and whose parents had either not been exposed to the bomb or were too far from it to have received significant exposure. In Hiroshima, of 2,200 children who were invited to come for examination, 97-1/2 percent did come. In Nagasaki, of 4,500 invited to come, 99 percent did so. So, four or five years after the Bikini incident in 1954, there was not much of a hard core of resistance as a result of that experience.

I would like to bring our attention back to Dr. Langham's question just before this discussion began: Why is radiation so evil? I think, since he asked the question, we have heard some of the answers to it. I wonder how he feels about it now, after hearing that the newspapers inflamed the public, the Japanese physicians were jockeying for position, and the governments, both U.S. and Japanese, were unprepared to handle the circumstances and made a mess of it?

LANGHAM: Well, I think this is the evil. No one respects radiation any more than I, but I don't think radiation is an insurmountable thing at all. It may be that the psychological impact created by the press and everyone else concerned is incompatible. This is exactly what I'm trying to get at. All of these affairs get blown into something that is far beyond their real importance. Now, why? Maybe some of the answers are coming now, but I don't think this means that radiation is something we can't live with at all.

DUNHAM: We can live with cranberries and pesticides, but for a while it got blown up all out of proportion, too. It just happens that radiation has created more of these situations than some of the others up until recent years.

EISENBUD: I think that this even is one of the really few important historical events in all of history. We woke up one morning and found that we had bombs that could be exploded if we knew how to use them. It threw our government into such a turmoil that they knew they had to say something but couldn't decide what to say until, when was it, Chuck, that the first real statement came out?

DUNHAM: Well, the first release containing any details came out nearly a year later, February 15th, or something like that, of 1955.

EISENBUD: It took a year for your government to formulate a position. This wasn't because they were dismissing it or that this wasn't important, but it was because they couldn't agree on what their actual position was.

UPTON: It seems to me we have here a very real concrete evidence of disaster. We have fishermen who are sick; fish that have to be thrown away and in turn, a ban against the importation of fish that aren't certified; economic disaster in Japan; newspapers which are eager to play up sensational stories; political groups who want to make capital out of this. There's certainly every element of a problem. The difficulty was assessing the magnitude of the problem soon enough.

EISENBUD: But, you see, there's one element that hasn't been brought out. That is that anyone could take that diagram and lay it on a map of Europe, let's say, by putting Bikini near some important Soviet airbase, and point the wind anywhere you choose to, and get 800 r per hour running through friendly nations. This is why I say we have bombs which we are probably no longer in a position to use; imagine the impact of this possibility militarily.

UPTON: But at the time, surely the dimensions of that zone were not known very generally, so that the Japanese couldn't really be sure how widespread the contamination of the sea might have been.

EISENBUD: Ralph Lapp, I think, published the first of these diagrams, and it seems to me it was in the Bulletin of Atomic Sciences

within a year. I think it actually preceded our official announcement, as I recall.

FREMONT-SMITH: We're not the only government that didn't know how to handle a radioactive accident. If you will all remember how the British Government fumbled the Wind Scale accident, announcing beforehand that there was no danger of any kind at all and then gradually having to admit that there was more and more, and then the milk all had to be dumped. I think that our lessons are there, but I think every government gets caught in this kind of thing or is in danger of getting caught in this kind of thing. But the first thing to do on the government's part is to deny that anything dangerous has happened, which is almost standard procedure, and then gradually it leaks out, whereas actually this is the way that people lose faith in the government. The credibility gap gets bigger and bigger, and I think certainly this is true in this country. If something happened and if we had a firm announcement from the government of this, the people of this country wouldn't have much confidence in this.

WARREN: This is true in industrial practice, too. If somebody let's loose a noxious chemical, they deny everything and then face the issue hours later or days later. The trouble is that their insurance figures are involved and the cost of paying off is involved, and they want to keep it as limited as possible. We are somewhat in the same frame of mind at the government level, aren't we?

TAYLOR: It seems to me it's a very, very important fact of life that the worldwide public has lost confidence in the official spokesmen of the governments of several nations as a result of a consistent denial . . .

FREMONT-SMITH: Of the truth.

TAYLOR: . . . of the truth by spokesmen for these governments, and that's the state of affairs that now exists.

FREMONT-SMITH: Then we are also talking about the credibility gap between the younger generation and the adult generation in any country which is part of the same thing. We have lied to the youngsters repeatedly, again and again, and the youngsters don't have any confidence in the adult world. I think it's a very broad problem we're talking about. This may be true in a good many other countries, too.

WARREN: And yet, as Wright says, the information is always there.

FREMONT-SMITH: What?

WARREN: The principles on which these decisions could be made have been there from the beginning.

FREMONT-SMITH: Right.

EISENBUD: I don't think it sinks in.

CONARD: I get impressed with the ignorance of the lay public with regard to the simple facts. When you talk to a group, it's obvious that they just don't understand the simplest things about radiation.

TAYLOR: I claim they haven't been helped by the official spokesmen, at least in the United States. They've gotten very little help at all because the very first words that were published were, "Don't worry. We know what's being done." Then followed Castle, the situation in which the natives were seriously irradiated, and yet obviously we didn't irradiate the natives on purpose. Obviously we didn't know what the hell we were doing. This has happened so many times. We deny the fact that we didn't know what we were doing, but there is no basis for confidence any more. I think that is central. I think that this central fact, that the public has, on the basis of the record, a positive lack of confidence in what they are told, is going to have a profound effect on what happens.

FREMONT-SMITH: In the future.

TAYLOR: If one or two explosions or a whole lot of explosions really start taking place in anger you will get irrational behavior which is a result of irrational behavior, namely, the way in which it's been handled by the U.S.

ROOT: We go to the other extreme in assuring the people that democracy can only exist on the basis of an informed public, that the public has a right to know. After the 1954 incident there were big headlines in London and other countries proclaiming "Ike Demands Candor. Ike says the people who are going to be subject to this and whose taxes pay for this have a right to be consulted as far as secrecy permits; that everything that can be told should be told." There was a great wait and then the British papers asked, "Where is this

candor? The United States population is waiting for candor." We don't realize here how much is made of that abroad, or how cynically the oft-repeated "people's right to know" contrasts with the official pronouncements when they do come out.

The first acknowledgment of the fallout from Bravo was one sentence: "During a routine test, some Marshallese natives and weather officials were dusted" or some such word. The mystery of that, with no follow up, and then suddenly the Japanese thing, I think is at the root of the fact that people can't even hear the words "hydrogen bomb" without going into paroxysms. What help have they received to understand what happened? As a result of not knowing, a mystique has developed that makes the very thought paralyzing.

UPTON: How long did it take to get the tuna industry back into more or less standard operation?

FREMONT-SMITH: In Japan?

UPTON: In Japan. It was disrupted there for a time. We heard Dr. Donaldson say that.

DONALDSON: It is difficult to put an exact time limit on this problem because the fear flares up or has flared up each time there has been a subsequent test. The pulse of the people is still associated directly with any testing or any announcement of testing. The surprising thing is that the French tests and the Russian tests haven't been upsetting to the same degree.

ROOT: I was in Japan during the Chinese test. The Japanese were busy demonstrating against the arrival of an American nuclear submarine in one of the northern harbors and paid little attention—other than a kind of pleased recognition that the Chinese pulled it off. I was told that the ceremonies commemorating Hiroshima Day would probably have ceased by now because there are few enough interested in going, but the Yaizu fishermen have given it a new and bigger lease on life. An interesting insight was when the Sino-Soviet split came. They had to hold two different ceremonies and Mrs. Koboyama, widow of the man who died, finally refused to go because she was being pulled in both directions. One of the meetings climaxed in heated argument about whether the Chinese Communist Government or the Soviet Government had sent greater contributions to support this memorial. The contributions were openly acknowledged.

DUNHAM: I think it points up again that it isn't particularly right because it's radiation. This is just something simply seized on.

WARREN: This is part of the cold war.

DUNHAM: Not the government people or most of the university people or most of the scientists. The fallout they've had from some of the Chinese tests has not been played up very much in the Japanese press.

TAYLOR: I think the mystique is right here at home, typified by a comment that President Kennedy made to Jerry Wiesner when they sitting together in the White House and it was raining out. Kennedy asked Wiesner whether there was fallout in the rain that was falling on the White House lawn, and Wiesner said, "Yes, there still is." This was an intense emotional experience for the President, to see rain with fallout on the outside; nothing connected with anything in any way quantitative at all. As far as he was concerned, that rain that was falling outside was bad.

ROOT: I think it's a little dangerous to equate radiation with cranberries, though, because we know what radiation can do. There should be a legitimate and respected fear of it.

DUNHAM: I'm not saying it shouldn't be respected, but it happens in certain areas where the psychological seed has already fallen.

ROOT: I think the psychological seed germinates and flourishes because of the ultimate lethal threat.

DUNHAM: The pesticides are lethal. So is radiation.

WARREN: Not everybody buys cranberries and couldn't care less, but everybody is subjected more or less to the fallout.

DUNHAM: So is Vitamin A. It's toxic, too.

MILLET: This, I think brings up another point perhaps. We've been talking about our dissatisfaction with leaders for not giving us the information that we ought to have. I think we're getting into the area of the mystique of the leader in this country, and perhaps one of the great problems hasn't been touched upon sufficiently yet, which is that our leaders are not sufficiently well educated to know what to

think, and therefore, what to act or what to say. They are constantly changing their minds from one position to another, which is one of the problems that is due to their political needs and their careers.

It seems to me we have two ends to work on here: How to get correct information that is capable of solving problems to our leaders and how to educate the public. Now, if the general public doesn't want to be educated, this is something we've got to know, and perhaps we could do more than we've been doing in our educational system to get them to understand the environment in which they are thrust when they are born. We can only do a limited amount in getting them interested in the world in which they live. On the other hand, the leaders are certainly very interested in the world in which they live. Perhaps this is the primary goal for our efforts, to try to get the proper knowledge to our leaders.

WARREN: What you are saying is that our leaders don't have the proper father image for the community of the world at large, and in this case the father image has been tarnished if not destroyed.

ROOT: But they always talk the right father image. That aggravates the problem.

MILLET: Yes.

DUNHAM: Isn't it one of the fundamental problems that leaders, almost by definition, are amateurs? They've never faced a particular crisis until they face it.

FREMONT-SMITH: That's right.

DUNHAM: This is a dilemma that the world has been facing for a good many years and I don't know how you can just suddenly say that these people are more stupid than somebody else. It's a personal problem, as you hinted at.

FREMONT-SMITH: And the thing is partly compounded by the election every two or four years, which means leaderships change or there are desperate efforts to maintain leadership at any cost, because that's the time you'll be able to really show your responsibility, after you've been re-elected.

DUNHAM: Yes.

FREMONT-SMITH: So you've become irresponsible in terms of the election, hopefully in order to be responsible later, and the thing goes on in a vicious circle.

ROOT: I think the professionals have not demonstrated any greater aptitude than the amateurs. It was President Eisenhower who said, "We must give an accounting of this. We must let the nations know." He was sensitive about the NATO reaction and the public reaction. He wanted as much information released as possible—to help them understand. But State Department rules are rigid. Certain formulas determine our dealings and interchanges with our own people and with other countries. Those are the things that are sterile and constricting. I think if more responsibility were left to the amateur who has the confidence of the people inasmuch as they put him up there, and to the man in the affected area who knows the customs and the temper of the people concerned, there would be less suspicion and hostility in times of crisis. The sad part is that though the crisis passes, the feelings tend to persist.

WARREN: I would like your consultation and that of your confrere on your right, because this is what we're really talking about in this whole meeting. So I don't expect to get an immediate answer on this, but isn't this an opportunity?

ROOT: I know that I can get more information abroad, as Congressman Morse pointed out in the security hearings, about situations abroad, and about situations at home than I can get at home. I think that we have one of the most hysterical, panic-ridden attitudes toward releasing information of any free country.

FREMONT-SMITH: On account of security, on account of classification.

ROOT: This delusion of grandeur impedes scientific progress and destroys public confidence. . .

DUNHAM: I would like to challenge this.

ROOT: . . . because it's really going to destroy us.

DUNHAM: You mentioned the ineptitude of Wind Scale. I've seen what the British atomic authority releases, and some of the things they don't release in the way of information. And if you think we are . . .

ROOT: No. I know Wind Scale.

DUNHAM: Not Wind Scale, because it all came out. The British public never even hears about it.

ROOT: It didn't even come out about Wind Scale because, as the person who told me called it, of a failure of management. He said, "You can count on management to fail because they are protecting other values. Wind Scale has never been accurately explained, and they are doing it."

I think the British Government picked it up from us. They used to be much more open.

FREMONT-SMITH: Yes.

DUNHAM: I don't know if it's all our fault.

FREMONT-SMITH: A good share of it is our fault, a good reasonable share.

DUNHAM: The British don't publish a lot of the kinds of information on radiation exposures that we've published and things like that.

FREMONT-SMITH: Look what we've done. What is tolerable radiation dosage in industry? We've had to lower the amount year by year. Instead of coming out with a cautious statement and then finally coming out year by year and saying, "Yes, we can tolerate a little bit more," it's been in the opposite direction, hasn't it?*

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*WYCKOFF: It is of interest to document this decrease. In 1936 the Committee now called the National Council on Radiation Protection and Measurement (NCRP) recommended a provisional "tolerance dose" of 0.1 r per day, but suggested that a "generous safety factor" be applied (NBS Handbook 20). By 1949 the NCRP was recommending a "permissible dosage rate" of 0.3 r per week (NBS Handbook 41). The rationale for the reduction was contained in NCRP recommendation of 1954 (NBS Handbook 59). The differences were attributed to different types of measurement (surface dose initially and at that time to dose in the organ of interest), to a large variety of radiation sources and to a greater knowledge of the biological effects of radiation. However, it was pointed out in that document that these recommendations

DUNHAM: It has been.

FREMONT-SMITH: I think this is part of the same thing we're saying. Say they announced a kind of thing that would make everybody feel more comfortable and then they found that they were wrong?

DUNHAM: Yes, but some have gone up.

FREMONT-SMITH: Yes.

DUNHAM: Some have gone up and nobody says boo. The British do the same thing. They wait until there's an international agreement on it before these things are changed anyway.

FREMONT-SMITH: Still I think the essential feature is that I don't think one can be very proud of the way we have dealt with the public in terms of . . .

DUNHAM: I think there's a great deal of holding back, but to say that the British are so open or so frank with their people compared to us I think is a lot of nonsense because I know just how frank they are not.

FREMONT-SMITH: I'm against the British! [Laughter]

DUNHAM: I love them.

FREMONT-SMITH: I know. I'm teasing.

DE BOER: It is not a question of secrecy alone. In this week's Industrial Research, Admiral Rickover characteristically criticized the Navy and contended that the Navy had gone "downhill." He listed three things: (1) the so-called "new religion" of cost effectiveness studies; (2) the "Zero Defects Program" which he equated with "motherhood;" and (3) "the unwillingness to assume responsibility," as the

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*(cont'd) excluded consideration of genetic changes manifestable in future generations. Additional information on genetic effects and possible shortening of life span obtained from animal experiments and human exposure at considerably higher doses indicated a further reduction in 1957 (Addendum to NBS Handbook 59). The exposure of a larger fraction of the population was also involved. It should be pointed out that no relatable effect has been observed for any of these levels.

cause of this phenomenon. It is particularly the third reason which has a direct tie-in with secrecy. In eleven cases before a Senate subcommittee, secrecy or security was claimed, while trying to identify the man responsible for making certain decisions. The facts were that after days and days of digging, the decision maker could not be found. Everyone was hiding behind someone else.

FREMONT-SMITH: Who are you quoting?

DE BOER: Rickover. These were eleven cases in which the responsible man was never found. In other words, something was originated, like a contract, but nobody was willing to assume the responsibility for that contract and say: "Here I am. I originated that contract and I was right in doing so because at that time, etc. . . . ." No, there was always someone who could say: "I was told to do so but I can't divulge the source." This comes close to secrecy although it is not officially labelled so.

EISENBUD: I think Chuck Dunham is correct when he says that generally throughout the Atomic Energy program there's been a candid policy. I don't think we need take the time to explore it unless you want to. I think the policy has been a candid one, but there's something different about this particular instance and it doesn't necessarily involve the Atomic Energy Commission in this respect. The fact of the matter is that when I learned that Miss Root was working on the historical implications of this matter, I referred her to a package which I had left in the New York operations office in which I pulled together all the documents that I thought would be useful to somebody someday; I left it with instructions that it shouldn't be dispersed. Most of this is pretty innocuous stuff, things like meterological reports, teletypes which give you the time when various decisions were made to do various things and a long series of telegrams of several pages a day which I sent from Japan, which was the only chronological record of what went on. I've forgotten it; I don't remember it. I forgot that they dug up the fish, which I was reminded of, and I learned yesterday that this stuff is still classified; there's no hope of getting it out. That's been sent to Washington because, on my suggestion, Miss Root asked for some of the material in that packet. It was sent to Washington for review and it's still there. How do you explain this?

FREMONT-SMITH: It will take them years to declassify it. They haven't got a staff to do it.

EISENBUD: Yes. Let me tell you something else. I thought we had access to all the information we needed at the time. I think we did, if we had asked the right questions, but sometimes you didn't seem to ask the right question. It wasn't until a few days before this shot was scheduled to go off that I actually knew that it was going to be at Bikini and not at Eniwetok. Nobody told me they were going to move to Bikini; most of my planning had been done on the assumption that it was going to be at Eniwetok and nobody told me otherwise.

DUNHAM: Yet the tower was being built all the time.

EISENBUD: Yes, but we were preparing in New York, and actually it could have been disastrous if it weren't for the fact that through a stroke of luck we had instruments at Rongerik Island. But, based upon our own meteorological projections we assumed it was going to be fired from Eniwetok, and you may say that's a dumb thing to do, but it never occurred to me as to where it was going to be fired.

FREMONT-SMITH: There's an old religious phrase of "Need to know," out of the Bible, and I'll give you an illustration: Norbert Wiener, who, as you know, invented cybernetics and who was also working in a highly classified bomb situation during the war, told me personally that during this highly classified work he ran into a discovery which he knew to be of great importance to another highly classified group. He spent two years trying to find a way in which he could tell them what he had discovered and he was never able to do it because he couldn't demonstrate the fact that they needed to know. In other words, he was never able to tell them.

I also have a hunch—and I don't expect to have it confirmed locally—that the Manhattan Project would never have been accomplished if all security had been protected. I suspect that a number of people told each other things and then discovered they had a need to know afterwards, and that's the way the thing got off the ground in several instances. But anyway, I really bring this up to point out the devastating effect—Norbert Wiener is only one example, I have several others—of this principle.

I would like to add one thing. I really do believe that, by and large, and undoubtedly there are exceptions, our own scientific advances and our own security have been set back by our security more than if we had been much more open. I think we have blocked our own advance by failure to make available to scientists a lot of information

which they could develop and then lead into new directions, and that if we were to release this information, even though it would be perhaps of use to the enemy, we would be getting ahead faster and gain more by the release than we gain by the protection. This is my personal opinion which I throw out for nobody else's use.

ROOT: I think, going back to Dr. Warren's question, that applies also to the press. You say you get "on the beach" and you have "no place to hide" and you get all these distorted reports. But what are those writers going to do? I unfortunately have a disciplined background, having been a research scientist myself, which holds me up terribly in this profession. But everything is a struggle. And journalists and writers eventually give up. There are very few instances, I think, in which, if a subject is entirely in the open, there's not great cooperation between the scientists and the writers. It couldn't be greater, and I know Dr. Langham has helped writers at great cost to his own time and energy, I'm sure. But when it impinges on an area which is not necessarily classified, but on one in which there is uncertainty as to classification, I've talked with people and quoted figures and they have stared back as though I had leprosy and could contaminate them. They hadn't known and they would say, "Where did you get that figure? It's never been published." And I would say that it had been published in such-and-such. It's just too great a task. It's a lifetime work to keep up with what is declassified and what remains classified. So, the only way for sanity is just not to say anything. But then we expect the writer to be able to communicate to the public who support the research and who really are an informed public, the strength of the democracy, and he's got nothing to say, but he's got a job to fulfill.

UPTON: I think the morning session has to be brought to a close, and I'm reminded of an amusing anecdote. We've been talking about an information problem, really, and I heard a story about the Wind Scale incident which indicates how frequently in an astonishing situation where one is caught by surprise and has one's source of information down, one has to say something and may not say the right thing.

AYRES: There's a formula called "No comment"! [Laughter]

UPTON: A group of power industry executives and engineers were being flown over the Wind Scale plant and were being briefed by a guide on the wonders of nuclear power. As they crossed the plant in the airplane and he pointed out various installations on the ground, the accident occurred and a big black plume went up out of the stack,

and everybody's eyeballs popped out and they looked at this thing in astonishment and turned to the guide and said, "What is that?" He was just as astonished and bewildered as they, and not knowing what else to say he smiled and said, "Well, you get that, you know!" [Laughter]

SESSION III

THE 1954 THERMONUCLEAR TEST

(Continued)

(Initiator: Robert A. Conard)

SESSION III
THE 1954 THERMONUCLEAR TEST
(Continued)

THE MARSHALL ISLANDS PROBLEM

BRUES: This afternoon Dr. Conard will initiate the discussion without, I guess, telling us where it may lead.

Bob!

CONARD: I have outlined on the board a few topics I thought might be worthy of a brief review and discussion. Also I put down below the main groups that were involved in the 1954 fallout accident with the numbers of people involved and the approximate dosage of radiation that they received. (See Figure 1 and Table 1.)

In discussing the case of the Marshall Islands accident, I think it's important to point out that this represents a situation on a coral atoll and it may be quite different from other fallout situations that might occur. Characteristics of a particular fallout situation depend on many factors such as whether the bomb is detonated over water, under water, over land, the geography of the terrain, the populations exposed, time of fallout arrival, length of fallout, etc. Fallout effects are somewhat different from those produced by direct effect of the bombs. In Japan, for instance, the major casualties came from blast and heat, with fewer casualties from radiation exposure, whereas with fallout it is a purely radiation exposure situation.

In Japan there were psychic trauma, physical trauma, starvation, disease and many complications; in the Marshall Islands the Marshallese people had a minimum of these factors involved. In addition, the fallout produces a more complicated type of radiation exposure in that you have not only whole body exposure but also the exposure of the skin and internal deposition of radioactive materials.

A few other points of comparison with the ABCC studies might be made. The Marshallese groups, of course, are considerably smaller than those of the ABCC studies. The vital statistics are very poor in

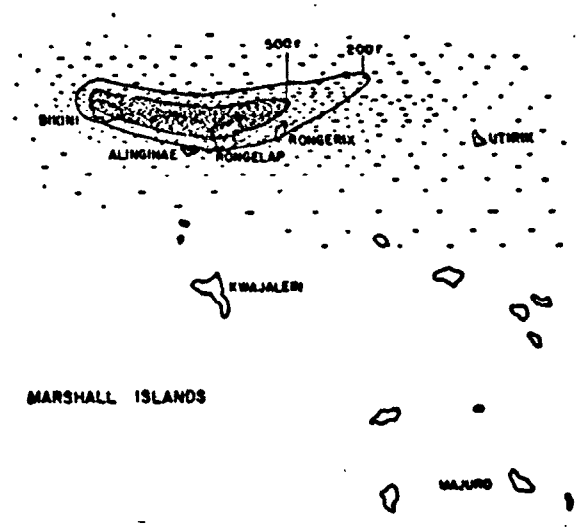

Figure 1. Map of fallout area, Marshall Islands, March 1, 1954. From R. Conard. (Courtesy Annals Int. Med.)

Table 1. Summary of fallout effects.

Group*	Composition	Fallout Observed	Estimated Gamma Dose (Rads)	Extent of Skin Lesions
Rongelap	64 Marshallese	Heavy (snow-like)	175	Extensive
Ailingnae	18 Marshallese	Moderate (mist-like)	69	Less extensive
Rongerik	28 Americans	Moderate (mist-like)	78	Slight
Utirik	157 Marshallese	None	14	No skin lesions or epilation

*Also exposed were 23 Japanese fishermen who received a sublethal dose.

the Marshallese people but the radiation dose is probably better known in their case than it is in the case of the Japanese. Documentation during the acute period was fairly complete for the Marshallese and not so complete for the Japanese. (The Marshallese findings are reviewed in References 8 and 9, the Japanese Hiroshima and Nagasaki data in numerous publications by the Atomic Bomb Casualty Commission.)

The Marshallese population under study is fairly stable.* We go back from year to year and find little attrition. We have an excellent comparison population composed of relatives of the Rongelap people who have moved back to live on the island of Rongelap. They match reasonably well for age and sex.

The Japanese fishermen studies were made difficult by the complexity of the dosimetry, the fact that on board the ship they lived part time below decks, where they were more protected, and part time above, etc. Perhaps later on Merril Eisenbud might say more about the dosimetry in that group. (The data on the Japanese fishermen are reviewed in References 6 and 10.) In addition, it was two weeks before they arrived in port where the situation could be evaluated. Another complicating factor was that during the course of treatment they were given multiple blood transfusions and many of them developed jaundice, liver disease, and one even died, probably as a result of repeated blood transfusions.

Figure 2 is a photograph of Rongelap island taken on March 1, 1954; a typical South Sea Island village with loose palm construction.

Figure 3 is a rough sketch to show the types of radiation that people were exposed to. The wavy lines represent gamma radiation, that is whole-body penetrating type of radiation. The stippled area represents beta radiation which was largely responsible for the skin lesions that developed and also the internal deposition of the fallout material.

The spectrum of the gamma radiation from the fallout was fairly complex. There are quite a few different energy peaks as contrasted to ordinary laboratory studies in animals. The calculations of the

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* The medical studies of the Marshallese are sponsored by the U.S. Atomic Energy Commission and are carried out under the direction of Brookhaven National Laboratory in conjunction with the Trust Territory of the Pacific Islands (Department of Interior).

Figure 2. Rongelap village as it was in 1954. From R. Conard.

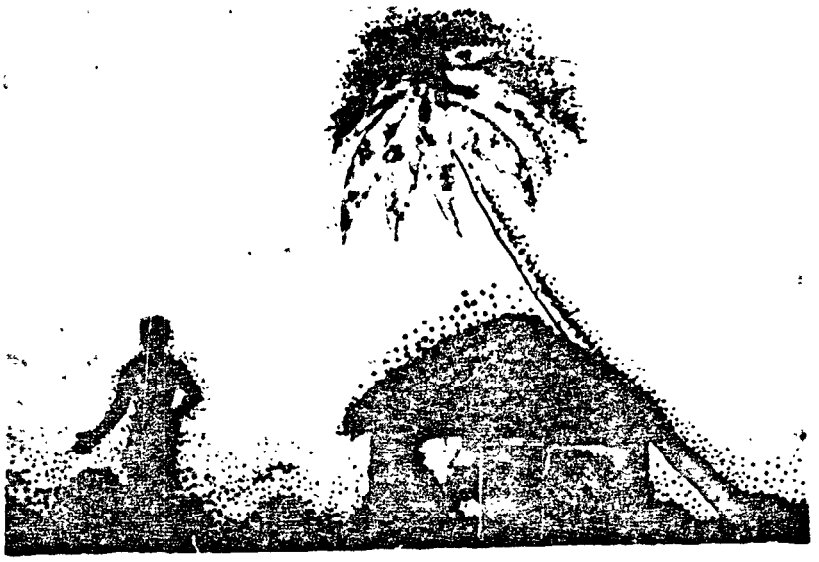

Figure 3. Rough sketch showing fallout deposition. Wavy areas represent gamma radiation, and stippling represents beta radiation. From R. Conard.

gamma dose to the Rongelap people was made on the basis of estimation of time of arrival of the fallout (which was believed to be at about four to five hours after the detonation), the length of time of fallout (which was calculated to be around 12 hours), and by the readings that were taken on the Islands at the time of the evacuation, roughly two days later. There was a telemetering device on Rongerik, as was pointed out this morning, which gave valuable information on the time of arrival there of the cloud of fallout and the 30-minute period that it required to go off scale. In addition, on Rongerik there were many film badges and the readings from these film badges afforded valuable information on the dose and agreed reasonably well with the other estimations.

In the case of the Japanese fishermen, the doses calculated were around 170 to 700 rads based on extrapolation back to Day 0.

Gamma radiation in a fallout field produces a more penetrating type of radiation than occurs with ordinary laboratory uni-directional radiation. Due to the geometry of the planar fallout field, the midline dose is increased by a factor of about 1.5.

So, this really gives a better indication of the biological effectiveness, and we might take the Rongelap dose of 175 rads of whole-body radiation and say that it actually represented possibly 260 rads or so as compared with ordinary laboratory type of radiation. In the case of the Rongerik group, from 78 to 120; the Alinginae, from 70 to 100; and Utirik, from 14 to 20.

DUNHAM: What do you mean by the ordinary type of radiation?

CONARD: I mean uni-directional type of radiation.

BRUES: The numbers you give are rads in air?

CONARD: Yes. These were based on readings three feet above the ground.

BRUES: And midline doses within the person.....

CONARD: .....were derived using the factor of 1.5. The skin dose was impossible to really calculate. As you know the beta spectrum in fallout has quite a smear of different energy components along with some soft gamma. The energy spectrum of the beta radiations

showed about 50 to 80 percent around 100 keV and 20 to 50 percent around 600 keV. So most of it was pretty soft. There was also a beta contribution from the fallout on the ground. It was estimated that the feet got 2700 r from the ground source; at hip level about 600 and at head level, 300. The hair follicles must have gotten in the range between 400 to 700 rads in view of the fact that epilation developed but was not permanent in most cases. The internal radiation was calculated indirectly from urinalyses that were taken starting about 15 days after the exposure and thereafter on numerous occasions. It was estimated that about 75 percent of the radiation from fission products was due to the radiostrontium, radiobarium and the rare earths.

Table 2 shows the various radioelements that were calculated to be in the urine at Day 1 as compared with Day 82. Probably radioiodine is the only isotope that they absorbed that exceeded the MPC level. By 82 days you will note that these activities had diminished to practically zero. These people were able to excrete this material very rapidly.

Table 2. Estimated body burden of Rongelap people (μc).

	Activity at Day 1	Activity at Day 82
Sr^{89}	1.6 - 2.2	0.19
Ba^{140}	0.34 - 2.7	0.021
Rare Earth Group	0 - 1.2	0.03
I^{131} (in thyroid gland)	6.4 - 11.2	0.0
Ru^{103}	0 - 0.013	--
Ca^{45}	0 - 0.019	0.0
Fessile Material	0 - 0.016 (μgm)	0.0

Now I would like to take a minute or two on the thyroid dose because the thyroid situation turned out to be one of the most difficult problems we had to face in these people. The thyroid dose is usually calculated on the percent uptake of the radioiodines by the thyroid gland, the half-life in the gland, the size of the gland, and the various isotopes to which the thyroid is exposed. In the fallout we have iodine-131, 132, 133, and 135. Quite a few isotopes are involved, most of which are very short-lived — the iodine-131 having the longest half-life. The earliest direct measurements were made by Payne Harris* at Los Alamos on 15-day urine. By using this indirect approach from the urine it was calculated that at that time about one-tenth of one percent was still being excreted and this, extrapolated back, gave about 11.2 microcuries in the thyroid gland originally. This represented about 160 rads of radiation to the adult gland, plus the whole-body exposure, of course.

In the children it was a different story because of the smaller size of the glands. James at Lawrence Laboratory (Reference 11) has calculated for us that the children probably received in the range of 700 to 1400 rads to the thyroid gland. It was decided that the beta irradiation of the neck which produced "beta burns" as shown in Figure 4 did not contribute significantly to the thyroid dose in view of the superficial nature of the beta radiation.

Figure 4. "Beta burns" of neck (subject No. 39, March 1954). The area over the thyroid was a frequent site of "burns". From R. Conard.

* Harris, Payne—unpublished data.

To go on with the story, people were evacuated by destroyer, some by plane, two days after the accident and were taken down to Kwajalein Atoll where we had a large Navy base. We arrived on the scene about eight days after the accident to carry out the extensive examinations.

When they arrived they were quite contaminated, particularly their hair, and we had great difficulty in getting them decontaminated.

Figure 5 shows the people out in the lagoon at Bikini with soap and detergents, cleansing themselves. In many cases we had to cut off their hair because of the coconut oil holding in contamination. We had to take their clothes away from them, and some of the women on Kwajalein gave clothes to the Marshallese women to wear. It was quite a sight to see them walking around barefooted in Fifth Avenue types of clothing.

FREMONT-SMITH: No pictures of that?

CONARD: Unfortunately I didn't get any pictures of that.

None of them died. After the skin burns healed, etc., we moved them south to another island temporarily because Rongelap Island was too hot at that time for them to move back. The Utirik people, however, were moved back during this period since Utirik Island had a very low degree of contamination.

Figure 5. Marshallese bathing in lagoon at Kwajalein in March 1954, to decontaminate skin and hair after fallout contamination. From R. Conard.

In 1957, surveys of Rongelap showed that the Island was safe then for the return of the people even though it still had a low level of contamination. Figure 6 shows the new village that was constructed for them, which is far superior to the village they previously had.

FREMONT-SMITH: Did they like it?

CONARD: They liked it very much.

FREMONT-SMITH: This is unusual, isn't it, to have people like something that's been made for them?

CONARD: They had a hand in planning it.

FREMONT-SMITH: That makes the difference.

CONARD: Yes

Now, to go on. In regard to the lingering radioactive contamination of Rongelap, we have carried out extensive studies of the radio-ecological situation and I may say more about this tomorrow. Later I may also comment on some of the psychological reactions to receiving compensation from the U.S. Government, about $11,000 per exposed person

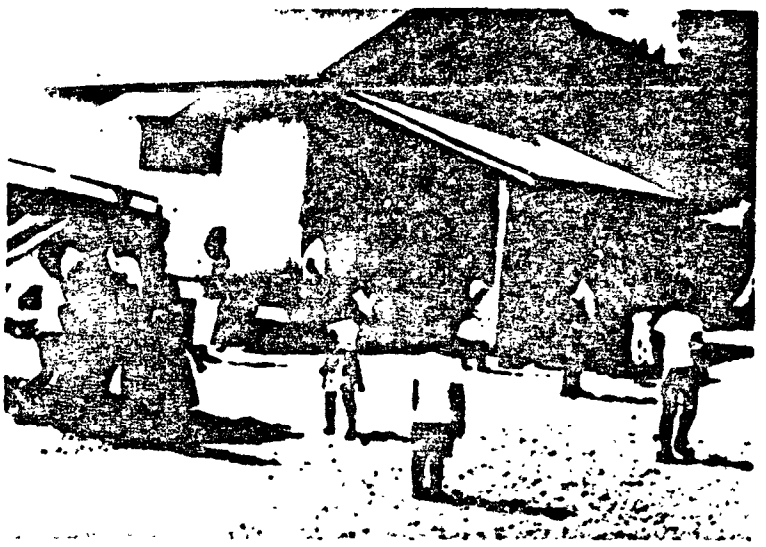

Figure 6. Rongelap village today. From R. Conard.

for injuries sustained. This has made them very happy, I might add.

DUNHAM: What do they buy with that money?

CONARD: Motor boats, things that they get out of the Sears-Roebuck Catalogue and other things. We're just hoping that they are going to spend it wisely. So far they haven't gone too wild with the spending.

DUNHAM: Does each one have an account?

CONARD: The Bank of America came in there and they have deposited their money there in most cases. In a few cases they wouldn't put their money in the bank, but a lot of them are living off the interest of their bank accounts.

WOLFE: When we tried to go out there with the ecological group the Commissioner was very greatly disturbed for fear we would break up their way of life. I don't know of any better way to break it up than to give each one of them $11,000 to buy motor boats and things like that.

CONARD: It was being broken up before that, though. The onslaught of Western civilization was rapidly coming into these islands, and in the other islands it's also evident—not just in Rongelap, where they have this money. You can see signs of advancing changes due to American influence all the time.

I would now like to discuss the acute effects of exposure on these people; first, the whole-body gamma penetrating radiation effects. Just to refresh your minds, you will remember that human beings respond with various syndromes of effects related to dose received. (See Figure 7.) The most acute syndrome, of course, is called the central nervous system syndrome, as depicted in the upper left-hand part, associated with doses greater than 3000 and 4000 rads. Predominantly one sees ataxia and disorientation, signs of brain involvement, and life is, indeed, very short for these people. Then with smaller exposure (above about 1000 or 1500 rads) we have the gastro-intestinal syndrome, so named because signs of nausea, vomiting, diarrhea and dehydration related to the gastro-intestinal tract dominate, and the individual usually dies within a matter of four to nine days from acute dehydration and other effects. The bone marrow syndrome

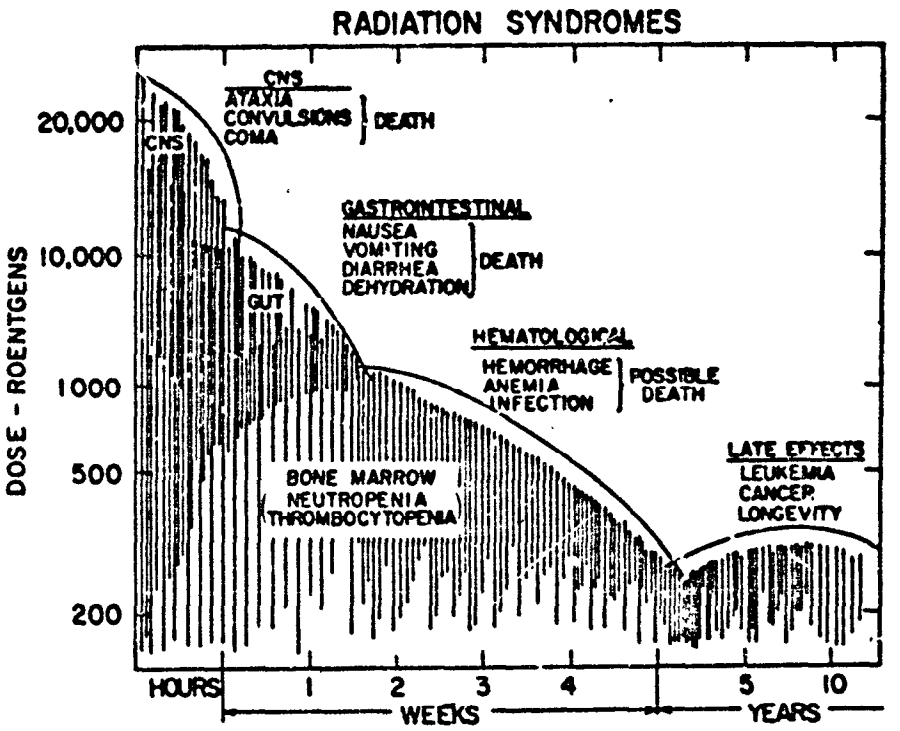

Figure 7. Radiation syndromes (schematic presentation). From R. Conard

or the hematological syndrome is that which occurs following doses in what we call the lethal range. Of course, we really don't know what the lethal range is for man, but it is guessed roughly to be between 250 and 450 rads. Owing to the effect on bone marrow, the reduction in the blood cells results in infections and the development of bleeding results from the blood platelet depression, and death may result.

Then, of course, if there is recovery from these acute effects, there is the possibility of delayed effects of radiation occurring, such as leukemia, cancer and many other possible late effects.

MILLER: I just wanted to mention that the figure makes no mention of cataracts, the intrauterine effects of radiation and the possible genetic effects.

CONARD: It isn't meant to include organ effects, only the major syndromes.

FREMONT-SMITH: It also doesn't say anything about the central nervous system effects of low level radiation which you remember the Russians had always claimed were so and which we have always denied until we recently confirmed it at the Naval Radiological Laboratory.

CONARD: All these syndromes overlap and there are many effects in each of them.

FREMONT-SMITH: I wonder whether there is anything in this group of people in terms of behavior which show that they had any of the low level radiation effects on the central nervous system, which apparently at the level of complex behavior patterns, conditioned reflexes, and so forth, are now recognized to be so?

CONARD: We did not observe any, Frank, and at that time we didn't go into sensitive means of testing this sort of thing. We had many more important considerations. We didn't know whether they were going to live or die, or whether we were going to have to request a hospital ship to take care of them and that sort of thing.

FREMONT-SMITH: Yes. And at the time we were also denying it existed.

CONARD: We weren't.

FREMONT-SMITH: I mean as a government we were.

CONARD: Yes.

Figure 8 shows the characteristics of the hematological syndrome with nausea and vomiting occurring early followed by rapid depression of blood elements resulting in a critical period at the nadir where infection and bleeding may be serious results. Hopefully, then the bone marrow will start producing sufficient blood cells to bring about survival; if not, death will ensue.

In the case of the Marshallese, they suffered from the early effects of radiation. Three-quarters of them became anorexic (lost their appetites), some of them vomited and a few had diarrhea. This occurred over the first two-day period and cleared up after that. When they arrived at Kwajalein they seemed to be perfectly healthy. The Japanese fishermen also went through an early period of fatigue, headache and anorexia, nausea and so forth.

EISENBUD: I think there's one interesting point which also seemed incredible to Wright, but the first dispatch that we got following the evacuation reported that the natives were seasick and nauseous.

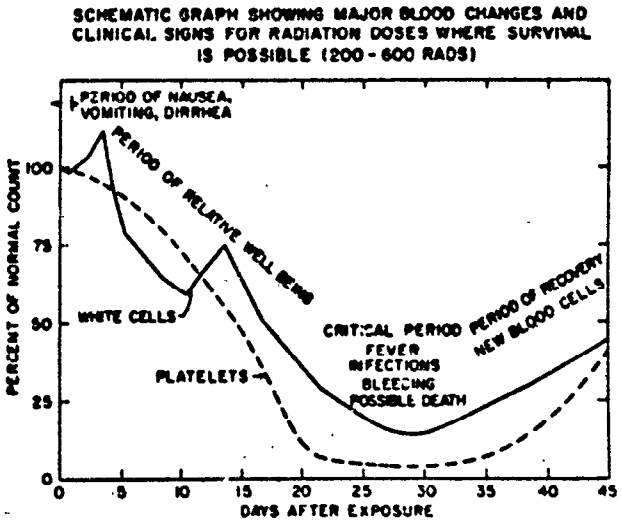

Figure 8. Schematic graph showing major blood changes and clinical signs for radiation doses where survival is possible (200–600 rads). From R. Conard.

CONARD: Were seasick?

EISENBUD: Were nauseous from seasickness.

CONARD: Yes, I think that was the original interpretation, but it soon became apparent that none of the Alinginae, the other group that received less exposure, showed the sickness and since only the heavily expose Rongelap group showed the sickness it was apparent that it was radiation-induced. The blood elements showed considerable depression, down to one-half and more below normal levels, but, fortunately, they didn't get low enough in the Marshallese people to cause any real evidence of infection or bleeding; we used no specific treatment and none of them showed any signs of acute radiation sickness as such.

In the case of the Japanese fishermen, some of their blood elements dropped even lower than in the Marshallese, indicating perhaps a higher dose in some of them. But I would not say, looking at the blood work, that any of them received greater than 500 rad because the depression didn't seem to reach levels that would substantiate that.

EISENBUD: What allowance can you make, Bob, for the fact that they received a dose over a 14-day period?

CONARD: I agree that that certainly would moderate the effect. But most of the dose that the Marshallese and the Japanese fishermen received occurred during the first 24 hours, I would say over half of it, and so it was really more in the acute type of exposure classification.

There was a slight weight loss in quite a few of the Marshallese people and we were not sure whether that was due to their radiation exposure or to the fact that they had a change of environment and were eating different types of food, although they seemed to eat it with great relish. The Japanese, as I mentioned earlier, were given multiple transfusions over a number of days soon after they arrived in Japan, and shortly thereafter quite a few of them developed infectious hepatitis and jaundice and then, of course, one fisherman died in September. It would seem to most of us in this field that his death was most likely due to the blood transfusions that he had received.

DUNHAM: His peripheral blood picture just about returned to normal in July, before he died. The jaundice came on at about that time and he died with essentially a normal blood picture. At least the total count was in the normal range.

CONARD: Yes, that is so.

FREMONT-SMITH: That would fit.

CONARD: But the Japanese have made quite a bit over the fact that this liver disease might be radiation-induced, which is not at all agreed to.

Now, to turn to the skin lesions. The Marshallese had symptoms of itching and burning during the first 24 to 48 hours. This fallout material clung to the skin as a white frosty dust and it was very difficult to remove.

FREMONT-SMITH: Do you know why it would cling to the skin? Why was that?

CONARD: As you know, in this climate the perspiration made it cling and it got caked into the skin, I think.

Ninety percent of the people developed these so-called beta burns beginning about two weeks after exposure. These lesions were first characterized by pigmented skin, increased pigmentation, parchment-like thickening of the skin and gradual desquamation; the epithelial layer shed and a nonpigmented area was left beneath. In some people the burns were deeper, as evidenced in the next few figures.

Figure 9 shows one of the boys who wasn't wearing much in the way of clothing and had multiple superficial lesions of the skin.

EISENBUD: What's the time of this one, Bob?

CONARD: That was between two and three weeks. About three weeks, I believe.

DUNHAM: April 16th.

CONARD: That was quite a bit later. These first appeared on many about two weeks after exposure.

Figure 10 shows "beta burns" of the feet. Figure 11 shows the loss of hair, which occurred in about 90 percent of the children and 40 percent of the adults, and which was usually spotty in nature. There were usually beta burns on the scalp in the areas of the epilation.

Figure 9. Numerous superificial "beta burns" of the skin of a young boy who was wearing little clothing at the time of the exposure. From R. Conard. (Courtesy Annals Int. Med.).

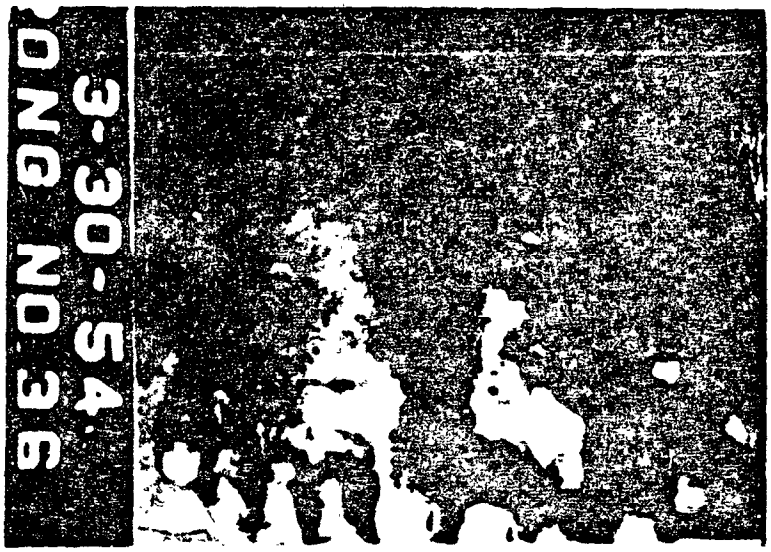

Figure 10. "Beta burns" of the feet. From R. Conard.

Figure 11. Epilation in the temporal area of the scalp of a young girl. From R. Conard. (Courtesy Annals Int. Med.)

WARREN: Is some of this because they slept on the sand without a pillow?

CONARD: Since the epilation was distributed over the head, no more so on the back of the head, I don't think that this was a factor

DOBSON: Bob, you spoke this morning about the caustic action of the fallout Is there any evidence that this played a significant role?

CONARD: I think that it might have aggravated the burns. It was caustic and we know that the caustic chemicals in combination with radiation will enhance the effects of radiation. So, it's entirely possible that this material did enhance the severity of the lesions.

DUNHAM: There was nothing to see for ten days at all. The skin looked perfectly good.

CONARD: Yes. We didn't see any erythema, even.

EISENBUD: Did anybody measure the pH of this material?

CONARD: Yes, I think that's been done. That's the reason it was declared to be highly alkaline. It was incinerated coral, calcium carbonate, calcium oxide.

EISENBUD: Excuse me. I didn't see how it could have helped, assuming it was calcined initially. It was in intimate contact with water quite a long while before it actually fell out. It would seem to me if it was calcined it would be hydroxide. This is an interesting speculation and it's a really interesting point which I hadn't given much credence to. I was hopeful that someone had done some work on this. It's too late.

DUNHAM: There are no notes by the medical personnel about skin lesions and for ten days after we got there we saw none.

EISENBUD: The normal humidity of the atmosphere in that part of the world I should think would result in conversion of the oxide. Apart from that, this whole fireball sucks up enormous amounts of water which eventually cool the fireball; and then there are rainfalls. It just seems incredible to me that calcium oxide could persist for four hours in that atmosphere, in this case seven hours, but this is just speculation.

BUSTAD: I think Chuck Dunham's point is quite a critical one in this case, in that a radiation burn will show up after a considerable period of time during which there may be no manifestation of injury.

CONARD: Yes. This is characteristic of radiation burns, that there's usually a lag after the burn before the lesion shows up as contrasted with thermal and chemical burns.

WARREN: A chemical burn would come within a few hours, 24 hours or so.

BUSTAD: Yes, except with radiation you may have had a transient erythema within a few hours. In comparative studies on small pigs using beta particles we observed a transient redness which disappeared within the first 24 hours.

CONARD: Yes. This was true of the Japanese fishermen, too.

BUSTAD: In the light-colored swine, injury would be manifested in 14 to 21 days.

TAYLOR: Are there any other examples of beta burns to human beings besides the Bikini ones?

CONARD: Yes, there are quite a few.

TAYLOR: Are these reactor accidents?

CONARD: "Beta burns" have been reported in persons carelessly handling fission products (Reference 12) and from exposure to other radioactive sources (References 13 and 14).

LANGHAM: There are hundreds and hundreds of examples of burns of human skin.

DUNHAM: Lowry had a case.

They have been reported by dermatologists and cancer therapists.

EISENBUD: Could I ask one question about this lye. Isn't coral calcium silicate?

DONALDSON: No. There's very little silicon.

It's calcium carbonate.

WARREN: You might have flakes of calcium oxide or hydroxide which could burn a moist skin, but a very dilute lime water has been used as a soothing solution for burns.

CONARD: There are about 15 cases in the Rongelap people that still show some residual pigmentation and scarring as a result of the burns. The Japanese fishermen had some rather severe beta burns, particularly on the hand with which they were handling the fish lines between the thumb and the index finger. One area that was heavily involved was on the crown of the head. They frequently wear a handkerchief around their head and the crown of the head was exposed. The belt line was a frequent site of involvement. (See Reference 15.)

BRUES: Dr. Tsuzuki told us that the older fishermen had more damage to the skin of the head because, in general, they didn't wash their hair as often as the younger fishermen did.

CONARD: That's interesting.

From the Marshallese experience we learned about certain factors that influence the development of "beta burns." The Americans on Rongerik recognized the danger of the fallout and immediately went indoors in their Butler buildings. They took showers and changed clothes. As a result their skin exposure and internal exposure was minimal compared to the Rongelapese. The older Rongelap people who stayed indoors and others who went wading and swimming had fewer skin burns. A single layer of cotton clothing was proved to be sufficient to protect the skin.

The internal absorption of the radioactive materials produced no acute effects that we could observe. They had three millicuries of fission products that were calculated to be in their gut but this produced no effect that we could see.

Probably the strontium and radioiodine are the most serious of the radioisotopes that are present in this acute fallout situation.

DOBSON: Excuse me, Bob. How many millicuries did you estimate they had in the gut?

CONARD: Three.

DOBSON: Three?

CONARD: Yes, three.

TAYLOR: Was that probably by inhalation?

CONARD: Mostly ingestion. The particle size of the fallout was too large for optimum absorption into the alveoli of the lungs.

UPTON: Do you wish to imply that there were not depressing effects on the marrow from internal contamination, Bob?

CONARD: Yes, I feel that's true, since I think it was calculated that the dose over the whole period of time that the Marshallese received to their bones was in the order of several rads—something of that nature.

UPTON. Surprising.

FREMONT-SMITH: Why does it surprise you?

UPTON: I've been apparently laboring under a wrong impression for many years that the internal dose to the marrow was higher than you say it is, Bob.

TAYLOR: Is that from concentration of strontium-90 in plants?

UPTON: Just total fission product intake from one source or another.

EISENBUD: Are you talking specifically in these cases or in general?

UPTON: No, the Rongelap cases.

WARREN: They weren't there right along to eat local food or get exposed internally.

CONARD: The actual body burdens of strontium-90 that had accumulated over years for the Rongelap people amount to about 5 percent of the MPC for adults and ten percent for children.

EISENBUD: The Japanese fishermen lived at sea for 14 days in very intimate contact with fallout. It's quite a remarkable thing that Koboyama had, I believe, when he died, 2 millicuries of strontium-90 per gram of calcium in his bones, which is about 20 percent of what children have today. I mean it's a small dose. I think that one of the comforting things that came out of this experience is that the human body in close contact with surface contamination apparently has better defenses than we had anticipated against absorption of at least the less soluble components.

Now, the iodine did get in, as Bob indicated.

CONARD: We felt very encouraged about the whole internal situation. To be honest with you, we were misled. We felt that the internal situation was far less of a hazard than any of the others and, of course, we still do, but we certainly did underestimate the hazard of the absorption of radioiodines, as you'll see in a few minutes when I get into that aspect of it.

WARREN; Wouldn't the radioiodine be in gaseous form and inhaled rather than ingested, and wouldn't that be why the concentration could have been higher?

CONARD: They must have inhaled some from the cloud as it passed over but the majority of the radioiodine absorbed probably came through contaminated drinking water since it rained the night of the fallout. Moreover the people were on water rationing, everyone receiving about a pint a day including the children. So most of it was in the drinking water.

WARREN: Yes. It would be scrubbed out in the rain.

CONRAD: So, during the years the Marshallese people have remained generally in good health and we have not seen any illnesses or any deaths that we could directly relate to the radiation effects except for the thyroid situation which I will come to shortly and the one death in the case of the Japanese fishermen. They have been healthy over the years.

As far as mortality is concerned, 15 deaths have occurred among the 84 in the most heavily exposed group, which represent about 13 per thousand, and this is compared to about 8 per thousand in the Marshall Islands as a whole. So we do have some increase in mortality but whether this is significant in such small numbers it is difficult to say. We have a greater number of older people in the original Rongelap group also.

As far as malignancy is concerned, there have been two cases of cancer in the exposed group plus one case of cancer of the thyroid. So we have to keep an open mind as to whether we will eventually have an increased incidence of cancer. Again, the numbers are small.

As far as the skin is concerned, the only late effect that we have noted in the Marshallese is in the appearance of moles, benign nevi, in the areas that were more heavily irradiated.

Figure 12 shows some of the moles that have developed in the case of one woman who had fairly clear ulcerations on the side of her neck early after fallout during the acute period. Figure 13 shows residual scarring resulting from a severe "beta burn" of the ear. Figure 14 shows a case of one of the Japanese fishermen. I took this in Japan four years ago. It shows an area of permanent alopecia. The Marshallese hair all regrew except that in one case there was a slight alopecia, but in the Japanese fishermen there are two cases that still show some degree of alopecia, that is, a permanent bald area from the radiation. Some of the fishermen had "beltline" lesions with some degree of blood vessel dilatation (telangiectasia).

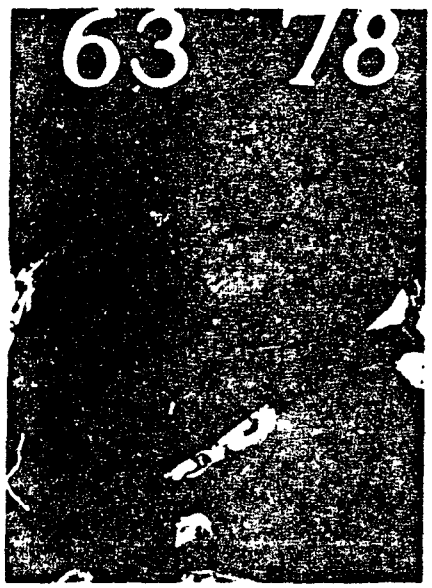

Figure 12. Benign nevi (moles) that developed 8 years later in area of "beta burns". From R. Conard (Courtesy Annals Int. Med.)

Figure 13. Scarring of ear from "beta burns". From R. Conard.

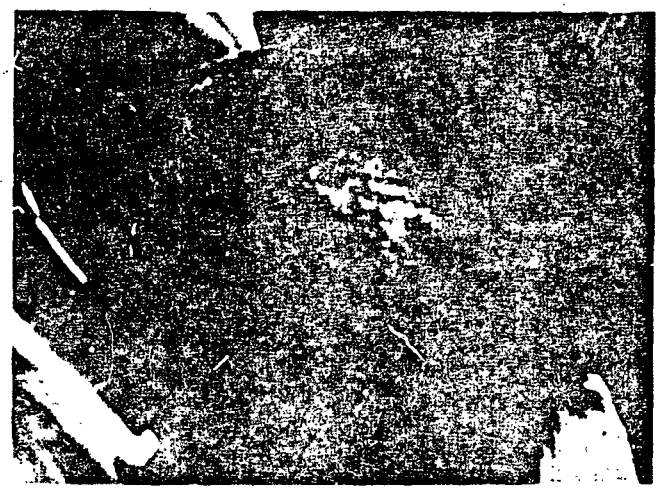

Figure 14. Permanent alopecia in Japanese fisherman. From R. Conard.

Now, a more serious finding in the Marshallese was that over the years the children, particularly the boys exposed at less than 12 years of age, have tended to be somewhat stunted in growth, have shown some lag in growth and development (Reference 16). We have carried out numerous growth measurements and x-rays for bone growth, and so forth, and this finding has become apparent.

Figure 15 shows a comparison of bone age in some of the males. The dotted line represents the exposed males compared with the unexposed males on the left, and on the right the females. The base line represents the American standard. The Marshallese tend to be somewhat smaller than American standards. Shortly, I'll have a little more to say about this lag in growth in the Rongelap children.

We have carried out blood work every year, of course, and Figure 16 shows that there's been a slight lag in complete recovery of the white count and platelet count up until about 11 years after exposure. The straight line represents the unexposed control population.

We have carried out numerous aging studies to see if we could detect any premature aging effects and we haven't seen anything along that line.

Life shortening has not been apparent in these people from this limited study.

Fertility based on birth rate has shown that about the same birth rate has existed in the exposed population as compared with the unexposed population. They've had about 70 babies and these babies on the whole appear normal. We haven't seen any greater incidence in the congenital defects in the babies of the Rongelap exposed as compared with the unexposed.

Whether there was an early sterility or not, we do not know. We did not test it, of course. It probably did occur during the early period. The Japanese fishermen showed quite a drop in sperm count which lasted for three years, but since that time they've had children repeatedly and recovered their sperm count.

During the first four years the exposed women showed some increase in miscarriages and stillbirths. About 41 percent of the births during that period ended in nonviable babies compared with only 16 percent in the unexposed group.

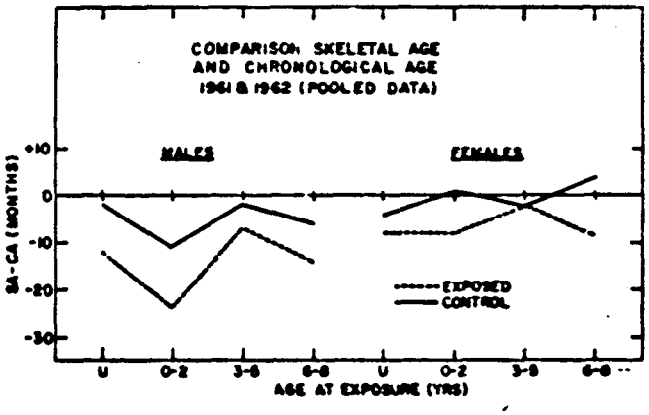

Figure 15. Comparison of skeletal age and chronological age (1961 and 1962 pooled data). From R. Conard.

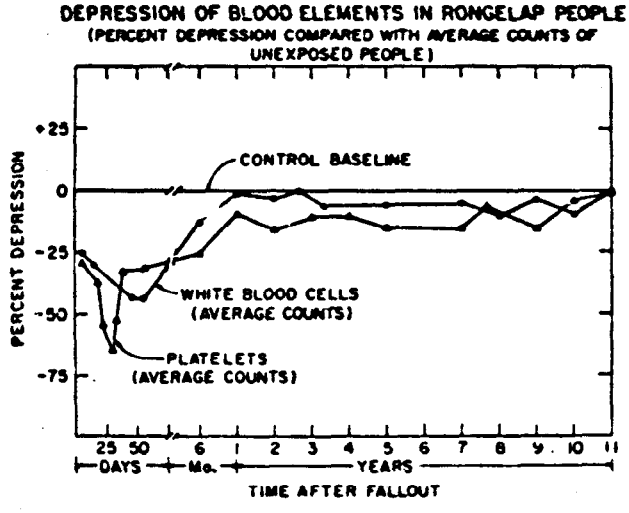

Figure 16. Depression of blood elements in Rongelap people. Percent depression compared with average counts of unexposed people. From R. Conard.

Examination of the lens for possible opacities has not revealed any evidence of radiation-induced opacities of the lens. Remember, of course, that the Marshallese didn't get neutron radiation, which has a much higher RBE for opacity than gamma radiation.

We haven't carried out any specific studies of genetic effects, particularly in view of the generally negative result of the studies of Neal and Schull (Reference 17) and others in Japan. I'm sure there must be an increase in the mutant pool of these people and we have seen evidence of chromosome damage in the peripheral blood cells. We have cultured their blood and found an increase over the normal in the number of chromosomal aberrations.

FREMONT-SMITH: Were these persistent?

CONARD: Yes. This was ten years after exposure.

FREMONT-SMITH: You don't know what they were earlier?

CONARD: We didn't test them earlier.

MILLER: More than the Hiroshima survivors?

CONARD: Yes. I was going to say that also in the Hiroshima survivors and in the Japanese fishermen there's been a persisting increased level of chromosomal aberrations. So I suppose we would have to expect that there are genetic mutations that exist in these people. Perhaps Bill might say something about that.

FREMONT-SMITH: At least in the blood cells.

CONARD: Yes.

FREMONT-SMITH: We don't know whether they are operating in the genes.

CONARD: I should imagine there would be some increase in general somatic mutations.

FREMONT-SMITH: All right. I thought you meant the genetic mutations.

CONARD: And also in the genetic.

FREMONT-SMITH: The genetic, too.

EISENBUD: What's known about consanguinity in this group?

CONARD: This is a good point. We've gone into that and it turns out that these people probably do have a somewhat greater degree of consanguinity than we do, but in the exposed group we checked the number of first-cousin marriages and second-cousin marriages and this sort of thing and found that actually they had a lower rate of consanguineous marriage than occurred in a comparison population. This also has bearing on growth and development because the children of consanguineous marriages are known to be somewhat retarded.

SCHULL: It might also have a bearing on the finding of increased percentages of abortions. If the latter reflects immunologic incompatibility between mother and fetus, fewer abortions would be expected among the pregnancies of consanguineously married individuals than among those of unrelated spouses.

EISENBUD: I don't see how they can get away from their cousins on a small island like that. I don't think they are completely inbred. Do they mix up much with the other islands?

CONARD: Yes, there's quite a bit of communication with other islands and people come in and bring in fresh blood! [Laughter]

FREMONT-SMITH: You mean small transfusions? [Laughter]

ROOT: I had heard that they had a low birth rate and that's why the custom of adopting other children into families had arisen.

CONARD: It may be true from the point of view of infant mortality which up until more recently has been quite high, but now we have brought in better medical care, and so forth, and the infant mortality is greatly reduced. But they do adopt children, too.

Now I would like to discuss the most serious finding in the Marshallese, that is the development of the thyroid abnormalities. Until 1963 we had thought that these people had normal thyroid glands. We had already detected this lag in growth and development in exposed boys and we really didn't have any explanation for it. We carried out numerous thyroid tests and so-called PBI (protein-bound iodine) tests of the blood, which are good indications of thyroid activity,

and we found them to be normal. However, since that time, as an aside, we have discovered that these people have a peculiar protein in the blood, an iodoprotein which is quite high and no doubt gave us false levels of the PBI readings earlier, and this may have thrown us off the track.

DUNHAM: It is true of all Marshall Islanders?

CONARD: Yes.

DUNHAM: I see.

CONARD: Four years ago we first noted a thyroid nodule in a 12-year-old girl and since that time there have been increasing numbers of these abnormalities until now we have 19 cases of thyroid abnormalities; 17 people with nodules and two boys with completely nonfunctioning glands, that is, a hypothyroid situation (References 9, 18, 19).

WARREN: Myxedema?

CONARD: They had signs of myxedema, yes. They were the two most dwarfed boys in the village.

FREMONT-SMITH: Were these thyroids that were destroyed or never developed?

CONARD: Presumably destroyed.

FREMONT-SMITH: How old were they?

CONARD: They were at the age of 15 to 18 months, which seems to be a critical age for children.

FREMONT-SMITH: When they were exposed?

CONARD: Yes.

MILLER: Was there any other child in that age range at the time of exposure?

CONARD: There were several in the one-to-three years of age range.

ROOT: They would be closer to the ground radiation and would get a bigger dose, wouldn't they—in addition to the fact that the organ itself is smaller? They would be at the level of the most intense radiation.

CONARD: That's true. That probably increased their whole body dose somewhat, but this would be negligible compared to the increase in the absorption of radioiodines into their glands, and that is the biggest factor by far. In other words, 700 to 1400 rad radiation came from radioiodine absorbed compared to only 175 of whole-body radiation, and if you want to assume that the children were getting a little more, you might increase it to 200 or so.

ROOT: They would be crawling around at the age of 15 months probably.

CONARD: Yes.

FREMONT-SMITH: Would their thyroids be in a stage of development where they would absorb a greater percentage from a given dose of iodine?

CONARD: I think it's assumed that their glands absorb as much as the adult, but being smaller, the same dose is distributed in a smaller gland.

FREMONT-SMITH: Right. So this means per gram of gland they were absorbing more.

CONARD: Right. They were getting a higher dose.

BRUES: Is there also a possibility that the thyroid in these children would be close enough to the skin that the beta dose would be greater, or at least would be appreciable while not as appreciable in the adult?

CONARD: We didn't feel that that was the case, Austin, because the beta radiation was so soft that it was attenuated in only less than a milliliter of the skin.

CASARETT: Bob, were these nodules appearing in relation to the onset of pubescence in most of these cases? Could the pubescence period and the endocrine disturbance associated with it be a stimulating factor in the production of nodules at the time they did appear,

which is apparently about ten years after the exposures for the first case?

CONARD: That's right. I think that's very likely to be the case, that most of these children were going into adolescence and there was a greater requirement perhaps on the thyroid due to increased metabolism and this could put a greater strain on the thyroid, and then they began showing the effects of a hypothyroid state.

Table 3 shows the distribution of cases; "R" represents Rongelap, "A" Alinginae, "U" Utirik, and "C" control. Here in the first four groups we have children less than ten years of age. You will notice that in the Rongelap exposed group there were 19 children that received a gamma dose of 175 and a thyroid dose of 714 to 1400 rads. We found on the last survey another thyroid nodule, so we have 84 percent instead of 78.9. The incidence in the Alinginae group—six children, none; Utirik—40 children, none; and the control children—61, none. In the Rongelap adults there were three nodules in the 36. The Alinginae adults had one nodule, which was not typical of the other radiation-induced cases. You can see that in the other populations there was only a small percentage of nodules and most of these were in older people which appears to be a normal incidence.

Table 3. Thyroid nodules (including hypothyroidism) in Marshallese populations.

Group	Age At Exposure	No. in Group	Gamma Dose (rads)	Estimated Thyroid Dose (I*, rads)	% Thyroid Nodules
R	< 10	19	175	700 - 1400	84.2
A	< 10	6	69	275 - 550	0.0
U	< 10	40	14	55 - 110	0.0
C	< 10	61	0	0	0.0
R	> 10	36	175	160	5.5
A	> 10	8	69	55	12.5
U	> 10	59	14	15	3.4
C	> 10	133	0	0	2.3

(R=Rongelap; A=Ailingnae; U=Utirik; C=Ur exposed)

Eleven cases were operated on, nine children and two adults. Figure 17 shows the nodules at surgery. Note the hemorrhagic nodules. It turned out at surgery that practically all of these glands had multiple nodules whereas at the clinical examination we had only been able to feel one or two; at surgery in most cases the glands were pretty well shot with nodules.

FREMONT-SMITH: Does this mean that a lot of other cases where you didn't feel anything also probably had multiple invisible nodules?

CONARD: Yes, it's quite possible that we were unable to palpate minute nodules in some cases, I can't deny that.

FREMONT-SMITH: Yes.

CONARD: Figure 18 shows one of the glands in one of the children that was sliced up just to show you the consistent nature, the multiple nature, of these nodular changes in the gland.

WARREN: Is that pigment or extravasated blood?

CONARD: A lot of that is hemorrhagic blood pigment. The histological examination of these nodules showed that they were all benign; they were of the type usually seen with iodine deficiency but, of course, we know that on Rongelap there's no iodine deficiency. The iodine level in the foods is normal and the urinary excretion of iodine, checked in quite a few of these people, has been within the normal range. Furthermore, we don't know of any goitrogenic foods on the Island. The evidence seems overwhelming that this is a radiation-induced phenomenon in these people.

There was one case in a 40-year-old woman in which the nodule was malignant. Now, one can argue that this may be just a normal occurrence. A lot of people believe that cancer of the thyroid is not easily produced by radiation exposure, but certainly in a small group like this heavily-exposed one it has to be considered as a possibility anyway.

MILLER: You said that there is overwhelming evidence that this is radiation-induced. You didn't mention yet that part of this evidence is observations made in other radiation-exposed groups.

CONARD: Yes, that is certainly true.

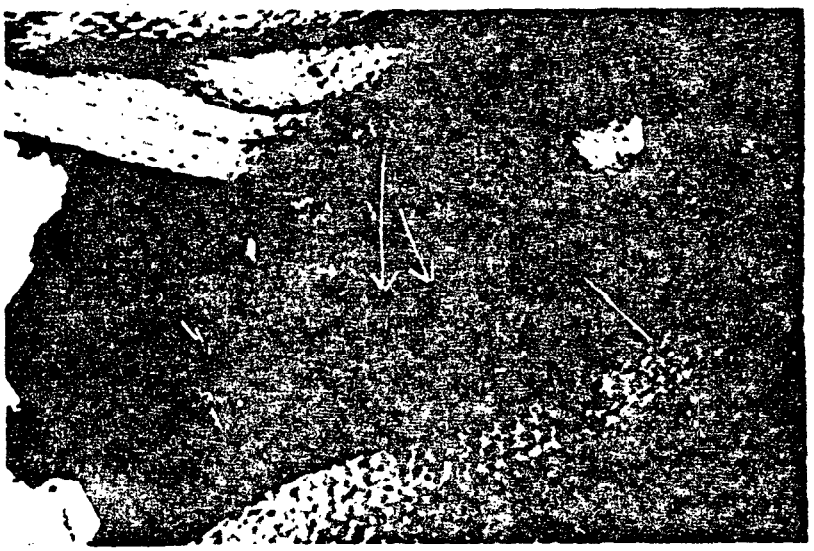

Figure 17. Benign thyroid nodules at surgery. Arrows point to nodules. From R. Conard.

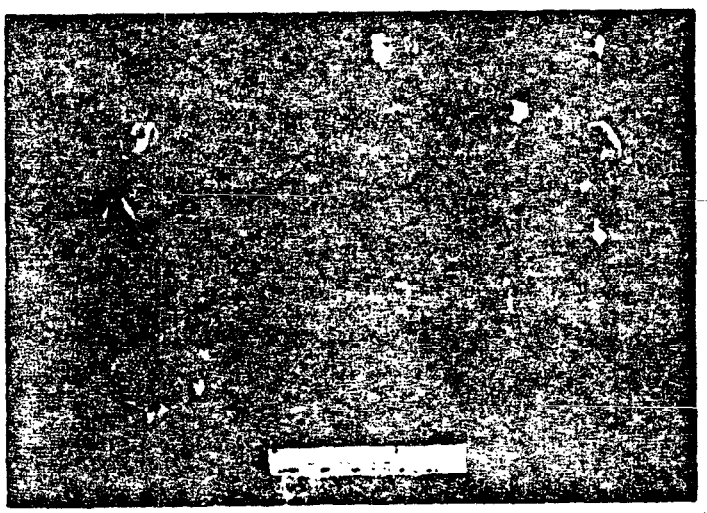

Figure 18. Sectioned thyroid gland showing multinodular, cystic and hemorrhagic nature of the gland. The nodules were benign. From R. Conard. (Courtesy New England J. Med.)

MILLER: Which others show it?

CONARD: Which other examples, you mean, from the literature?

MILLER: Right.

CONARD: Numerous animal studies have shown the causative relationship of radiation of the thyroid with later development of both nodules and malignancy (References 19 and 20). This also applies to radioiodine (References 21 and 22). Also examples in human therapy include patients, particularly children, treated with radioiodine for hyperthyroidism, which have been shown to later develop nodules (Reference 23).

MILLER: Then external radiation also has had some effect?

CONARD: External radiation certainly in children. A causal relation of irradiation of the neck region in infants and later development of thyroid cancer and nodules has been clearly demonstrated (References 24 and 25). There appears to be an increased incidence of thyroid carcinoma in inhabitants of Hiroshima and Nagasaki exposed to radiation from the atomic bomb explosions (Reference 26).

TAYLOR: Was this given for diagnostic purposes?

CONARD: Radioiodine was given for treatment of hyperthyroidism, to destroy part of the gland.

DUNHAM: Big doses.

CONARD: Yes. It takes about 10,000 rad to successfully treat hyperthyroid conditions whereas in some cases, to ablate the thyroid gland, such as in angina pectoris (heart disease) they use doses of 50,000 to 70,000 rad to the thyroid gland to destroy it.

We haven't seen any recurrence of cancer in this one Marshallese case. She's had complete surgical and radioiodine ablation of her gland.

Now, the correlation of the development of these thyroid abnormalities and the growth retardation in children has become increasingly clear. These children in recent years have shown more and more evidence of reduced activity of the gland, and, as I said, the two

dwarfed boys that were four years behind in growth and development show a definite correlation there.

So, beginning two years ago it was decided that we should treat all of these exposed people with thyroid hormone in the hope of reducing further development of nodules, to prevent cancer and hopefully, give an increased growth rate in those children that had shown the lag. Figure 19 shows the skeletal age development of the two boys that were most dwarfed. You can see that at the time of thyroid hormone therapy institution there was an almost immediate spurt in growth. We hope that in the next survey we will see increased growth rate in other children as a response to the treatment with the thyroid hormone. We are having difficulties getting these people to take their daily tablets. They just don't seem to want to do it. I was very disappointed when I returned from the last survey to find that the blood levels of the thyroid hormone in the affected children were quite low, which meant that a lot of them were not taking the drug. So we have a real problem getting them to take the drug for the rest of their lives, particularly the children.

DOBSON: Bob, in your earlier discussion of these patients, did I understand you to say that you are differentiating among different iodine-carrying proteins in the blood?

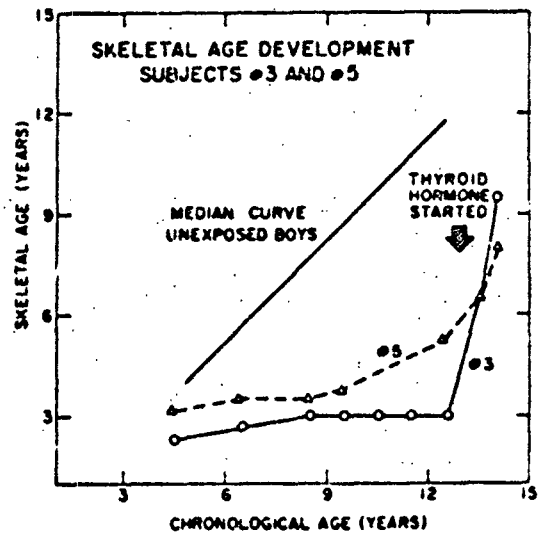

Figure 19. Skeletal age development before and after hormone therapy in two boys showing greatest growth retardation. From R. Conard (Courtesy Annals Int. Med.).

CONARD: Yes. We've done considerable work on the different protein-binding levels of the different blood proteins.

BUSTAD: On the basis of our work with radioiodine in animals and also a fairly extensive review of human data, I would not have predicted, nor can I find very many people that would predict, that you would see frank hypothyroidism with 1400 rads from radioiodine and 175 r from gamma exposure.

DUNHAM: Have you kept any animal ten years?

BUSTAD: Yes. In fact, we have fed sheep radioiodine for 11 years every day of their life.

MILLER: These were little sheep?

BUSTAD: Yes, they were exposed in utero since their mothers were fed radioiodine. In the cases of the Marshallese children, their dose was an acute one at a sensitive time, but since the calculated dose appears insufficient to cause hypothyroidism I'm wondering if there could have been two or three times the thyroid dose in some children. Maybe they drank more water or maybe a few children licked themselves and contaminated objects around them and realized significantly higher exposure. I have difficulty getting three times as much, which I would say might be the minimum exposure from radioiodine which would result in frank hyperthyroidism. I would like some reaction to this.

FREMONT-SMITH: Is this potentially a species difference?

BUSTAD: I think generally the acute ablating dose for most animals is very nearly the same. In an adult person it's reported to be about 30,000 rads, which is similar to that we have observed in sheep.

FREMONT-SMITH: Many other experiences with animals show that you do get species differences of various kinds and therefore prediction from several species of animals that you have used doesn't apply to humans.

UPTON: How about Sol Michaelson's work, George, in dogs?

CASARETT: That work (Reference 27) bears out the fact that external radiation with x rays will cause hypofunction of thyroid, myxe-

dema, with much lower radiation doses than those required from internally administered radioiodine. After 2,000 rads of x rays to thyroid the myxedema appears in about a year. With reduction of x ray dose the time taken for the myxedema to develop in dogs increases in a manner indicating a slower progression of the underlying mechanism at lower doses. There is a possibility that radiation from external sources, in addition to the internal radioiodine, may have contributed to the thyroid changes in the children in question.

BUSTAD: That was my next point. I think that 200 r or 300 r is not an insignificant amount from the standpoint of thyroid damage. These children probably received a considerably more effective dose per rad from external gamma to the thyroid than from I^{131} and there is some substantiation for this from animal data. And if I can then stretch a point and say, "Well it's five times more effective".....

UPTON: .....because of dose rate or dose distribution within the gland?

BUSTAD: Yes, I think there are at least two things that contribute to this. In order to get the same equivalent rad to the thyroid from I^{131} you have a much lower dose rate because it's extended over many days. With the total body radiation it was a sudden thing over a matter probably six or eight hours. In any case it was very acute. The other thing is that a lot more than thyroid tissue was affected following external gamma exposure. With the radioiodine, however, the periphery of the thyroid gland is probably receiving 25 percent of the dose at the center of the gland while in the case of external irradiation the entire thyroid gland is being uniformly irradiated as are the contiguous structures. I think this too is important.

The other thing that is worthy of note is that in Dr. Hempleman's studies (Reference 28), which I briefly discussed during our first meeting, he noted a high incidence group of about 268 children who were irradiated early in life anteriorly and posteriorly for total doses of 200 to 600 R or more. Of the 268, there were 20 that manifested thyroid neoplasms. Half of these were cancers. It's interesting to me to note that in your Marshallese group, Bob (Dr. Conard), the children manifested no cancer, only thyroid adenomas (Reference 29). This was also the pattern in our sheep studies. We had one fibrosarcoma and one adenosarcoma and 30 or more adenomas, and this pattern of response has characterized most of the followup studies of the children who were exposed early in life to radioiodine. That

is, there seems to be a higher proportion of cancer per total neoplasms than with the radioiodine studies, but I'll admit there isn't always comparability and many of you could then say, "But we don't know how many of these who are still walking around may have adenomas." We know that in adult populations there is a high incidence of thyroid adenomas; in fact, in those of you who are over 50 years old, if we removed your thyroid (if it isn't already removed) we would probably find adenomas in half of you. A study was done several years ago in which it was shown that half of the people over 50 had thyroid adenomas and most of them didn't know it and seemed none the worse for them. The moral of the story is if you're going to have a neoplasm, choose the thyroid.

FREMONT-SMITH: Were any primates used in the experimental animals, any monkeys which might be closer to man?

BUSTAD: The only studies in the sub-human primates that I'm familiar with are those by Pickering (Reference 30), and he was concerned mainly with the uptake in the very young—the fetal thyroid. There have been no long-term studies with radioiodine in primates to my knowledge. I think that most of the data that I've reviewed—and I think I have reviewed most of it in this field—certainly indicated to me the species that have been worked on.....

FREMONT-SMITH: But they're all lower species?

BUSTAD: Well, no. We've also looked at human cases where there was radioiodine given.

FREMONT-SMITH: Okay.

BUSTAD: The effect is similar. It will take an acute dose of 30,000 to 50,000 rads to ablate the thyroid of sheep. Dr. Goolden (Reference 31) in England, looking at a lot of human cases says it will take a comparable dose for a human adult. There's one exception to this that some of you may bring up, and that is the work of Dr. John Garner now at Colorado State Univerisity (Reference 32) who says that cattle thyroids are unusually resistant and may take over 100,000 rads. In all of these cases a lower dose will cause hypothyroidism if you wait long enough.

WARREN: The jack rabbit is susceptible, too.

BUSTAD: Yes.

CONARD: Are you intimating that in the case of the Marshallese their gamma dose was probably significantly higher than we have calculated?

BUSTAD: I'm only trying to generate ways these children may have had more radiation than was estimated. In addition to you, I've talked to many other people who have had association with it and they will admit that maybe it could have been something over 200 (quite a bit over 200 possibly), and then we have to admit that 1400 rad as a maximum may not be a true maximum depending on what the experience of these children was during this period. I'm also worried a bit about the short-lived isotopes which can really contribute very heavily to a radiation dose, and I'm speaking of iodine 135 and 133. The fact is they may contribute up to half or more, especially in the early period.

MILLER: You keep speaking of 30,000 r to ablate the thyroid in an adult; how much is required in an animal one-month old?

BUSTAD: I feel that—and this is partially intuition—it's possible to see hypothyroidism. If you permit me to choose any animal and choose a certain dosage regimen, I could produce it with maybe 5000 or 6000 rad in an animal that's very young providing you wait the ten years or so that Dr. Dunham mentioned earlier.

MILLER: There is a need to make a study in animals that duplicates the experience of these children.

BUSTAD: Well, I've discussed this with Dr. Dunham some time ago and some of this is under way.

DUNHAM: I think the point is well taken that it is a combination of internal and external.

BUSTAD: That's right.

DUNHAM: And in the other data, it's either one or the other.

BUSTAD: That's right. And I feel strongly about this.

BRUES: Ablation of the thyroid is a different matter as regards the production of adenomas. You have to leave some tissue but remove enough so that the pituitary sees a thyroid deficiency and stimulates

the thyroid cells that remain with thyroid-stimulating hormone. So I would suppose that the adenomas would go through a maximum at some point. In addition to that, if with radioiodine, as you have said, Leo, the irradiation of the thyroid is not homogeneous so that the outer layer gets less of a dose than the internal part, there might remain a reservoir of cells on the periphery which would be stimulated by the pituitary response to hypothyroidism.

CONARD: But we had two cases, remember, with ablation and with practically no thyroid function. These glands are gone.

DUNHAM: What's your evidence that there is ablation? You said hypothyroid. How hypo were they?

CONARD: Their PBI's dropped to below 2 micrograms percent, their glands were no longer palpable, and their iodine uptake was nil. I do not see how you could account for this ablation on the basis of the increased whole-body radiation since, if the whole-body exposure had been increased by even a factor of two, we would have seen considerably lower white counts than we did.

BUSTAD: If you look back on these two boys can you really separate out the blood picture from, say, 150 r versus 250 r exposure?

CONARD: I think so. I think if they had had 250 rad we would have seen signs of infection or bleeding in these kids.

AYRES: You said a while ago that the thyroids of these young children would absorb about the same amount of iodine as an adult but the glands were smaller. Is that taken into account in the internal dosage calculation?

CONARD: Yes. This is what brings the child's dose up so much higher than the adult dose.

AYRES: I just didn't notice.

BUSTAD: A factor of ten.

BRUES: In fact, the ratio is better estimated than the absolute dose.

CONARD: Perhaps! [Laughter]

CONARD: Figure 20 shows one of the boys with greatest growth retardation (on the right) standing beside his brother who is a year younger. Shortly after this picture was taken we started the boy on thyroid hormone treatment. Figure 21 shows the same stunted boy on the left before treatment and on the right a year later.

ROOT: His features — the myxedema is gone.

CONARD: Yes. He's changed in appearance. I hardly knew him when I saw him after treatment with the hormone.

We'll go on then to the chronic exposure from residual fallout. I refer here to the period following the first few days of acute exposure. In this situation we have low dose rate whole-body irradiation, possibly some irradiation of the skin, and internal absorption of some radioactive isotopes.

We know that chronic low dose exposure such as this will increase to some extent the incidence of leukemia and cancer of the skin and has been seen by radiologists over the years. But we are in a region that we really know very little about in regard to human effects. We get down into the region in which there is controversy over whether or not there is a linear dose effect relationship and whether or not there is a dose threshold for the effect.

In the case of the Marshallese, at the time of their return to Rongelap Island there was a low level contamination consisting mainly of the radioisotopes cesium-137, strontium-90 and zinc-65. Though the body burdens were well below the MPC levels, it has afforded us a unique opportunity to study the radioecological situation in the Marshallese. Perhaps in your discussion tomorrow, Lauren, you might bring in a little bit more on this aspect of the thing.

DONALDSON: Yes.

CONARD: I find it extremely difficult to visualize what the situation will be during the aftermath of the atomic bomb. I have tried to visualize the importance of residual fallout in this situation and I just can't give it too much emphasis. To me, if one survives the acute fallout situation the economic, transportation, and psychosocial problems will far outweigh the residual fallout problem in importance.

Figure 20. At right, a 12-year old boy with greatest growth retardation; at left is his brother a year younger. From R. Conard.

Figure 21. Same boy with retarded growth as shown in Figure 20; at left, before thyroid treatment, and at right 6 months after treatment began. From R. Conard.

UPTON: Could I ask, Bob, about the dose rate at the time they were evacuated? Suppose it had been impossible to get them out promptly? Suppose one had waited a few days or a few weeks, would the situation have been vastly different in the outcome?

CONARD: There wouldn't have been as much difference as you might think. The total dose would have been, say, around several hundred rads, around 250 I believe it was, if they had stayed on there.

DUNHAM: And never left at all?

CONARD: Yes.

ROOT: Is it because of the short half-life of most of the elements that there would have been no appreciable increase with time?

CONARD: It's due to the fact that the shorter-life elements are dying out and only the longer-life ones are left, so that the radiation dose rate reduces with time and the dose rate would have been considerably less as time went on.

ROOT: Like, for instance, if you have strontium-90, does the body take up as much as it can in the initial stages so the residual strontium-90 doesn't have much effect?

CONARD: You do reach a point of equilibrium with the environment, that is provided the dietary source of strontium-90 remains constant.

UPTON: But the total dose wouldn't have been twice what it was had they remained indefinitely on the island?

CONARD: No, not the whole-body dose.

TAYLOR: Is that independent of strontium-90 concentration in the food that they eat? I thought that that didn't really come up.

CONARD: In the Marshallese the majority of the present body burden of strontium-90 is from their native dietary source after moving back to the island.

AYRES: In the first few days the concentration of strontium-90 would have been very, very tiny, whereas ten years later it would have been a significant fraction of what was left.

CONARD: Relatively greater, yes.

EISENBUD: As a general rule, as many of you know, the dose rate goes down by a factor of 10 for every sevenfold increase in time. The dose rate must have been down to about 10 percent of what it was when it started. Had they stayed on then, as you said, it would have been a smaller figure, something like 25.

AYRES: It's not true in the early hours, when you're not at ground zero, because of the delayed arrival.

UPTON: But this is simply the external radiation. This doesn't take account of continual recontamination by fission products in the environment. The internal burden would presumably continue to increase.

DUNHAM: Relatively speaking the strontium-90 is unimportant to begin with, as Dr. Ayres points out. The amount of strontium-90 that they are now living with isn't very different from what it was when they left. It was the material on the surface of the food that they might have eaten on the first two days that was important.

CONARD: I think we should seriously consider the possible psychological reactions to the residual fallout situation. It would be a great mistake if this hazard were overplayed. It could cause psychological unrest and interfere seriously with realistically facing the recovery problems. I think this point deserves serious consideration.

FREMONT-SMITH: Also, there would be a credibility lack if we made less of it than we should and it was then discovered that we had made less of it.

CONARD: Yes, that's true.

ROOT: Could I have a word about the crab that was a staple in their diet. I've heard two things: one, that the crabs ingest their own shells so they are forbidden as food; and the other, that they have disappeared entirely.

CONARD: No, they are still there. They're reduced in number. The coconut crab is quite a delicacy among the people.

DUNHAM: It's not a staple, it's a delicacy.

CONARD: They are very fond of it.

DUNHAM: They say there's a distinction between this and a staple, which is something they must have to live on—a main constituent in the diet. Crab is a delicacy when they can get one.

CONARD: These crabs have a concentration of 4000 to 5000 units of strontium-90.

FREMONT-SMITH: In their shells? In their meat and their shells?

DONALDSON: It's in their digestive gland. It's characteristic of crustaceans to build up reserves of minerals to use at the time they molt and this then is translocated into the shell from the storage house, in this case in the.....

FREMONT-SMITH: It stores minerals in its skeleton and then releases them when it's going to make a shell. When the crab makes its new shell it takes it not from the skeleton but from the digested matter.

DONALDSON: This translocation takes place in relatively short order. One distinct difference between the coconut crab and the usual crustacean is that as soon as the crab finishes the molting process and the new shell is formed, the crab eats the old shell and thus these minerals are returned to its body.

FREMONT-SMITH: They eat what?

DONALDSON: They eat the shell.

FREMONT-SMITH: The old shell?

DONALDSON: Yes.

FREMONT-SMITH: So they don't lose anything.

DONALDSON: So it preserves the materials and they go on perpetuating this process year after year. This is a particular situation peculiar to the coconut crab. It's not typical of crustaceans in general.

FREMONT-SMITH: I'm sorry. This eating the shell is what the coconut crab does?

DONALDSON: Yes.

AYRES: Perhaps I may make a further remark about the relative importance of strontium-90 in this case as opposed to, say, a nuclear war. Probably it's not important in the long run on Rongelap compared with the initial dose that people had, but it might be important in the aftermath of a large number of nuclear weapons if you're talking about the region away from direct fallout.

CONARD: You mean where it was involved immediately?

AYRES: I'm not saying that the strontium-90 would be important when compared to the damage to the area of direct fallout, but where local fallout didn't fall, strontium-90 would be one of the most important things with which to contend.

DUNHAM: Are you talking about worldwide fallout?

AYRES: Yes.

CONARD: In the situation that we're talking about, if you had a nuclear war, aren't you going to have practically everybody involved and isn't the amount of strontium going to be trivial to the problems of transportation and all of the other problems that are going to exist?

AYRES: I think probably so.

DE BOER: We don't have to talk about an all-out nuclear war.

AYRES: The point is that people tend to worry about the most important residual effect that affects them, and in some parts of the world strontium-90 might be the most important residual effect. In other parts, not.

DUNHAM: In other parts it might be something else.

AYRES: Possibly. In the areas more directly damaged it would be a relatively minor thing except very late again.

FREMONT-SMITH: If people recovered from this damage, then it would come in again.

AYRES: Yes, many years later.

FREMONT-SMITH: So the assumption is if you neglect it,you don't recover from the damage.

EISENBUD: I think we should bear in mind that through a process of testing we have disseminated around the world a very sizable fraction of the total amount that would be produced in an all-out nuclear war. Hasn't there been about 500 megatons of testing? Let's say in nuclear war you talk about 10,000. Now you've got a good tracer experiment. You see, you're up to maybe somewhere between 1 and 10 percent of what would be released. If you increase the present level a hundredfold without creating a risk it would be significant compared to the social consequences of the bombings themselves in the immediacy.

AYRES: That's just a few hundred megatons over a decade although most of it was concentrated over 3 or 4 years.

EISENBUD: What's the difference? It's all long-lived stuff.

DUNHAM: We're talking about the late effect.

EISENBUD: It doesn't matter. It's undistributed.

AYRES: Yes, but the uptake phenomenon very much depends on the timing here.

EISENBUD: For strontium-90?

AYRES: Uptake efficiency is much smaller for strontium-90 in the soil, compared to uptake of strontium-90 from foliage. If you have a lot in the atmosphere at one time you may get quite a considerable dose and, of course, it's stored in the bone.

EISENBUD: As I say, it can increase about 100. You take the social consequences of the bombing themselves and the immediate consequences and compare that with the worldwide consequences of, let's say for the sake of argument, everybody having 500 picocuries of calcium. I would say that the late effects would be a minor thing.

TAYLOR: There's still one other case and that is when you consider strontium-90 in the region where there was heavy fallout but the people were protected, let's say, by fallout shelters. The question is, what is the remaining hazard then? Let's say people are out of their shelters after a month. I don't know. I'm really asking. Is it clear that

in such a case strontium-90 is the main source of radiation to these people?

CONARD: I think it is. It's probably the main hazard from a radiation point of view. I still contend that this small selected group of people is going to be faced with many more problems that far outweigh possible contamination from strontium-90.

AYRES: I accept that, but I wanted to bring out these points.

CONARD: The last item I have here is protection, survival, and recovery measures. I don't think I need to emphasize to this group the fact that taking shelter in either homes or basements or fallout shelters is quite protective. I think that one might want to consider such things as the use of the stable isotopes, perhaps strontium and particularly iodine, during this acute period. It only takes about three to four milligrams of iodine a day in the adult to suppress the absorption of the iodine uptake of the gland, two to three milligrams in the case of children. I don't think it would be unreasonable to have a little Lugol's solution, potassium iodine, available to add to the diet and perhaps stable strontium or calcium.

WARREN: How much are we getting now in the salt? At one time we had a lot of hypothyroidism and myxedema around the country and there was a drive to put increased iodine portions in the salt. I think the Morton Salt Company has done that, but I'm not certain.

FREMONT-SMITH: They have. In fact, you buy it in the grocery store. We do regularly.

WARREN: Yes, but is the iodine still added?

FREMONT-SMITH: So it says. You can't taste it, but it says so.

CONARD: It's a small amount, a very small amount.

BUSTAD: I would exercise caution, I think, in recommending stable strontium because I think the amounts that you would require to really affect the uptake would be toxic.

AYRES: Any stable calcium would be just about as good, wouldn't it?

CONARD: That's right.

WARREN: .....because the strontium can only join if there is a gap in the calcium. Do these people have milk? Is there calcium in the coconut milk?

CONARD: I'm not sure what the calcium content of coconut milk is but they certainly had no cow's milk and there were very few children that were nursing at that time.

WARREN: Fish bones have some, don't they, Lauren? Fish bones have some calcium.

DONALDSON: Yes.

WARREN: Don't they eat small fish total?

DONALDSON: They eat the entire fish.

WARREN: So this is one of the sources of their calcium. You don't know whether they've got a calcium deficiency, do you, so that they sop up calcium?

CONARD: We don't know that specifically.

WARREN: This could vary from day to day.

CONARD: We've done the strontium-calcium ratios in their urines, and, as I remember, the calcium was within normal limits.

DUNHAM: With all that coral dust blowing around the atolls there, they can't be deficient in calcium.

DONALDSON: There are several cyclic phenomena here. One has to realize that the atolls are made up almost exclusively of calcium compounds. There is a tremendous availability of calcium although most of it is not in soluble form. There are noticeable deficiencies of some elements in the area, particularly iron. Thus, some of the plants don't grow well because of the lack of this element. On the other hand, the natives' diets are geared to this type of environment through survival patterns, or whatever one wishes to call them.

One of the greatest sources of minerals in these diets, beside the coconut crab which is a delicacy, is the giant clam. This clam also is a great filtering mechanism for the sea. It tends to concentrate

its mineral requirements from the sea in not only the shell but particularly in the digestive gland. People eating the entire organism are thus actually being supplied with the minerals they need. The same is true if they are eating the fish; they eat the entire fish. Maybe you can say it's the Japanese influence, but as part of their diet they will eat many of the algal groups and here again they have a good source of minerals.

I think one might comment that one of the things we have feared was that a nice handout would change their food habits appreciably— they are eating rice now and canned goods to a very great extent. This new diet may have a much more specific effect upon them than some of the things we've been talking about in the context of radiation contamination.

FREMONT-SMITH: Do you think their diet might become deficient now because of the canned goods?

CONARD: I don't think so. I think they're getting more protein now. They eat canned salmon.

FREMONT-SMITH: But what about minerals?

CONARD: We haven't seen any real evidence of nutritional deficiency.

BRUES: If I may quote from your most recent monograph (Reference 9), the 1965 urine analyses showed around 100 milligrams of calcium per liter. I suspect this is a little low rather than high.

CONARD: I've forgotten exactly what it is.

MILLER: The question was raised this morning as to whether or not radiation was singled out as a special horror when, in fact, it was not special. Yet you have shown us that among the Marshallese there was no serious consequences from fallout at first, but after 10 years a high proportion of children were found to have thyroid nodules and two of the children were very markedly dwarfed. Why shouldn't there be fear, then, about radiation in particular? These people had no control over it. Exposure need not come from nuclear war; in this instance it was a nuclear accident. As Dr. de Boer said, nuclear wars or nuclear weapons now may be more limited in their effect, more limited in their areas of influence where perhaps fallout will be a serious consequence. But, even if it is not, the fear among the people may be deep and widespread.

TAYLOR: I would like to ask two questions that are related to a possible lesson from the Bikini experiences that might apply to a nuclear war situation. One can certainly visualize circumstances in which there is heavy fallout in an area and there are shelters of some kind available, but in the process of getting into the shelters people are subjected to some amount of fallout. The question is how important is it likely to be that they decontaminate themselves to get rid of any surface activitiy that is clinging to them that is gamma radioactive? Is there any estimate of what fraction of the total body dose the natives got that was due to gamma emitters that was in the white ash that stuck to their bodies and would follow them into the shelter if they had gone to one?

CONARD: It was a very small proportion. Usually they say the beta-gamma ratio is about 100 to 1, so they were getting about 100 times more beta radiation on the skin than they were from the gamma.

TAYLOR: So the necessary decontamination would be to get rid of the source of beta burns?

CONARD: Yes.

AYRES: Is this 100 to 1 ratio based on specific studies?

CONARD: I think this is just a general statement from my understanding of it.

AYRES: Well, I've heard numbers like that but the only pertinent research I am aware of was done by Steve Brown (Reference 33) at SRI about two years ago. It suggests rather smaller ratios more like 25 to 50 to 1.

CONARD: I've heard that; it's controversial, I'm sure.

AYRES: They have actually taken the fission spectrum and done detailed calculations for the first time to my knowledge.

CONARD: But, even so, that's quite a ratio.

AYRES: Yes. It's a useful number.

WARREN: I wouldn't like to leave the impression that I think it's unnecessary. I think the precaution.....

TAYLOR: Assuming it is very hard to get this stuff off, the question is how important, really, in a major disaster situation would it be to get the stuff off? I get the impression it wouldn't be terribly important—that people would get beta burns but that these really are not terribly serious anyway.

CONARD: They can be serious, but it's fairly easy to decontaminate the skin. Even with a damp cloth you can probably wipe enough fallout material off so that you won't get a burn.

BUSTAD: I wouldn't sell beta burns short. They are very irritating, at least that's what my pigs told me! Furthermore, there is a long latency for the development of skin cancers. I would also point out, although it may not be very significant, that iodine may be readily absorbed through damp skin. (The radioiodine in case of fallout originates from tellurium in the fallout.) I would recall for you that we can obtain our requirements for iodine if we just rub tincture of iodine on our skin. We'll get enough that way to satisfy our demands.

FREMONT-SMITH: You mean all over or just a little bit here and there?

BUSTAD: No, you don't have to rub it all over.

WARREN: Well, in the mass casualty situation you wouldn't want to have to supply all of the materials, ointments, and bandages to protect the skin while it was breaking down, and if you could eliminate this from the consideration, it would be worth doing.

TAYLOR: It sounds like a difficult job.

CONARD: To get it completely decontaminated. It was very difficult in the Marshallese but I'm sure they would never have developed any further skin burns if we had gotten it off completely.

FREMONT-SMITH: Baths may not be available in a disaster area for everybody. There may not be that much uncontaminated water to use.

WARREN: Some did go into the ocean and were less contaminated.

FREMONT-SMITH: But we're not all staying close to the ocean.

WARREN: I mean in their case. Of course, all the shelters will have showers.

FREMONT-SMITH: Exactly. I said there wouldn't be enough shower water.

DUNHAM: You know, the problem is a little like the flash burns in Japan. What clothing is worn makes a little difference.

WARREN: Yes.

EISENBUD: I think it's awfully hard to be adequately imaginative about these things and most of us, I think, have kind of insulated ourselves. I used to think about it more than I have in recent years and it used to impress me. Frankly, I haven't thought about it recently, but I think basically you've got to face the fact that you have a pretty high doctor-to-patient ratio. You didn't have the complications of blast; you had adequate food supplies; you had adequate water supplies; and you didn't have panic. When I think of the kinds of inte acting of medical and logistic problems that would arise in the event of a real nuclear war, it seems to me that almost any type of injury would greatly lessen the chance of survival.

FREMONT-SMITH: Absolutely.

EISENBUD: It could be even a minor injury to a finger. If a man has got to dig himself out of the rubble and has a broken finger, he may not be able to get out and we haven't faced up to the fact that these things do interact in a way which is not only unpredictable but incalculable. I don't know how to apply numbers to these things.

CONARD: That's true. You may have a severe leukopenia that develops and this, in conjunction with a laceration or even with beta burns of the skin, may result in serious infections from a tiny wound. You may have a very serious situation.

FREMONT-SMITH: A small infection then could be fatal.

EISENBUD: That raises the question of what is the LD-50 in a populace exposed to mass bombing, and I don't know whether you want to get into that or not.

FREMONT-SMITH: How many assumptions do you have to make alternatively to try out that figure. There are at least 10 or 15

separate sets of assumptions you can start off with and each one leads in a different direction. I bring this out because in the very simple hypothetical situation we had in which only two counties in northern New York State were exposed to the bomb we couldn't settle down to really reach conclusions as to what we should do because there were different kinds of assumptions you could start off with which lead in different directions. I think if we had a nuclear war it would not take very long to list 50 different things which would make what you were planning to do quite different.

EISENBUD: Yes.

BRUES: You have innumerable little judgments in the case of water. If someone has a half-pint of water, how much does he drink and how much does he wash off with?

FREMONT-SMITH: Exactly.

BRUES: And does he drink contaminated water or does he wash off with it, or both?

EISENBUD: May I take a poke at the government again in connection with this. This was the first shot of that Castle series and it delayed the second shot. It proved what a lot of people had suspected; you can have massive fallout following a surface detonation of a megaton bomb. Based on early, very sketchy data collected by two or three individuals, certain isodose curves were drawn which are, at best, approximations. Those of us who have had the experience of actually measuring these fallout patterns from smaller weapons find that they are not quite so uniform, that they tend to be amoeba-like and are harder to find.

There arose out of this experience the need for an experiment which would make it possible to get better approximations of the total amount of debris that falls out; physical and chemical characteristics. This wasn't done, and as far as I know hasn't been done in any other subsequent explosions during the period when they were still testing in the Pacific. I think that, from the point of view of national security, we are without information which is badly needed.

Now, it's needed to simply answer questions. It may be totally useless in the sense that there may not be, even with the present information, a satisfactory answer to all the complications of mass fallout and the way it would interact with blast.

DUNHAM: There were a couple of heroic efforts. One was to actually sample with rockets to find out what was coming down into the air shortly after the explosions, but the rockets all failed or something went wrong. There was also quite a lot of effort to collect stuff on barges and things. The NRDL was involved in this.

EISENBUD: When you say "heroic", what people were trying to do was slip things in. Then you remember the way we laid 400 rafts and couldn't find them afterwards. But this was all stuff that was done in a hurry trying to fit our requirements into a schedule that was already laid down and couldn't be changed.

DUNHAM: One of the big problems was simply the old business of trying to guess where the wind is going to be if you're talking about surface collecting, and they tried to get around that by a whole program of rockets. Dr. Alvin Graves of the Los Alamos Scientific Laboratory and Dr. Willard Libby, then one of the AEC Commissioners, were promoting this and it just fizzled. I don't know what happened to the rockets, but they never did get much data.

TAYLOR: I think the reason that the experiment just is not done is there's no place to do it. If what one wants is to fire a few megatons on the surface of the dry land somewhere where there isn't a lot of water involved, the question is where do you do it?

BUSTAD: You can do it in China! [Laughter]

EISENBUD: Granted. And, of course, this is an extremely important point.

AYRES: What is it that we don't know?

EISENBUD: Would you want to set national policy based on a single set of observations which yielded data which at best were just scavenged?

AYRES: Which types of data are you referring to specifically? There's much more than one set of data on this.

TAYLOR: Not a megaton.

AYRES: There's a lot of kiloton data that's very different and some megaton data (Reference 34).

DUNHAM: It's still not known whether one-third or two-thirds comes down within several hundred miles of a megaton burst.

EISENBUD: You can measure the doses and not have to reconstruct them.

AYRES: The particle size distribution, I believe, is now much better understood than it was two years ago.

DE BOER: This is an area you can't discuss very much because you get into classified information. I think you're really treading on thin ice now as far as that's concerned.

MILLER: May I ask how many casualties there were in the Marshallese?

CONARD: What do you mean by casualties?

MILLER: That's what I want you to tell me.

FREMONT-SMITH: You want to know how many there were or what do you mean by casualties?

WARREN: It's the qualitative rather than the quantitative definition. You mean some of their white counts fall and there's no other evidence and they are nauseated and some of them had beta burns and some didn't.

CONARD: Almost all of these people were affected in some way.

FREMONT-SMITH: 100 percent casualties.

MILLER: Yes, among the Marshallese. But then there were casualties in Japan. Dr. Donaldson has told us that the mother of one of his students was a casualty, and Dr. Schull has told us about a mother and daughter from Osaka whose fears caused them to seek medical advice, and who were, I suppose, psychological casualties. And there may have been many more but these were just a few we happened to hear about. I am wondering if perhaps there were not mass casualties as a result of the Bikini experience. One must think of the people around the world, especially those in Japan, who were casualties medically, economically and/or psychologically.

CONARD: Well, in this small population we were not able to observe any casualties other than those produced by the radiation effects. There was nothing that I would classify as psychological casualty. As far as their relationship with the other Marshallese people and this sort of thing is concerned, we were not able to observe anything unusual.

MILLER: N[illegible] oint was that the casualties may not be limited to the Marsha.

UPTON: The[illegible] has been a thyroidectomy, hasn't there?

CONARD: Eleven people have been operated on; 11 surgical cases.

FREMONT-SMITH: Do they count as casualties?

CONARD: I just don't know what definition to give.

UPTON: How did they react to their experience? Would you say this has been a source of distress? Has it been disconcerting?

CONARD: Several have come up to me in the last survey and said, "Can't you find a nodule so that I go to the United States and get operated on?" [Laughter]

FREMONT-SMITH: The mass casualties are all those who haven't been able to go to the United States!

MILLER: As I said before, my point is that the casualties may not have been limited to the area of fallout. They may have occurred in Japan, affected indirectly by the fallout, by economic troubles, by suicide, by other psychological disturbances, and by the uproar in general. That was my point—that there really may have been many more casualties than one can count in the area of the Marshall Islands.

FREMONT-SMITH: And the whole of Europe was disturbed and had a different reaction toward the U.S., and that's a major casualty.

ROOT: Yes. That's a grievous psychosocial effect.

WARREN: There's another generally insidious casualty which affects all levels of government. To mayors, supervisors, governors, and on up. Civil Defense has fallen flat on its face on the basis that it's impossible to meet the situation which we don't think is going to happen

anyway. It would cost a lot of money and trouble so we're not going to do anything about it until we have to.

FREMONT-SMITH: Yes.

WARREN: That puts us in a very vulnerable situation. There was a general participation and training up to about 1955 that could have provided a fairly competent protection in the possibility of warfare affecting the United States. But now there exists nothing that is much more than a paper organization, very poorly supported, and not well-understood or known.

FREMONT-SMITH: This was the psychological casualty, wasn't it?

WARREN: This was the psychological casualty.

FREMONT-SMITH: Of the whole country.

WARREN: There's a group of assistant professors who know nothing about World War II and still less about atomic warfare. They have a kind of vague apprehension. They would like to know more and they are beginning to work on it. Of course, there's a very small group that is willing to work on it. The rest of them think it's a horrible thing: "We must stop all war." This is a nice goal but we haven't gotten very far on that goal yet.

FREMONT-SMITH: Not quite.

WARREN: This is really where we run into trouble in the long run. How do we bring this situation to a focus and how do we deal with it? In our culture and history it seems to me to lead eventually to a war, because our people will eventually get sick and tired of the harassment and impasse of cold and hot partial wars and atomic war blackmail. Earlier, I heard this very often from audiences. A prominent businessman and other leaders in the community will stand up before four or five hundred people and say, "Well, if it's that bad, let's get it over with while we're ahead. Why are we waiting around? Let's go and do it now."

FREMONT-SMITH: You remember, Staff, the conference that we attended. We had a group of steel people from Pittsburgh, and on the first day of the conference that was their attitude.

WARREN: Yes.

FREMONT-SMITH: "If it's that bad, we'd better damm well have it quickly before everybody else can do it." We didn't end up with that mood but I think it is not an uncommon attitude and there are even some people saying this in Washington today.

BRUES: Another way of dealing with the frustration is with drugs. I wonder how much of the current drug usage is.....

FREMONT-SMITH: You mean we give drugs to Washington?

BRUES: Perhaps we should. [Laughter]

FREMONT-SMITH: Excuse me, sorry. You mean drugs to protect people?

BRUES: I mean, how much of this business is another reaction to this same frustration?

ROOT: You mean LSD?

BRUES: For instance.

ROOT: Yes. Drugs are one way to deal with frustration. I think so very strongly. You listen to the flower children talk—the bomb is coming and we've got to get out. I know one group that's really setting up a colony in the Amazon. They've got it all figured out that the Southern Hemisphere will get less fallout. Behind this kind of "there is no tomorrow" philosophy is very much the feeling that they have been betrayed, there's nothing they can trust. The only true experience is Now. In that sense we have lost a very serious war in that we are losing an increasing percentage of our youth.

FREMONT-SMITH: That's what I meant by the gap between the adult population and those who don't trust anybody over 30. Most of us are over 30 and so we can't be trusted.

BRUES: The ones over 30 have the other irrational way of dealing with it, which is to have it over with.

FREMONT-SMITH: Yes.

UPTON: Before we get too far away from the Marshall Islanders, I find it really quite intriguing that a population can be dusted, can develop burns, can be moved off their home island, can see their children stunted, can develop thyroid tumors and can accept this philosophically without great emotional upheaval.

FREMONT-SMITH: Have they really understood it?

UPTON: Yes. I would be interested in asking Bob to say a little more about how this situation was explained to them in the beginning.

FREMONT-SMITH: If ever.

UPTON: Do they really understand its implications? Do they worry about a recurrence, for instance? What do they think about it all?

CONARD: Well, it's really hard to know. They have sort of the Oriental viewpoint on things and they are a very phlegmatic type of people. Their reaction to this whole thing has been very calm and collected. They have accepted things as they have arisen. Moving them to another island to live, they took it in their stride. These people move around from island to island very readily anyway. They like to go over to Utirik or some of the islands to see other members of their families that are living there. It's nothing unusual. In the old days they used the outrigger canoes to go by family to the island and now they use the interisland cargo ship, the copra ship. They crowd on the decks of that and camp there.

FREMONT-SMITH: Have there been any anthropological studies made by Orientally-oriented anthropologists who might understand them; a Rorschach test for the Marshallese people?

CONARD: No, sir, not that I know of.

FREMONT-SMITH: I think this is the only way one could get an answer because one doesn't know what has been repressed in this so-called phlegmatic attitude. Our Negroes were also very phlegmatic and something unphlegmatic seems to be coming to the surface now.

CONARD: They certainly don't have any of the headhunting aspects that I had been led to believe existed when I went out there. I haven't seen it.

EISENBUD: I spent quite a while on various of the islands in 1956. This was two years after the event and there were tests in progress then, I guess—the Red Wing exercises. There certainly wasn't any official apprehension on the part of any of the natives. In fact, I think I spent one night on Utirik on an expedition. They were all very friendly and pleasant and somewhat excited by all that was going on.

WARREN: Historically they've had to worry about food and typhoons and drought and invasion by other peoples, not the least of which is the colonizing groups—the Japanese and now the Americans. While they probably have some radios and they hear a lot, they don't really have control over their situation and yet don't want to change it or do anything about it. I'm reminded about the time in our culture when tuberculosis and fatalities from lobar pneumonia were just accepted as being unfortunate. If the old man got kicked by a horse, had a broken leg and laid around and couldn't do the farm work, well, this was part of life. It was just tragic. It wasn't all right, but it was acceptable. We're not in this culture today nor in that frame of mind. We've got miracle drugs and we've been told about all of the advantages and the wonderful life, etc. Our young people don't see it our way. They haven't been raised in a family where members died at inopportune times nor have they lived in a general population which was close to the bare subsistence level.

Also, I think I remarked last time that our people came West with a gun and always had a gun handy and knew something about Indian fighting and predators of various sorts, human and others. We haven't had these experiences recently and a certain amount of self-reliance has now been lost.

FREMONT-SMITH: Do you mean we can't use the Marshallese experience very well to extrapolate what will happen in the Midwest after an atomic war?

WARREN: Bob has just indicated that people adjust, and I think we would adjust to whatever happens. What else is there to do? You've got to eat every day and sleep.

AYRES: It's interesting; there's a difference between our culture and the Oriental. We believe deep down that we can change our surroundings.

WARREN: Yes.

AYRES: In that we do not differ from our ancestors who went West with guns. We still believe we can change the Vietnamese, and that somehow we're going to make democrats out of them. We wouldn't be here if we didn't believe we can have some effect on our own country. But the Marshallese don't believe they can have any influence on their surroundings, and that's probably why they accept things like this. These are just Deus ex machina.

CONARD: As Staff pointed out, these people do show some of the characteristics of stoicism and the rigors of a hard previous life. They never celebrate a birthday, for instance, until a child is one year old because they're so used to them dying before they reach that age. They don't accept them as human beings before that.

WARREN: That would save them some trouble, wouldn't it?

CONARD: And the older people are all set aside. "You're old now. We'll give you some rice or something and you take care of yourself." But they love the children.

MILLER: Isn't there another feature? They are in an isolated place under American control and no one can reach them to inflame them or to pattern their thinking as in Hiroshima.

DUNHAM: People go down there from time to time and try to stir them up but they don't get very far.

CONARD: They got stirred up as far as to institute a suit against the United States Government. They tried to sue the American Government for something like $40 million. There were several lawyers that got into the case and tried to push the thing for them but that fell through.

EISENBUD: They've had several missionaries down there.

UPTON: Do they wonder why you're coming back every year?

CONARD: Yes. For a long time, for the first six or eight years, they were puzzled because we would tell them at the end of the examination, "Well, we find that everything is fine, that you're doing fine." Then they would say, "Well, why do you have to come back and take our blood and examine us again if everything is fine?" This is very difficult to explain to a naive group of people like that. But we did the best we could. We told them that we wanted to be sure that something might not develop. They accepted it gradually and over the years now

I think they look forward to our coming out. We consider them our friends.

UPTON: You speak of mobility. Have a number of them moved away? Have you lost any of the population?

CONARD: They have only moved to islands that are readily accessible to us. The "Paris" of the Marshall Islands is Ebeye, which is an island just next to Kwajalein, where a large number of Marshallese work for the government. We have a big Nike-Zeus testing station there.

AYRES: Relative to what you have been saying in the present conversation so far, of course, nothing serious has happened. They've had these nodules but they are not very obvious. What do you imagine would happen if something fairly visible occurred, let's say a skin cancer or one of these beta burn lesions, and you came out and made quite a fuss about it? Do you anticipate some very serious psychological reactions building up?

CONARD: I think they have the capacity to become emotionally upset about these things. They showed some degree of homesickness, for instance, when they were on the other island they were temporarily living on. Then the word got around, falsely, by the health aide that none of the women were going to be able to have children again, and this caused quite a bit of furor and concern until we were able to reassure them.

FREMONT-SMITH: How did they show this? Just in statements?

CONARD: I got the word indirectly. They didn't come to me with this but I heard that the health aide had told all the women that they were not going to be able to have any more children. Of course, this was a very bad state of affairs and we got all the people together and talked to them and explained that this was not the case.

FREMONT-SMITH: The crew on the destroyer I was on were all sure they were going to be impotent until I explained to them that the one thing they would not be was impotent.

IMPLICATIONS FOR DEFENSE POLICIES

TAYLOR: I would like to point out what I think is a deeply significant difference between a psychological reaction of the Marshall Islanders and people, particularly in the United States, who might become involved in even a limited sort of nuclear war. To the Marshallese apparently this was some kind of fairly important but not overwhelming catastrophe which just suddenly happened. We have conditioned ourselves as a country for twenty years now to a state of mind that says when nuclear war breaks out, all is lost. The whole idea of massive deterrence is built around the idea—it was built around the concept of making the war as bad as possible and therefore avoiding it. I think it's really clear from many things that have happened and many things that haven't happened, that it's our national policy not to accept as a working premise any kind of a nuclear war.

FREMONT-SMITH: That's right.

TAYLOR: Therefore, if it does happen, even in a limited way, it seems to me there's going to be a very deep psychological fact of life that will lead to a reaction to disaster which would be very different from the characteristic human reaction to disaster: that is, to rise to the occasion and do the best that one can. There will be a feeling of hopelessness that we have built up very carefully and thoroughly and almost studiously over many, many years.

AYRES: I could add to that. A lot of civil defense planning is predicated on the assumption that people will cooperate with agencies of the government and with its appointed representatives in an emergency. Yet privately I worry that if a nuclear war occurs, because of this conditioning that you speak of, one possible reaction is a great intensification of the kind of distrust that we've been talking about. "They really did us in this time. Now we can't believe a word they say. Lynch them, hang them from the nearest lamp-post. Don't follow directions. They're just leading us down a garden path."

MILLET: I'm interested in this apparent preoccupation with the anti-missile defense which seems to be one of the things we are struggling with Russia about most of all right now, with the possible exception of Vietnam.

If it is a true assumption that we're going ahead to increase our anti-missile defense potential, it would seem to me that any attempt to make any kind of recommendation would have to be focused around that possibility. This looks as though perhaps the tide might be turning away from this assumption of absolute disaster toward the possibility of some kind of defense and it isn't clear as far as The Times or other public media information states whether the orientation of this defense system is primarily anti-Russian or primarily anti-Chinese.

TAYLOR: Hasn't it been called primarily anti-Chinese by the Secretary of Defense? It seems to me that was fairly definitely said.

MILLET: Yes, it has been said, I think, but.....

TAYLOR: So far as the real reasons for going ahead with that decision are concerned, I think there's one interesting development that hasn't happened yet that will help reveal what was really in some people's minds in making that decision, and that is the decision with respect to fallout shelters.

MILLET: Yes.

TAYLOR: Curiously, in the recent decision to go ahead with the small ABM, whatever one wants to call it, I've seen no mention of any kind about any civil defense measures associated with that decision. It's always been coupled in the past, but this time it was not. The question is why?

MILLET: I was coming to that point in a sense. I was thinking that if we assume that this is going to be the policy, then this would seem like a great opportunity for public works possibilities for putting a lot of people to work to build appropriately distanced shelters to take care of a lot of people.

TAYLOR: The difficulty is that other few billion dollars which have not been mentioned so far as part of this decision to spend $5 billion on this active part of the defense. This would be an unpopular thing to promote.

FREMONT-SMITH: Do we have to consider the fact that we are just prior to election year? The policy is going to be influenced as much by the oncoming election as it is by national security perhaps.

TAYLOR: I'm surprised the fallout shelter issue has not been brought up again.

ROOT: Would this indicate that the distribution of anti-ballistic missiles had been worked out in a way that makes fallout shelter unnecessary?

TAYLOR: I don't see how, because no one, I think, is arguing that any ballistic missile defense system against any threat will be perfect; therefore there will be some leakage. If there really is an attack, there will be some exposures on some U.S. cities. This will simply cut down the number. There's a huge difference in the number of casualties that would result from, let's say, a single explosion; I mean, this works pretty well if there's only one explosion in one city. The difference between the casualties with some kind of a recovery plan and fallout shelters and no such plan is a factor between 10 and 100 in the number of people that would be killed.

FREMONT-SMITH: Do you think it's possible that those who are responsible for making policies are well aware of the fact that the public is not going to respond to the fallout shelter issue; that the whole thing has gone down, and that they just hesitate to propose a plan which is tied to a fallout shelter system? I think it would be very hard to get Congress to vote vigorously for fallout shelters now unless there was a much.....

AYRES: The presumption is that the Defense Department wants ABM?

ROOT: Yes. Given the credibility gap, it seems more like a trial balloon to test public reaction.

DE BOER: If one looks at the development right now, I feel that the United States and Russia, whether they have agreed in principle, both know that in an all-out attack between the two big powers they would destroy each other. Let us consider China or other potential sources and this feeling of security and certainty fades away rather fast. Our Government does consider this a threat since we are planning a defense system. But what worries me is how we will retaliate when provoked by China, or even more sinister, by an unknown source, such as was

pointed out to us by Dr. Taylor yesterday. Will there be mass hysteria if part of New York or San Francisco blows up, and will there be a cry for all-out retaliation? Retaliation in what? What are our plans under these circumstances? How much weaponry shall be used in order to retaliate against China; and if we have these plans, what is eventually coming back to us in terms of fallout?

TAYLOR: On Russia.

DE BOER: Or on ourselves as far as I'm concerned. Do we have any plans of this nature? I don't think we have. I think it's worthy of consideration in terms of an open forum, whether these plans are here officially or not officially.

AYRES: As long as you're speaking of China, a lot of people believe that the Russians are probably building their ABM system because they're worrying about China.

DE BOER: As Dr. Taylor mentioned yesterday, it does not have to be China. There are other sources quite capable. Are we ready to make up our minds as to how to proceed? Also, fallout has to be considered if China attacks Russia alone.

FREMONT-SMITH: And you put this out very effectively. How about if we don't know?

DE BOER: The question is how much of our strength is necessary to subdue the enemy? It is easily done if one knows the threat. Even in the case of Russia, we have enough weapons.

TAYLOR: Not if we don't know who did it. Who do we go and hit? Kill everybody?

DE BOER: We'll have an idea as to where it comes from—from the east or the west.

TAYLOR: All of a sudden a bomb goes off in New York. It went off because somebody put it into the basement of somebody's house.

HEMLER: I say it doesn't have to come in by ICBM to go off.

WARREN: It can come in on a ship in the harbor and go off in the harbor.

TAYLOR: Off a ship in the harbor on to a truck and wherever they want to put it.

DE BOER: Let's think about it now. This is the very point. We may even have to do something about it now.

WARREN: That was the argument.

FREMONT-SMITH: We can't possibly get a record if everybody talks at once, which is natural for us to do under the excitement.

WARREN: I wondered, when this came out, whether this wasn't part of the cold war effort by China, to have us thinking a bit more about the situation (blackmail). They have relatively few weapons and trying it, risking our uncertainties and unwillingness to really retaliate, might very well be the case. Could we stop a couple of them with our defense missiles, which might settle the matter without our getting grievous injury? We might not get involved, but they're not going to send them in clouds for a long time; they haven't got the potential yet.

AYRES: I don't see that they have any such intention. They haven't even attacked Quemoy. Yet, everyone seems to agree that China is a great threat.

TAYLOR: There's no reason that I can see that we are taking any kind of comfort in the notion that they just have two or three. They've already exploded a little stockpile.

ROOT: As far as the unknown threat is concerned, we know which nations have the capability.

TAYLOR: Ninety-six nations at least.

ROOT: Have already tested?

TAYLOR: No, no. Have the capability.

ROOT: But they won't send it over until they have tried it out and we'll know when they test. Anybody with an Atoms for Peace Program has the capability.

TAYLOR: There are 96 countries with an Atoms for Peace Program. That's what I mean.

ROOT: I would not think you could anticipate the delivery of a hydrogen bomb from a country that had not already tested. We know Great Britain is no threat to us. It would be either Russia or China.

TAYLOR: The fission bomb is a different story. That could be delivered by anybody.

ROOT: Yes.

BRUES: I think the timing is another question. It seems to me that, at least in our public statement—I don't know about our inside knowledge—we have consistently underestimated the rate at which the development would be made in other places.

FREMONT-SMITH: Yes, every time.

ROOT: The lag has always been less than we have given them.

FREMONT-SMITH: That's right.

WARREN: You can't tell whether it's a fishing expedition. They may know a good deal more but they made the charge and then see what happens when they get a rebuttal; more information comes from it. The trouble is we've got all kinds of activities at different levels that we do not know about.

FREMONT-SMITH: We don't know perhaps about each other.

UPTON: You mentioned some American personnel on an island nearby there. Are data available on relative degrees of contamination in comparison with the ground level in these groups? I myself am wondering to what extent sheltering was effective under those conditions.

CONARD: Well, they were certainly quite effective from the point of view of the skin contamination and the internal absorption of materials.

UPTON: And the thyroids were not particularly hot?

CONARD: We didn't examine the thyroids from that point of view originally. We didn't suspect that they would have any thyroid accumulations at that point. But the American servicemen definitely had

fewer skin lesions and lower body burdens of radionuclides. However, their gamma exposure was probably more in line with what you would expect from the amount of fallout that occurred on the Island.

UPTON: Let's assume hypothetically that one could have been able to predict that contamination on Rongelap and to send warning to the natives there. In absence of a shelter, could they have done anything under the circumstances?

CONARD: I can't see how they could have done anything to avoid the whole-body gamma exposure.

EISENBUD: They could have gone to sea in their canoes.

CONARD: They don't have enough boats to get the population to sea.

EISENBUD: Even if they stood in the lagoon for several hours, this would help.

DONALDSON: Yes.

DUNHAM: They came back and walked in it and got it on the backs of their feet and got the skin burns. They would have to stay there until they were removed.

EISENBUD: You would cut down the external gamma dose considerably by just going out in those outriggers.

CONARD: What are you going to tell them on the radio, "Everybody go out in the lagoon and stay there?" Or what?

FREMONT-SMITH: Exactly. "Eat fish."

EISENBUD: I don't think you could do it without advance preparation. But to answer Art's question, I think there are things that could be done.

CONARD: You mean with some advance planning.

EISENBUD: Yes.

CONARD: I thought he meant under the circumstances as they existed in the village then. If we had contact with them, is there anything that we could have told them to do to protect themselves. Is that what you mean?

UPTON: Yes.

WARREN: How long did the white ash fall?

CONARD: About 12 hours.

WARREN: So they couldn't have been standing neck deep in the lagoon for 12 hours.

CONARD: No. They would have had to hold their breath and go under water.

UPTON: Because one may imagine that the best preparations are not likely to be made in the event of such a thing in the future, one may have to improvise in every case.

WARREN: So you have to have the knowledge to know what to improvise for.

FREMONT-SMITH: You would have to have about seven improvisation plans depending on what actually happens.

WARREN: Yes.

UPTON: We know that reactors are going to blow up from time to time. They will be localized events. What does one do in a case like that?

EISENBUD: It's an altogether different problem, Arthur. They don't blow up. This is a misconception.

UPTON: Well, take Wind Scale.

EISENBUD: Wind Scale didn't blow up. You said there was great fatality in the event. I'll let you take literary license with it. What happened was that the lighting failed and the fuel began to burn and it went out over the countryside. That's generally the type of accident you could expect. With the melt-down of fuel and the release of the volatile constituents, unless we are awfully wrong—and I don't see how we could be at this late date—the only exposure would be to the radioiodine.

BUSTAD: Possibly the cesium.

EISENBUD: Yes, and the radioiodine problem would be greatly potentiated in areas where there are crops and forage and dairy cows. This is manageable in a variety of ways. But I don't think the problem is anything like what you would have from a nuclear weapon. I've often thought that the single thing a person could do in a metropolis in the event of an attack, assuming that the weather conditions permit it, would be to get in a small boat—and the smaller the better—and get out in the middle of a lake and just stay there.

DUNHAM: With an umbrella! [Laughter]

EISENBUD: Well, you would get your beta burns in that situation and you could probably take care of that, too. If the Japanese had been on a larger boat they all probably would have died. One thing that saved the Japanese was that it was a small boat and they were not in the middle of an infinite plane. If they were, the dose could have been as much as three times higher.

DONALDSON: It rained a great deal, too.

EISENBUD: If there are not too many other boats there, of course, you might get by.

SPEAR: There are lots of ways in which you can help yourself.

WARREN: As Ralph says, there are lots of ways in which you can help yourself. You can get under a roof that is fairly high and the wooden building would get you some attenuation.

EISENBUD: We're not considering mass evacuations, blast, fire and things like that.

CONARD: I might say another word or two about the treatment aspects. Of course, we know that under the best of hospital conditions we can save a person from two or three times the LD-50 dose, perhaps, by giving him very careful attention with blood transfusions, platelet concentrations (and perhaps white cell concentrations), the use of antibiotics and by maintaining fluid balance, and so forth. But it really takes quite a hospital staff to take care of even one serious radiation casualty. So this sort of thing would be out of the question during the time of nuclear warfare.

I think the best we could hope for is to stock up on the antibiotics and perhaps plasma and have these things located at strategic areas for use.

AYRES: It's also important not to waste them on the worst cases.

CONARD: Yes.

AYRES: How do you manage that? Our normal peacetime philosophy is always to give most attention to the most serious cases.

CONARD: How are you going to get the laymen to decide whether this case is fatal or not without any blood count or any other means?

AYRES: I'm asking.

CONARD: You could go somewhat on the degree of symptoms of nausea and vomiting that occur early. If that is very severe and prolonged, then you could suspect that survival would not be likely.

AYRES: Doesn't it seem reasonable to have simple pamphlets or instructions giving a kind of range of symptons that it's worthwhile using these supplies for?

CONARD: In general, if a person survives two weeks in a fallout shelter and then gets sick, he's a pretty good candidate for antibiotic treatment.

AYRES: In other words, perhaps the first rule would be "Don't use them at all for two weeks."

CONARD: Yes.

DE BOER: I think you are ahead of the game. What you are talking about simply is not there yet. Sure, we can talk about those things around this table, but before we can reach a reasonable consensus of opinion, millions of dollars have to be spent. The points I like to make are: How do we create public interest in these matters without causing mass hysteria? And let me tell you, public interest is a must if we want support. How do we set priorities? The priorities must be not only relevant to biology and medicine, but more important, relevant to our national goals. Is it more important to have better hardware going to the Moon, Venus or Mars, or hardware for a war to be fought in the future, than to have some fundamental knowledge about what to do today in a case of emergency?

DUNHAM: These space programs are still peanuts compared to the Vietnam War. I think your Civil Defense right now is competing like everybody else with the Vietnam War. I think it's as simple as that.

DE BOER: I don't necessarily agree with you on that. The Vietnam War costs us a lot of money, true, and there may even be an argument whether it is a worthwhile cause or not. But we are selling ourselves short if we let the Vietnam conflict be the reason that stands in our way of making progress which eventually may save our skins. The entire expenditure of the DOD cannot be laid on the doorsteps of the Vietnam conflict. For a true cost of that conflict, one would need an economic analyst. The facts are: (1) We are in Vietnam; (2) We need to know more about how to defend ourselves when attacked with nuclear weapons; (3) We do not have a sound and well-thought-out priority system—a defense system based on the best this country has to offer. Indeed, we argue, we compete and work hard, but not on the real issues.

FREMONT-SMITH: Supposing that the Vietnam War was stopped tomorrow, is there any likelihood—and I think it's highly unlikely—that the money which is now being used for the Vietnam War would be used for Civil Defense? I don't think it would be at all. I think it would be used for a variety of other useful things, but I think it would take a tremendous something, a change in attitude, to get any significant use of money for Civil Defense, whether there's a Vietnam War or not.

TAYLOR: I think that change is taking place and this is independent of whether the end of the Vietnam War comes, and the change is simply a transition from a state in which we are able to rely on stable deterrents to a new world in which we can't.

FREMONT-SMITH: In which there are no deterrents that we can rely on.

TAYLOR: There's beginning to be a sort of awakening in this country.

FREMONT-SMITH: Have you seen some signs of this?

TAYLOR: Yes, by all means. More and more people are concerned about criminals using nuclear explosives for all kinds of uses in which it is not necessary for them to identify themselves to serve their purposes. The material is becoming much more available. The combination of these things is making it much more rational to imagine some kind of even very limited use of nuclear explosives for violent purposes. As soon as that begins to be a really understood threat.....

FREMONT-SMITH: Since it's been used once.

TAYLOR:.....then the whole situation will change. I think that will happen certainly within two years.

ROOT: I remember after the release of the Bravo information there was a great fear that nuclear weapons might be brought in secretly. A lot of regulations were passed for tightened inspection. The FBI issued directives about examining all luggage coming into the country and they gave specifications; a gun bore so many inches across was the tip-off. I haven't heard anything about them since.

TAYLOR: I don't think they are enforced. I've gone to some pains to try to imagine how, on this last trip, I would have brought in 6 kilograms of plutonium. What people forget is that the important part is plutonium and U^{235}, as far as what's not generally available is concerned. Depending on what it is that one is trying to put together, requirements can range from material that is available down the street to material which is very difficult to design, and there's everything in between.

WARREN: May I go back to one thing we just touched lightly on, and that is this problem of the triage. A person gets bad burns and lethal rads yet he won't die for a week, and so on. We had a big push in Civil Defense and in the Atomic Energy Commission for dosimeters. I can remember a very serious conference on this in which people began to really face the situation and it was agreed that you couldn't do this. After that there was a general let down on these dosimeters all around. There are good ones; there are stable ones.

FREMONT-SMITH: It would be too disturbing for people to know about that or for anybody else to know that they were already dead.

WARREN: That's right. This psychological factor is something that you have to deal with.

FREMONT-SMITH: It's somewhat the same thing if you are in a shelter and your neighbors come and want to get in and there isn't room for them, which ones do you shoot? I mean only the ones that are over 65?

WARREN: Yes. It would be a good idea to re-examine the situation.

EISENBUD: One of the things that bothers me, having lived through this almost as long as you—I guess Stafford Warren has got two more years than I have, and after 25 years that doesn't seem important because these were exciting years—but I got into this field when we were thinking of one or two bombs, air-delivered because you had to consider their effect on blast, and so forth. That was 1945, when we were talking about 20 kilotons delivered by a propeller aircraft. In 1955 we were already talking about 20 megaton bombs delivered by jet aircraft. By 1965 the ICBM systems on both sides were pretty well dispersed, presumably by the hundreds, maybe by the thousands; I don't know. Now we're talking about 1975 when we expect to have an anti-missile system employed, and the impressive thing about this is that the technology has gotten to the point where you can even think of knocking a missile out of the air on about 10 minutes' notice or whatever it is. It may be less, and if the technology is that advanced, then what are the delivery systems going to be like? If the defense system has advanced to this point, what are the delivery systems going to be like in 1975 when we see what has happened in the last 20 years?

ROOT: Probably they'll all be obsolete. We'll be using lasers.

SESSION IV

ECOLOGICAL ASPECTS OF WEAPON TESTING

(Initiator: Lauren R. Donaldson)

SESSION IV
ECOLOGICAL ASPECTS OF WEAPON TESTING

INTRODUCTION

DONALDSON: This morning we should be able to give free rein to our scientific acumen as well as our imagination in our discussion of the environment and man's relationship to it. I'm sure we all have very specific comments and very specific opinions about how man relates to his environment. In the area of weapons testing, also, I'm certain we have an equal number of opinions on the effect of this activity on man and his environment.

I took our convener at his word that we were not to write speeches, we weren't to deliver orations; but after 41 years as a school teacher I'm tempted almost as if by heritage—my mother and my grandfather were also school teachers—to deliver the morning lecture which should come 22 minutes from now on normal schedule.

FREMONT-SMITH: We have 30 years of interrupting practice! [Laughter] We expect to challenge your 41 years.

DONALDSON: Looking around, I notice there are many school teachers in this gathering. I'm sure they will use the professor's prerogative to interrupt at any moment.

To set the scene, I should like to limit somewhat the parts of the world we are going to talk about. Figure 22 is a map showing the areas on which we shall concentrate.

Each of us has his own immediate interpretation of what we think of as environmental contamination. I think if we go back to the source for many of our problems we would go to the Hanford Works or to the Oak Ridge National Laboratory and eventually to the Savannah River plant, where materials are fabricated. We have learned to live with radiation in these areas and we have learned a great deal.

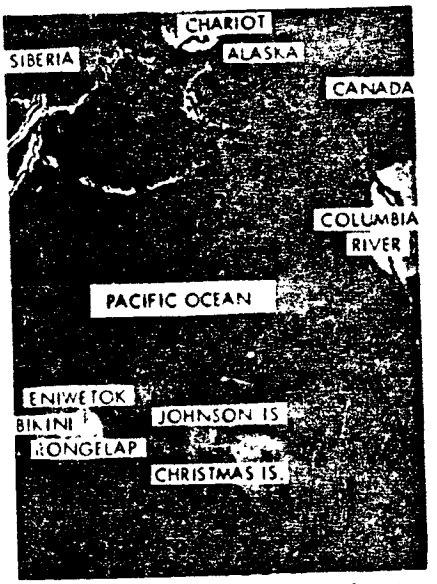

Figure 22. Map of Pacific, showing locations of major study sites for the evaluation of radiological contamination. From L. Donaldson

Or, we could concentrate on the Japanese problem, as we have in preceding sessions talked about the experiences at Nagasaki, Hiroshima, and on the mainland of Japan. Or, we might drop down to the Marshall Islands and concentrate on Rongelap Atoll and the fallout problems there, as we did in part yesterday, or on Bikini, or Eniwetok. However, with a few jumps, I should like to include some of the other areas in our discussion this morning, to give those of you who have worked in other areas a chance to participate and to bring in some special problems, as follows:

1. Johnston Island, where we have some of the usual type of problems because of an accident, which is not discussed usually but one which I think is germane to our deliberations here;

2. The Christmas Island area with yet another problem;

3. The northernmost tip of the North American continent up at the Chariot site, where Dr. Wolfe and his associates (Reference 35) have gathered a good deal of both factual and projected information on the problem of environmental contamination.

I did not include here the Amchitka area in the Aleutians, where, as many of you are aware, there have been some atomic detonations; they are preparing for one at the present time.

This presents a very big order in itself. It includes about 50 percent of the earth's surface and some very unusual environments. I think it would be well if we could include some information from the British tests of 1952 and subsequent years, particularly one off the Great Barrier Reef, which is germane to our discussion here, and possibly a word or two about the Russian tests. The Chinese tests were mentioned yesterday. For some reason—I don't know whether it's policy or not—the French tests in the Pacific and in the Sahara were not included in any of the conversations nor was there any comment. I guess this may be omission by purpose but it's not for me to decide in this case.

FREMONT-SMITH: There's no known policy behind that omission.

RADIOACTIVE CONTAMINATION OF PACIFIC REGION

DONALDSON. I think it's important that we consider the French tests in the overall problem of environmental contamination, particularly as far as the Pacific Ocean is concerned.

To be a bit more definitive as to locale and orders of magnitude, Figure 23, for the sake of contrast, superimposes the scale map of the United States over the area we will concentrate on, showing the Pacific testing center, which includes Johnston Island, Christmas Island and the Bikini and Eniwetok Atolls. (See Reference 36).

Specifically again, if we may review our natural history for a moment, atolls are most unusual structures. I like the statement found in "The Voyage of the Beagle" and others of Darwin's writings, that no biologist can really be considered a qualified biologist unless he has lived and worked in a coral atoll. These masses of coral are unique biological entities, and, I'm sure, those of you who have worked at Bikini and Eniwetok, or at other atolls, have cussed them or enjoyed them as your temperaments dictated.

There are dead atolls, such as Christmas Island, where the growth rate is not quite equal to the sloughing of the atoll. There is a great deal of scientific discussion as to how the atolls were formed. There was an almost complete lack of understanding of the formation of

Figure 23. Scale map of the United States superimposed over area of major test sites in the Pacific Ocean. From L. Donaldson.

atolls before the Pacific tests were initiated. I recall that the geologists in the group were convinced that the coral was a cap, and in some of the lectures on the Haven that were held under Dr. Warren's supervision, we were told that the coral was about 180 feet thick. This was supposed to be so because during the ice age the water had receded to about that level, thus the coral could only grow in the upper warm layer of water. Therefore, there would be just a little cap of coral. Many discussions were held as to the possibility of blowing this cap off the top of the mountain on which the coral was superimposed.

These discussions went round and round, Dr. Warren, you recall, during the voyage of the Haven out to the test ground and we listened very intently. In subsequent expeditions to the Marshalls it was possible to drill in the atolls to actually determine the thickness of the coral. During the 1947 expedition particularly, the drilling was geared to go down as much as possibly 1,000 feet into the base. Each morning when the assembled group would go out to drill we would ask them how they were coming.

"When you're down to 100 feet you ought to be striking base rock the next day."

"Yes."

"Then we'll be able to tell how old the earth is because geology is exact science."

FREMONT-SMITH: You remember I mentioned the half-life of facts is getting shorter and shorter. I'm glad to have it illustrated.

DONALDSON: The next day they might be down 200 feet, 300 feet, 400 feet, 800 feet, 900 feet, and they were quite convinced they were in a hole and they had to change their estimation of the thickness of the coral, which meant they had to revise their estimate of the age of the earth, and in turn their concept of how the moon was formed.

FREMONT-SMITH: And that includes the tides.

DONALDSON: Yes! [Laughter] And this went on until they finally reached a fantastic depth of about 1,200 feet and they still hadn't found out how old the earth was or how thick the coral cap might be. By this time we were running out of food and liquor, which worried everybody because the supply planes were just bringing mud to grease this hole they were drilling down into the atoll.

The following year the geologists moved over to Eniwetok and began to drill there. They drilled down to a total of some 4,300 feet before they came to the basal strata on which the coral was anchored.

FREMONT-SMITH: They did find it there?

DONALDSON: Yes. They actually found that there was a bottom to this boundless pile of calcium carbonate.

The illustration, I hope, is not wasted. But it's indicative of some of the needs to know in the natural environment in which we are working. The seas and the atolls within the seas are so imperfectly known that we sometimes find great gaps in our thinking because we don't have the physical and biological parameters upon which to base our work. It's like the statement of the senior Senator from our State who repeatedly has said that we know a great deal more about the back side of the moon than we do about the oceans that cover 72 percent of the earth's surface. With this as background, maybe we can be a bit more specific in the things that we are going to be talking about.

The tests were conducted, as I mentioned, at these various atolls and we may take a quick look, starting in 1946—not in 1947, as in the statement in the first volume. Figure 24 is a photograph of Eniwetok.

The atolls were selected, according to the Task Force reports, because they presented an ideal environment in which to work. They were isolated, they were in relatively favorable weather areas, and they did provide a safe anchorage for the fleet; probably equally as important, there were a number of outposts were instruments could be based.

As far as those of us who were interested in the environmental sciences were concerned, the atolls were ideal because they did provide a good cross-section of native flora and fauna. Each of these tiny islets has a peculiar environment quite its own, just as the entire atoll has its peculiar type of environment.

The emergent land area, about three square miles in each of the atolls, is divided up into some 20 small islands in each atoll. The land plants are limited to those forms that can survive in a tropical environment subjected to wide variations in temperature and salinity.

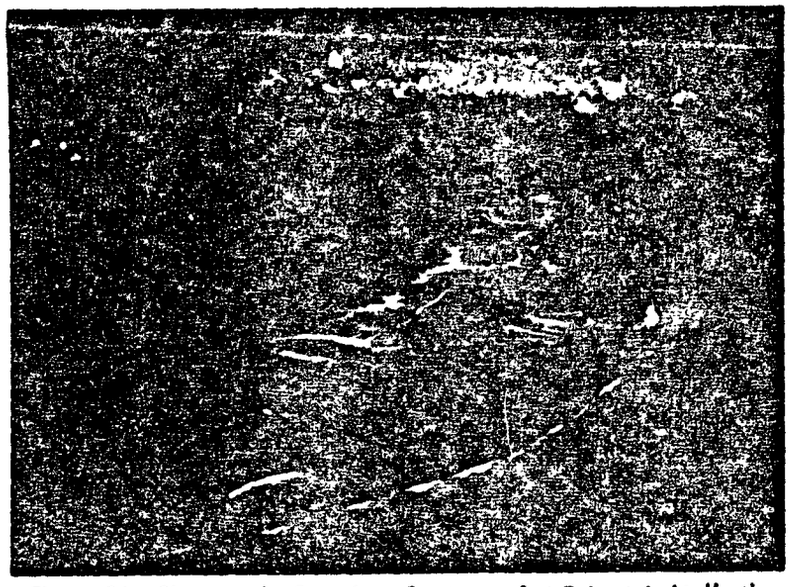

Figure 24. Photograph of portion of outer reef at Eniwetok Atoll, the site of numerous weapons tests. From L. Donaldson.

The land animals are limited to one group of mammals, three species of rats which were introduced apparently at the time the native people came to the atolls. The birds are limited to those aquatic birds that can fly long distances.

In contrast, the marine fauna and flora are extremely diverse. There are about 700 species of fish, compared to the 50 to 70 species in Puget Sound, where I work in my normal activity. The same is true with the algal groups, found in great diversity. Then there are the corals, which are something unique to this part of the world.

UPTON: To what extent do you think the limit in number of species in Puget Sound may have resulted from the effects of man on that basin?

DONALDSON: These are forms that were native there. We have introduced some forms. There are no species that have been exterminated in Puget Sound. All of the native forms are there.

Added to the complexity of the environment and the great distances, we have the problem of a great diversity of energy releases from the different types of nuclear devices. Just to review quickly, there have been 59 detonations at this test site; they varied in size from the normal device we've talked about, some 2,000 tons of TNT, on up to 11, 12 megatons, according to one statement. This was the March 1st test of 1954.

These devices have varied from rather primitive ones by present standards to some sophisticated ones up through the 1958 tests. They were detonated under a great variety of conditions, and this is germane to the subject we're discussing, from under water to high in the skies, from tower tests to tests on barges anchored in the water. This means that the fission products varied not only in quantity, and somewhat in composition, but also that the induced radiation varied fantastically in both quantity and composition. Thus, the numbers and amounts of radionuclides introduced into the environment run almost the entire gamut of possibility.

To try to evaluate in this weird and wonderful environment the impact of the detonations upon the biota presented a formidable task that would stretch the imagination, I guess, of most of us; at least it stretched ours.

We tried to determine—and I'll enumerate these studies rather quickly and then get on with the discussion aspects—the amounts and kinds of radioactivity released into the environment. Obviously this is important, but I would call your attention to the primitive nature of the instruments and of the evaluation techniques that were available, particularly during the early years. We are inclined to think in terms of what's available today rather than what was available in the hectic years of 1943 to 1946, and even in subsequent years as we went along. I recall that we borrowed a scaler, an old Victoreen scaler, from some of Dr. Warren's people and we would cut off a piece of fish tissue or some pieces of algae and push this material in the counter. If it went off scale we would say, "Well, there must be some radiation there. Throw it away and push in the next one." It was essentially a presence or absence situation in many instances. There was either some radiation or there wasn't. I will have to qualify my statement as to the determination of the amounts and kinds of radioactivity, which came somewhat later.

We were particularly interested in the uptake of radioactivity by biological systems and this again was dependent upon good instrumentation, which, as I have said, wasn't available during the early years. We were interested in the amounts and kinds of radiation within various systems, the selectivity and the concentration. This becomes germane when we begin to talk about permissible levels. Some of the algal groups will take out one radionuclide; for example, one will pick out iodine and concentrate it in the order of magnitude of 18,000 times the amount in the surrounding water (Reference 37). These blotting techniques then are very important to the overall evaluation because this alga is eaten by some of the fishes and the fishes in turn then will contain the iodine. The most specific radiation damage that has been measured directly is the destruction of the thyroid in some of the algae-eating fishes.

DUNHAM: May I interrupt and ask you what kind of stable content this does have normally?

DONALDSON: In Asparagopsis, iodine constitutes 0.092 percent of the dry matter.

DUNHAM: Does it have a high iodine requirement for survival?

DONALDSON: I don't know the physiology of it.

DUNHAM: When you say it concentrates, you mean compared to the concentration of radioiodine in the water?

DONALDSON: Yes. The total activity of the water was 24,000 d/m/g, and the activity from the I^{131} in the algae was 30,000,000 d/m/g.

UPTON: Rapid iodine turnover in this organism?

DONALDSON: I rather doubt it. I think it probably is maintained at a relatively high level and the limiting factor may be the amount of iodine available to it.

UPTON: Is it a rapidly growing plant?

DONALDSON: Yes. It grows rapidly.

UPTON: So that it's building new cells and building in new iodine.

DONALDSON: Yes. I think the specific concentrations are really germane to this sort of discussion because we base our interpretations on the familiar and forget that in nature there is a wide variety of uptake.

We were also interested in the rate of transfer and elimination. In the discussion yesterday, Dr. Warren mentioned the radioactivity on the sides of the ships, but if you recall, these ships were always upwind from the detonation. Thus the question would be, "How did the radionuclides, which would normally drift downwind, work their way upwind and become concentrated in the water around the ships, to be accumulated by the organisms on the sides of the ships?"

Of interest, too, was the dispersal in the open ocean. One of the intriguing things was the "breathing" of the atoll. It appears to pulse with the tide. Of course, it is the nature of an atoll to be constantly leaching material out to the open sea. There are other interesting transfers that we will be talking about, I hope, as we go along.

The usual transfer in our terrestrial areas is from the land to the sea, but in these atolls there is a very appreciable transfer, which takes place in a variety of ways, from the sea back to the land or the limited land area. Radionuclides are transformed by spray into the vegetation, and we find that this is a positive transfer. This occurred

in Japan to some extent, as those of you who followed the movement up on to the terrestrial area there can verify.

In the atolls, the more specific ocean-to-shore transfer of radioactivity is carried on by aquatic birds. This is a complicated thing to evaluate, because the birds carry back on shore nuclides selectively concentrated in the food web in the sea. This added group of nuclides is often very different in percentage composition as compared to the components in the original fallout on the land.

Then, I suppose, most specifically we are all interested in the amounts and kinds of radionuclides concentrated by the various tissues and organs. In making evaluations, we are always hard-pressed to sort out the different parameters that are involved. We have the overlay of the effects of blast and fire with radiation intermingled, especially in the closed-in areas. If I may, I should like to use an illustration or two to point this out.

Figure 24 is one of my favorite photographs. It was made under rather unusual circumstances, since we do not rate sufficiently high on the Task Group priority list to have the luxury of a photographic plane. We do travel occasionally—we did in the early days—by the older PBYs. Those of you who remember those old flying boats know that they usually didn't have the usual facilities found on a modern plane but they did have a place in the back called an air-flush toilet. By flipping up the lid of this fixture you had a place to take a photograph! [Laughter] This may be a bit unusual.

Figure 25 is an illustration of the type of plan that might be used to document some of the things I've been talking about. We, like the rest of you, tried to be very exact in our planning. We planned very carefully to document the distribution of radionuclides in this great mass of moving water and drew a navigational plan In order to accomplish this objective we had to occupy various stations in some logical sequence, so I drew these plots to guide us. We started at the point near A, A-1, and we made zigzags on the track, cutting back and forth and finishing up some weeks later at a point E-10. Everything is nicely in order now. Figure 26 illustrates the way it eventuated! [Laughter] We started, we went along very well and were going pretty much on course, except that the problem of doing oceanographic work from a destroyer presents its own problems. When we first started out we asked the skipper, in this case, of the Walton to let us go 50 miles this way and then stop. "Stop? I don't

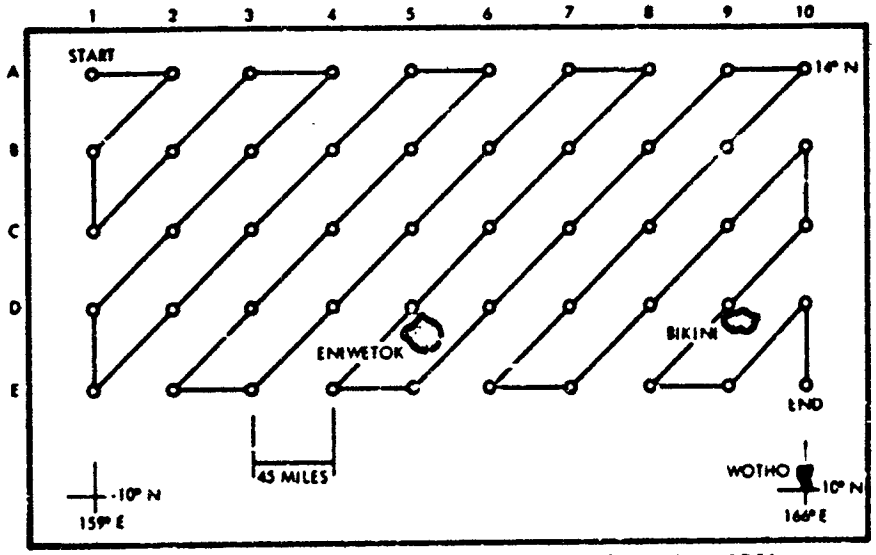

Figure 25. Proposed track of the U.S.S. Walton, June 1956.
From L. Donaldson.

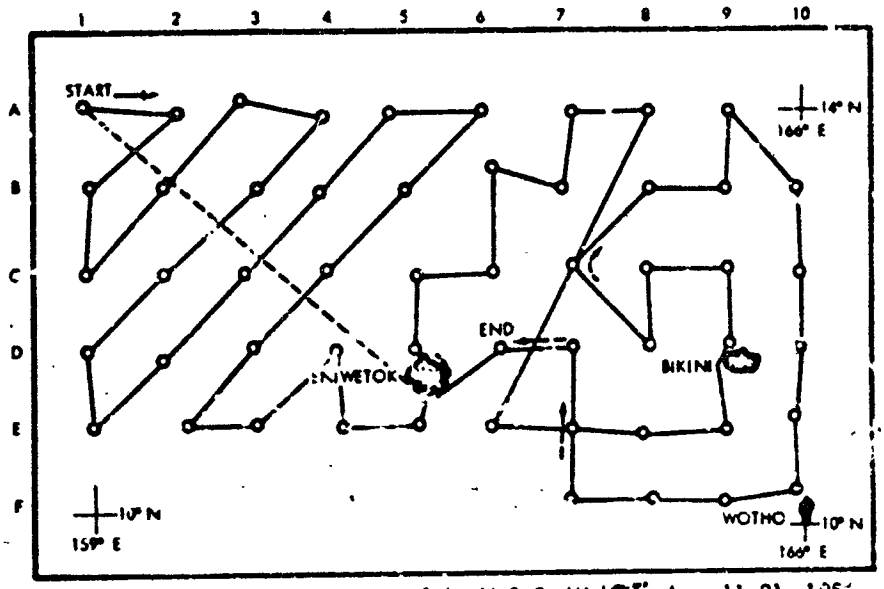

Figure 26. Actual cruise track of the U.S.S. Walton, June 11-21, 195[illegible].
From L. Donaldson.

know how to stop. I've never stopped this ship in the sea. What will happen if I lie there? [Laughter] You can't put this group of wire lines and rope lines over the side. You may get them caught up in the propeller if we stop." But he decided to stop. Then afterward we drifted awhile and lost course completely.

This problem of navigation really surprised me out there. Bob, you have lived with it for years, but it's so much better now than it was in the early days.

In 1948 when we were out there all by ourselves, we had one little ship that had no way of producing potable water. The Navy would very helpfully send us a ship every four weeks with a new supply of water. On the back of the little supply ship was a small box-like compartment in which 6 Marshallese boys lived. I asked the skipper of this ship, "Why do you have these Marshallese boys in this compartment on the back of the ship?" Actually it was a little cover on deck where they lived. He said, "Those are my navigators!" [Laughter]

"You have all the modern equipment."

"Oh, we have a compass and the sextant and the usual things," he replied, "but we don't have radar or any sophisticated equipment on this little ship. I couldn't get along without these boys to do the navigating."

CONARD: Did they stick maps?

DONALDSON: No, they just used their own intuition in this case.

FREMONT-SMITH: And their ears.

DONALDSON: Yes, their ears and their eyes and their built-in compasses. The story that he told seemed perfectly fantastic, so fantastic that it's worth repeating because it's incredible, as Wright was saying yesterday.

It seems that when this skipper first arrived to take this command, he was asked to take the ship from Kwajalein to Wotje Atoll. Wotje is 200 miles east of Kwajalein. Some of you who were out there in the war remember it was the place where they used to have the "milk run." They would go out and bomb it every day. So he set out for Wotje. His executive officer also was new; they plotted their course

—just two officers aboard this little boat—and when they arrived just where they thought they should be, they saw only the great big Pacific Ocean. They looked around and talked to the sailors awhile; the sailors were very reserved, of course, as sailors would be. This was the new Exec and the new Skipper and they didn't want to commit themselves. So they said, "Well, we'd better plot it again." They plotted again and they came out with this point and they were still in the big Pacific Ocean. In all fairness to them, atolls are very difficult to spot. They emerge only a few feet. Atolls of about eight feet with palm trees, mixed in the haze and the waves, are very difficult to spot. So they couldn't see it. They weren't close enough. Finally one of the sailors said, "I suggest you ask the Marshallese boys." Whereupon the skipper said, "I'm a graduate of Annapolis; I know how to navigate a ship," and his back went up. But finally in desperation he said to the boys, "Well, do you fellows know where Wotje is?" Their reponse is typical of their behavior, never a direct response. "We'll think about it for a while." This is a lesson some of the rest of us might learn. Rather than blurt out a quick reply, "Why, let's think about it for a moment."

So they had a little huddle; they walked around the edge of the ship; they looked in the water; they looked off at the sky; they had another consultation and they said "Wotje that way" (indicating). This was a great help. At least the skipper knew the direction to go! [Laughter] He thought maybe since these boys were so damned smart, he could ask them another question, "How far is it to Wotje?" Another consultation, another walk around the ship and another huddle, and "Wotje 40 more miles."

"Well, we're lost. We might as well try this." He commanded, "Sail that way 40 miles." They later arrived in the harbor of Wotje and dropped anchor and everything was lovely. The skipper began to think about this. He gathered the boys together again and said, "How did you know where Wotje was?"

"Oh!" This was a very serious problem. Another huddle, and another bit of discussion and then the great announcement: "Wotje always right here!" [Laughter]

FREMONT-SMITH: I think I have to give another aspect of this same story because as I was coming back from Bikini I was on a plane with a Navy captain who told me a very similar story.

They were in the fog trying to get into the entrance to an atoll—I've forgotten which one, it may have been Kwajelein but I don't think so. They had a native on the bridge and the native said to the skipper, "I think you've gone past the entrance." The skipper turned to the navigator, who said, "No." They tried to get in and found they were up against the sand. They went on and came back again, and then the native told them just where the entrance was. They went in at that point and he was right. The captain said to the native, "How did you know?" "I could tell by the sound of the waves," was the response of the native. You probably know that the winds provide waves that hit the atoll which then produce a backwash that flows out from the atoll. This activity makes a perfectly steady lap, lap, lap on the side of the ship. When you come to the break where the entrance is, there is a shift in the sound because the waves differ. The captain told me that this was so fascinating to him that the next day he flew over the atoll, and, by jingo, you could see these waves flowing out in circles and the break in the waves at the point of the entrance. Does this fit in with your experience?

DONALDSON: Yes.

FREMONT-SMITH: But I like your story better! [Laughter] "It's always right here" is the best thing I've ever heard.

DONALDSON: Let's have a look at a few illustrations.

Figure 27 is the Oak Shot. In the detonation, of course, we have some blast, some fire and some radiation. The meteorologists always are exact in determining the way the fallout is going to go. Sometimes they are right. It will leave a trail across the lagoon or in the sea, which one may or may not be able to find.

A detonation on the islets will produce a variety of effects. It will knock some palm trees over (Figure 28), break them off, and you can say, "Yes, the blast pressure was such." Here you can make a direct measurement of the amount of blast it takes to fell a palm tree. It's an appreciable amount of energy. Palm trees are made to resist winds of almost hurricane force.

It takes a certain amount of thermal energy to burn the leaves (Figure 29). One can make some exact compilations here of the amount of thermal energy that was produced at x number of miles. You can make this measurement directly.

Figure 27. Atomic explosion, Eniwetok Atoll, Marshall Islands. From L. Donaldson.

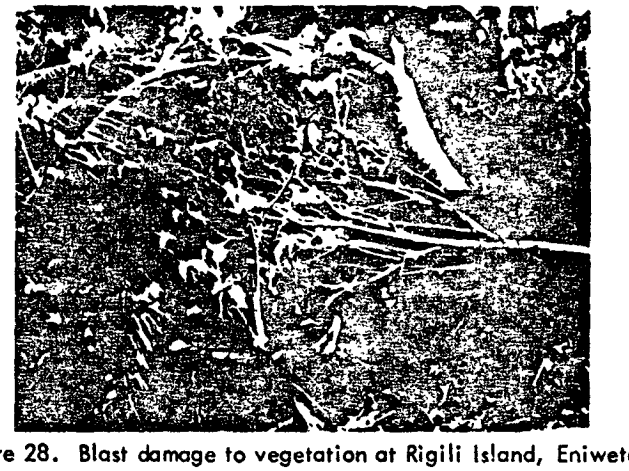

Figure 28. Blast damage to vegetation at Rigili Island, Eniwetok Atoll. From L. Donaldson.

Figure 29. Thermal damage to vegetation at Rigili Island, Eniwetok Atoll. From L. Donaldson.

Then there are the effects on the animal populations. It's possible to make some measurements on the aquatic birds, which were flying around apparently unharmed (Figure 30).

Figure 30. Bird flight over the reef at Eniwetok Atoll. From L. Donaldson.

This little fellow (Figure 31) forgot to take off when the rest of them did. Figure 32 is a closer look at him. He didn't fly very well and it was obvious that he had some particular problem. Figure 33 shows that he was pointed the right way, i.e., looking away from the

blast. However, his tail feathers were singed and some of the primary wing feathers were burned. He's in about the same shape, and has the same problem, as a ship without a rudder.

Figure 31. Injured bird on beach at Rigili Island, Eniwetok Atoll. From L. Donaldson.

Figure 32. Catching injured bird on beach, Rigili Island, Eniwetok Atoll. From L. Donaldson.

Figure 33. Noddy tern with burned wing and tail feathers, Rigili Island, Eniwetok Atoll. From L. Donaldson.

We returned the day after these photographs were taken. There were numbers of dead birds on the beach (Figure 34). The logical assumption, I suppose, that one would make is that these birds must have died from radiation damage. They were all right the day before, at least they were alive. Thus, we assume that they died from radiation damage. We took a look, examining them very closely. We tried to measure various things, do the best autopsy possible at the time, but we could find little or no radiation effects. So why did they die? This is the question we had to try to answer.

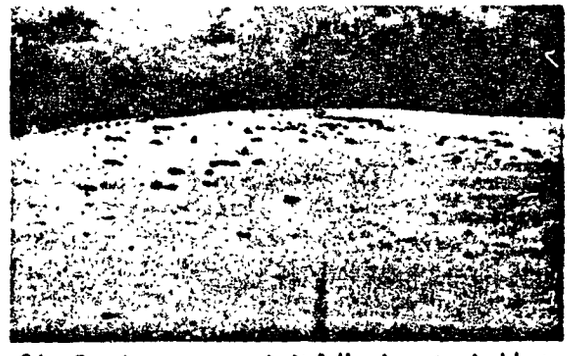

Figure 34. Dead terns on sandspit following atomic blasts, Rigili Island, Eniwetok Atoll. From L. Donaldson.

In our report we would just say they died of radiation, but we had to draw upon a little more background, the natural history of these animals. We have to realize that there's no water on the island for them to drink. If there's no water on the island they get their moisture from their food, their food being the fish in the sea. The salinity of the fish in the sea is the same as yours and mine, or about 75/100 of 1 percent. The birds are able to maintain their moisture balance if they can feed. If not, they may die from desiccation.

CONARD: Did those birds die in one day?

DONALDSON: Yes. These pictures were made on subsequent days.

CONARD: It seems as if it's a pretty quick death.

DONALDSON: But it's terribly hot.

FREMONT-SMITH: They dried out fast.

DUNHAM: Did you decide these died from desiccation or from thermal burns? I wasn't clear what your conclusion was.

DONALDSON: Desiccation, because the burns weren't serious enough to cause death.

UPTON: But the burns prevented them from feeding.

DONALDSON: Yes, it's the cause and effect relationship.

UPTON: They couldn't eat and therefore they couldn't maintain their food balance.

ROOT: This was obvious in the autopsy, too?

CONARD: Could this have been anorexia from radiation, loss of appetite, so that they didn't want to eat any fish?

BRUES: This is the old problem that plagues the pathologists and the epidemiologists. What is the cause of death?

DONALDSON: That's right.

FREMONT-SMITH: Multiple causality enters into it.

DONALDSON: Surely. Multiple causes that complicate this. Of course, the real differences that we have to come to grips with now involve the . . .

AYRES: May I interrupt for a second? Did you see any signs of birds whose tail feathers or wing feathers were lost later on because of beta burns?

DONALDSON: No, we did not.

CONARD: The feathers would protect the skin from beta burns.

AYRES: I'm just wondering whether the feathers themselves might have been burnt?

DONALDSON: The dead birds in the previous illustration had received thermal burns to the feathers extensive enough to prevent their flying and obtaining food and water.

TAYLOR: Didn't some of the birds, because of exposure to the thermal radiation, lose their ability to shed water so that they couldn't swim?

DONALDSON: Yes.

TAYLOR: Are these birds that normally would fish by landing in the water and then diving?

DONALDSON: They simply pick the fish off close to the surface, they don't dive.

The other major problem one might call attention to at this point is that we are dealing with two environments really. The birds live in both, but the other animals live essentially in either the terrestrial or the aquatic environment. The obvious situation that existed is that while there was a stratification of the fallout on the land, there was a three-dimensional distribution of radionuclides in the sea. In the terrestrial area the fallout radionuclides are available to the biota most specifically if they are in a soluble form. In this form they are picked up by the plants and enter the food chain of the animals that feed upon the plants.

Figure 35 is a partial illustration of the sort of fractionation that takes place in the land area. All of this spectrum of radionuclides, of course, is available on the land from fallout. This chart is for Rongelap Atoll which, in 1961, contained in its soil the radionuclides listed at the top of the figure. The plants picked up only four out of this complex and of those four the rats concentrated two: strontium^{90} and cesium^{137}. The fish, on the other hand, picked up manganese^{54}, cobalt^{60}, and zinc^{65}. We might add to this spectrum iron^{55} and cobalt ^{60}, which were the two dominant radionuclides in the open sea.

To make a sweeping statement of generality, the land animals and plants concentrated nuclides in the soluble form, while those in the sea concentrated nuclides in particulate form. Since the induced radionuclides of cobalt and iron are in particulate form, although finely divided, they enter through the food web more dominantly than do the soluble forms, which are distributed throughout the water.

Then we might comment on the competition that exists in the sea, which is completely different from that on land, for on land there are nutritional deficiencies, particularly in minerals, that, for the most part, do not occur in the sea.

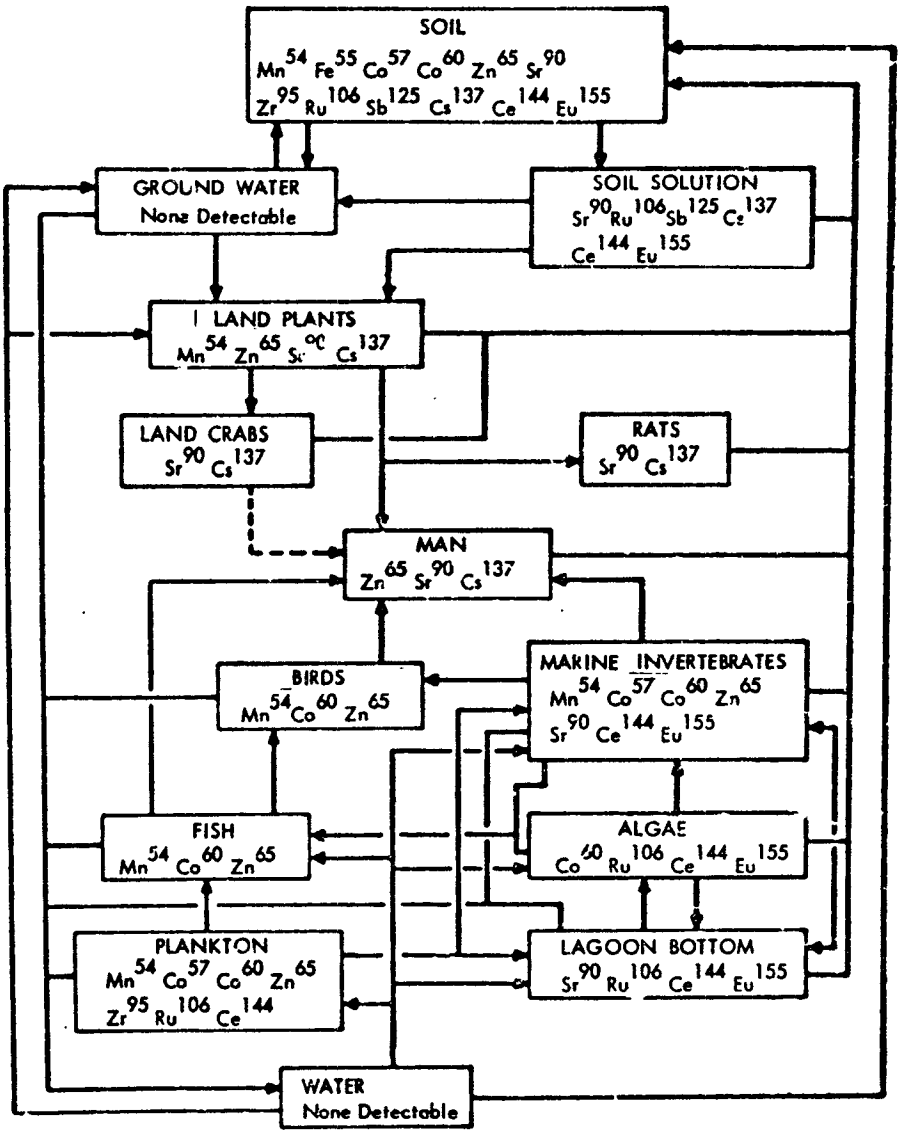

Figure 35. Distribution of fallout radioisotopes on Rongelap Atoll as determined by a survey in 1966. From L. Donaldson.

There could be a stray atom or two of cesium and strontium in the sea; we can't get excited at all about it, since it's not in the food chain. So, when we have this great nuclear war, I'm going to run out and catch myself a fish and eat it and feel quite sure that my food supply isn't in jeopardy.

In terrestrial areas there's very little chance for revegetation or regrowth if the soil is burned away and the seeds destroyed (Figure 36). The entire fauna and flora, one would assume, in this place could not be re-established.

Figure 37 is a photograph of an area where the soil has not been burned nor removed. You see a soil core where most of the organic material was in the upper inch. On the right is a radioautograph showing the concentration of the radionuclides in this material.

There is a different accumulation of radionuclides depending on the zone of feeding of the plants, as in the zone of feeding of the animals. Plants with shallow feeding roots (Figure 38) have a better chance to pick up the soluble forms and incorporate them in their tissues than do those plants that root deeper, like the coconut, for example. They feed deeper in the substratum and do not accumulate the radionuclides available to them in the soluble form.

The distribution in the sea (Figure 39) presents a constantly shifting pattern that changes with the seasons, with time, and, of course, with the currents. It changes from hour to hour or at least from day to day.

The radionuclides in the sea are incorporated in the lower strata first, since they are taken up by the small biota and then by other, larger organisms up the food chain. Many of these organisms are in the deeper layers during the day and migrate to the surface at night. Thus, there is a vertical diurnal migration as well as a constant shift, depending on the direction of the prevailing currents.

AYRES: Is that deep water?

DONALDSON: It's surface water.

AYRES: Diurnal irrigation doesn't normally extend into shallow water, does it?

Figure 36. View of islet near a test site, Eniwetok Atoll. From L. Donaldson.

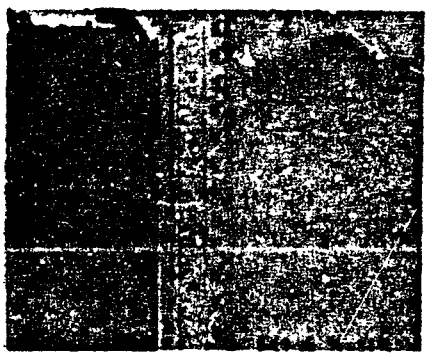

Figure 37. Photograph and radioautograph of soil showing correspondence of radioactive portions of organic matter. From L. Donaldson.

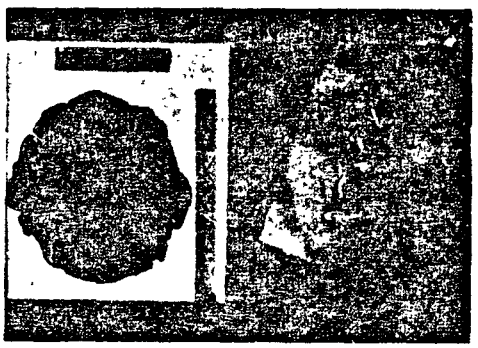

Figure 38. Photograph and radioautograph of Merrerschmidia stem with radioactivity in the outer, growing layers. From L. Donaldson.

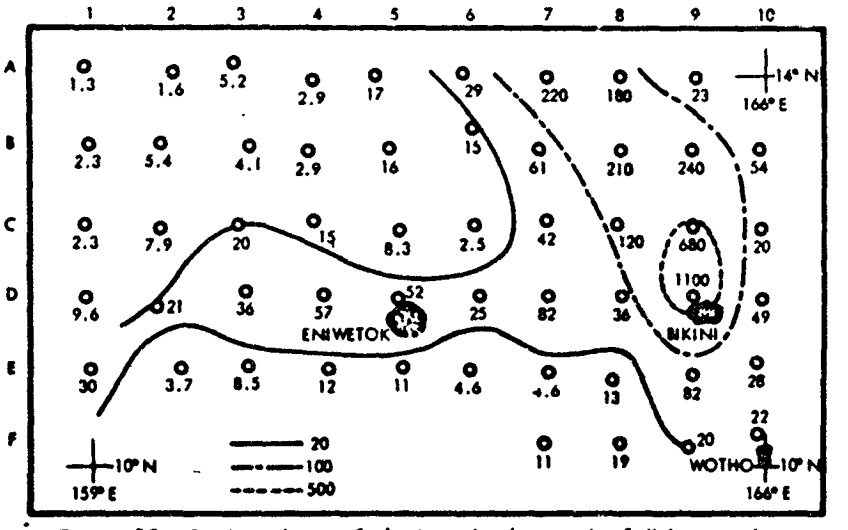

Figure 39. Radioactivity of plankton in thousands of disintegrations per minute per gram following atomic tests at Bikini and Eniwetok Atolls. From L. Donaldson.

DONALDSON: When I refer to shallow water I mean the water in the mixing layer, which, in this area, is about 600 feet. The deep water lies below this mixing layer.

DUNHAM: You don't mean shore water?

DONALDSON: No, I mean open ocean water.

WYCKOFF: What are the units for the numbers?

DONALDSON: They are contour lines of radioactivity in the plankton in thousands of disintegrations per minute per gram. These are the plankton that move up and down. They were collected through the entire mixing layer. This chart shows the distribution in the sea in the vicinity of the Bikini and Eniwetok test sites.

UPTON: How long after detonation were these measurements made?

DONALDSON: The plankton samples were collected June 11 to 21, 1956, during the testing period.

BUSTAD: But in spite of these high levels the only damage that was observed in the fish from the radionuclides was in the thyroid, wasn't it?

DONALDSON: Yes.

BUSTAD: Now, do you have any later results than that of Gorbman (Reference 38)? I think you lined him up to come out and study these and he did describe pretty serious thyroid damage in some of these fish.

DONALDSON: Yes.

BUSTAD: Have you run across any fish in later times in your collections that might have manifested thyroid neoplasms, say?

DONALDSON: I think so.

BUSTAD: Because the stage was set for it, sort of.

DONALDSON: Yes, the stage was set.

BUSTAD: Or couldn't they compete? Is that it?

DONALDSON: We have looked diligently over the years but we haven't actually seen or found fish that we could say were specifically killed by thyroid damage or damage from radioactivity to other organs or tissues. There's a complex situation here as far as the fish are concerned. No matter what the radiation levels are, no matter what the peripheral problems are, the "cleanup squad" moves in almost immediately and removes the incapacitated fish. This means that a fish that is just slightly weakened or disabled is removed within minutes, at least within an hour or so. Sharks move in and scavenge these places with great regularity. If it isn't the sharks, then it's some of the other predaceous forms. Thus, one's chance of actually finding or seeing a fish or an aquatic animal that has radiation damage would be very remote.

AYRES: Are there any top carnivora that might survive, like sharks themselves, even if they are somewhat damaged?

BUSTAD: The problem there, as far as radioiodine goes, is that they show the lowest concentration. They're not really getting very much radioiodine compared to herbivorous animals.

WARREN: Per body mass.

AYRES: You mean the concentration phenomenon doesn't extend right up to the top?

WARREN: What he's saying is that there isn't very much of the radioiodine ingested with any one fish because the thyroid is so small in terms of the body mass of the shark.

AYRES: So peak concentration is found in the lower forms then?

BUSTAD: That's right, and the radioiodine may destroy the thyroid or severely damage it and by then there will be no radioiodine left. It leaves the thyroid due to physical decay and biological turnover.

DONALDSON: There's another difference. The physiology of the shark is quite different from that of the bony fishes.

BUSTAD: But we have to admit, I think, that many of those fish that Gorbman picked up down there relatively early manifested severe thyroid damage but were probably not compromised from the standpoint of your cleanup squad. I mean he got there before they were appropriate subjects for the cleanup squad.

AYRES: Are there any turtles in the area?

DONALDSON: Turtles are not gregarious animals. They just don't like to have people around. They are there, true, but when the 4,000 or 5,000 members of the test group descend on the place, the turtles go elsewhere.

The turtles are back at Bikini now that the test group has departed. I hope we can—if the Chairman will allow us to—take a look at a film showing how the place looks now.

WARREN: I think we've left a little dangling in the discussion. You said the plankton have a diurnal change in depth, whatever their location. Does it occur in the atolls where the depths may be 200 or 250 feet or thereabout, as well as in the open ocean? The shallow waters you mentioned were meant to be the shallows, weren't they, at depths of 15, 20, 30 feet?

DONALDSON: In the lagoons of the atolls the plankton comes up on the shore at night. It's carried in the surface layers. The waves bring it up on the shore.

WARREN: And the circulation of the water in the atoll is downwind on the surface and when it reaches the other side then there's a return by the deeper currents, with considerable upwelling of cooler water on the upwind side. This is the deep circulation that you mentioned.

DONALDSON: In part.

WARREN: In part it leaks out into the ocean on the other side, too.

DUNHAM: How deep is an atoll?

DONALDSON: Most atolls are from 180 to 200 feet deep. In a living atoll, this seems to be about the growth limit. They grow into the wind, toward the east, since the prevailing winds are from the east.

CONARD: But you get a lot of coral heads, don't you?

DONALDSON: Yes, there are localized ones. But the coral heads are so spaced that they get food as it comes in with the currents.

ROOT: Was the species of algae you mentioned a heavy purple seaweed with a strong iodine taste?

DONALDSON: Yes. I'm hesitant because there are so many algae. If I recall correctly, there are some 170 species at Bikini alone. Many of them are various shades of purple and red.

ROOT: These would be the high iodine concentrators?

DONALDSON: Yes, within this whole group of algae some species accumulate radionuclides much more specifically than others, and because of this lack of uniformity of concentration, I think we must guard against making a definitive statement. We cannot say all algae do this, all fish do that, or that all corals and plants do such and such. It's self-defeating to do this sort of thing, because you lose the understanding that can be gained by examining all of the parameters.

AYRES: You have indicated that manganese and cobalt are both taken up preferentially in sea water, which would suggest surely that they are unduly scarce. Isn't that the implication you draw from that, that the requirements are greater than the supplies?

DONALDSON: Yes.

AYRES: And yet we have manganese and cobalt nodules forming somehow, which suggests a mystery.

DONALDSON: Yes.

WARREN: I think there's one thing you haven't touched on which ought to be put into the record. I think that you said, when you finished up at Bikini, that it was very fortunate that you had made prior studies because the sport rate or the genetic change going on in this population was much higher than had been suspected and it might have been blamed on the radiation later if it had not been found earlier. Is that still your concept, that normally the genetic change going on in these atolls is quite high?

DONALDSON: Again it's a relative sort of thing. The change in the biota may or may not be great. I think we have to go back to the flora, where we have definite, stationary organisms to study. I should like to refer this question to Dr. Wolfe. After all, he is the botanist-ecologist here.

WARREN: Well, I thought snails particularly were demonstrating this change.

DONALDSON: I don't know.

BRUES: Lauren, you were talking about the concentration of some of these elements in particular plants and this suggests that they are trace elements that are essentially cleaned out of the ocean by living things? We see this in fresh water. If you throw a little P^{32} into a pond, it all disappears into living matter. In fact, that's the major limiting factor, I suppose, in how much will grow. Does this happen in the ocean or is there plenty of all the elements to go around?

DONALDSON: I'm sure that there are plenty of elements in the ocean, but are they available? If you suddenly make many of the biologically essential ones available, they are blotted up, of course.

Maybe we can use the same illustration as that of the photograph of Shot Oak (Figure 27) we presented a while ago. In this case, there was a giant column from this one detonation over the north region at Eniwetok. The fallout from this shot came right across the northwestern edge of the atoll. We dubbed this one "the manure spreader" shot, for in making the reconnaissance sweeps over the atoll we saw a brilliant green band inside the rim of the atoll. You could fly from the clear blue waters of the lagoon over this green band, which persisted for several days, and the radiation instruments immediately would jump several orders of magnitude. It was obvious that the detonation had converted a good deal of the calcium carbonate, to take one element, into an oxide. The oxide had dropped in the waters to become hydroxide. Being soluble, it was picked up in an explosion of plant growth. There were other elements involved in this, too. In other words, nutritive media dropped in the sea had stimulated a very great growth of plants, in which one could find the concentrated radionuclides responsible for the jump in radiation levels.

Or, you can carry this still further. In the early days of planning at Hanford the cooling ponds were thermal coolers, as we originally designed them. These steaming vats had great masses of algae around the edge, and they still do. The algae in turn absorbed the radiation. At the time, there was very serious consideration given to a plan to simply collect the plant growths and put them in a disposal pit, since they blotted up the radiation very effectively.

EISENBUD: Lauren, iron-55 is an interesting nuclide. I wonder, have you looked for iron-55 in the fish over the atoll?

DONALDSON: Yes.

EISENBUD: Have you found evidence of concentration?

DONALDSON: Yes. In fact it was one of the most highly concentrated radionuclides.

EISENBUD: I would think so.

DUNHAM: My recollection from a visit to Bermuda a few years ago is that one of the marine scientists there said that iron availability in the waters around there was the limiting factor in perhaps the whole food chain inasmuch as one of the key algae couldn't go farther than the amount of iron available.

AYRES: You mean phosphorus was not the limiting factor?

DUNHAM: Iron.

EISENBUD: We found, in studies of our own staff in the laboratory, that some of our ladies who eat tuna fish a few times a week have blood levels of iron-55 that are about ten times higher than the rest of the staff. This led us to look at the Pacific tuna, which I think was done independently by the Hanford people, and they came to the same conclusion, that it was iron-55 from the fallout.

DONALDSON: Did you see cobalt?

EISENBUD: Cobalt-60? We haven't seen it. In fact, it isn't there. If it were there we would have seen it.

TAYLOR: Is there any persistent biological sign of the testing still at Bikini or Eniwetok in the aquatic part of the environment, either in the plants or in the fish? I mean, if you went out there now and didn't use radiation measuring instruments, but simply looked at the plants and the fish and the birds, would you expect to be able to tell that there had been this very intensive exposure of the area to radiation?

AYRES: Without radiochemical means?

TAYLOR: Without radiochemical means; just by examining the plants and animals?

DONALDSON: May I postpone my answer until we have a look at a film we produced during the 1964 survey of the islands?

TAYLOR: Yes.

DONALDSON: I think it will be more obvious when we look at the film, with the co-chairmen's and our host's permission, which I would like to show later on. The answer is . . .

TAYLOR: I guess the answer is yes.

DONALDSON: The answer is that you do not see it.

FREMONT-SMITH: The answer is no.

DONALDSON: You do not see evidence of it.

FREMONT-SMITH: I'm glad you gave the answer because the time to give an answer to a question is at the time it's asked and not postpone it, although it's nice to come back to it again later and say . . .

DONALDSON: Thank you.

WARREN: Well, on Miller island where the blast was . . .

DONALDSON: There's radiation, Dr. Warren.

TAYLOR: Yes. I was thinking specificially of aquatic life because you said in places where the surface has really been completely sterilized, there has been a change, I gather, in the surface life of the islands.

DONALDSON: Yes. There were very definite changes.

CONARD: There's some question as to whether some of the trees, the coconut trees, and the other plants on the northern islands of Rongelap, do not show some signs of genetic effects. There are some two-crowned coconut trees and this sort of thing, but it's questionable as to whether this is really a radiation effect or whether it's due to the aridity of that part of the atoll, and it hasn't been settled.

WOLFE: Someone has done a monograph on coconut palms.

CONARD: Fosberg?

WOLFE: No, Menon et al. (Reference 39). And this double crowning—he got a coconut tree in one place with 51 of these crowns and there hadn't been a detonation there. So this could come about maybe with a butcher knife by cutting off the terminal bud; I don't know. It might have been caused by radiation, but I don't think you can say so definitely.

CONARD: I agree.

WARREN: Weren't there some broad stems, flat stems, in Eniwetok?

DONALDSON: Yes.

WARREN: So that you were wondering about the neutron effects?

DONALDSON: Well, over the years we have recorded a number of variants from the normal, particularly among the plants. We don't know whether this is induced somatic variation or whether it's inherent. We don't see them now. They probably were unable to survive. Of course, we do see variations. At one time Dr. St. John (Reference 40) counted as many as 23 variants on one island. But these have not been reproducible in the laboratory.

WOLFE: In answer to that, that flattening of the stems; that's called fasciation. And that's not an uncommon thing. You can find it in all of the vascular plants if you look long enough; I've seen it mostly in the composites and it has nothing to do with radiation, although radiation might induce it.

WARREN: What is it due to, do you know?

WOLFE: It can result from insect bite or gall or sometimes there's no obvious answer. You can't attribute it to an insect; it may be due to some damage at the meristem, the growing tip where you don't get the radial development and it flattens out. I think this can be brought about. But it also occurs naturally.

WARREN: Would nutritional acceleration or acceleration from excess nutritional factors produce it?

WOLFE: I don't know.

WARREN: I've got a cucumber plant that's about 30 feet long and the stems show this and I wondered if they had been exposing the seeds to neutrons to produce the new variety. It's a lemon variety which is quite unusual.

FREMONT-SMITH: It was just exposed to you, Staff; that was it! [Laughter]

BRUES: That's the California climate!

WOLFE: I would not say radiation could not cause it but I would also point out that it could be caused by other things.

WARREN: Three inches wide and about a half-inch thick in a cucumber plant is quite large.

MILLER: Dr. Donaldson, what is the minimum study that would reveal in other organisms than man that the radiation had taken place? What is the minimum study that will reveal evidence of radiation exposure?

DONALDSON: I don't know the answer to that question.

CONARD: I was talking to a botanist and he thought it would be worthwhile to study some of the pollen from the coconut trees on some of the atolls, and he thought, I believe, by chromosomal aberrations that he could detect persisting radiation damage. I would think that this would be a fairly simple study that could be done.

MILLER: But it hasn't been.

CONARD: Maybe Schull might have something to add.

SCHULL: If memory serves me correctly, the Indians have examined the palm trees growing in the Chavara-Needakara area of Kerala to see whether they exhibited a higher frequency of chromosomal abnormalities, presumably ascribable to the higher levels of radiation encountered in this area. The results were somewhat ambiguous, I believe. There are, so far as we now know, no unique yardsticks of radiation damage of a genetic nature, and the crux of any study becomes the demonstration of a dose dependence. If there is no variability in doses, or the variability is so small as to preclude even ordering the exposures, it is difficult, indeed, to demonstrate a radiation effect.

There's a Japanese observation that I believe is relevant to the question that Dr. Taylor asked. In 1949 or 1950—probably 1950—Yamashita Kosuke, a geneticist at Kyoto University, undertook a fairly extensive study in Hiroshima of the distribution of abnormal forms of Veronica persica, a common garden flower. His data, published in 1956 (Reference 41), revealed a definite correlation between the frequency of aberrant forms of this plant and distance from ground zero. Atypical forms diminished with increasing distance from the hypocenter.

TAYLOR: Just looking at people's gardens?

SCHULL: Essentially that. In Japan, Veronica grows along the roadside in many areas, or did then. The aberrant forms Yamashita

counted are found all over, of course, but he was primarily concerned with their frequency as a function of distance from ground zero. I would suppose that a similar study could be made at Bikini but I wonder whether enough is known about the gradient in dose to permit one to make a fairly strong statement, either affirmatively or negatively, in the presence or absence of a cline of aberrant forms.

CONARD: You would have quite a gradient on Rongelap, 2,300 rads on the north island as compared to 265 on the southern islands. That's quite a gradient.

UPTON: But in point of fact no measurements of this kind have been made to date?

CONARD: So far as I know, they haven't.

EISENBUD: These are not high doses compared to what can be obtained in these areas of natural radioactivity. For example, in Brazil the ambient levels from external radiation are about 3 mr per hour downwards to normal levels, and this is about 12 r per year. So, in 100 years you have 1,200 rads. Presumably some of those forms have been there much longer. And then if you superimpose on that the dose from the internal, which is, incidentally, very hard to calculate because they are alpha-betas and the location and relation to the genetic material hasn't been worked out yet, the internal dose is presumably much higher, so that I think that there are probably situations in nature where this kind of a situation could be obtained if one wanted to.

TAYLOR: It just occurred to me that there's a mass of data sitting there at Rongelap waiting to be gathered and looked at.

FREMONT-SMITH: We'll have to plant some cosmos in there.

TAYLOR: No. Just observe what's there, as long as the dose levels are reasonably well known, but I'm not sure from the conversation whether or not they are really well known. Do people agree that the dose levels at Rongelap have been known within a factor of, say, one-and-a-half, in the total dose?

CONARD: I would think so; judging from the dose calculations and the hematological responses of the people, we're not too far off.

EISENBUD: One problem that's cropped up in Brazil which hasn't been solved and that might be pertinent here is the fact that it's hard to tell where these chromosomal aberrations come from. You take a sample of a plant and it's easy to calculate the somatic dose because presumably the plant has been there for its life. But what the dose is at the genotype of that plant is very hard to calculate because it goes back presumably many thousands of years, and maybe this plant came from a seed which was dropped by a bird two months ago and picked up ten miles away. I suppose to some extent this would be true in Rongelap where your coconuts tend to drift around. I don't know what the mean distance traversed by a cosmos pollen is, but this would even have to be considered in Hiroshima. In Hiroshima it certainly must be a large distance in relation to the radiation gradient in Hiroshima over a 10-year period.

WARREN: Looking at aerial photographs of this Brazil site, though, you don't see any change in the foliage when you come over the rolling country up to the edge of this.

EISENBUD: There are differences in the radioactivity partly due to the fact that there are also chemical changes associated with the mountains, which in turn give rise to the fact that it's radioactive. These chemical changes presumably are important. This is another factor that has to be considered.

WARREN: Is that a volcanic cone or this . . .

EISENBUD: It's a volcanic cone with an alkaline intrusion in the center. The alkaline intrusion is where the main radioactivity is about a couple of kilometers across, about 300 meters high, above. . .

WOLFE: Is it active?

EISENBUD: It was many, many thousands of years ago but not in historic times. This was a major volcanic eruption. The cone is about 50 kilometers in diameter. Within the center of it is an alkaline intrusion which is just a knob which brought up a lot of rare earth minerals associated with thorium. This is a few kilometers across and is where the work is going on.

WOLFE: I haven't seen it.

WARREN: I've only read about it. You don't run sheep on this because there's no grass or not enough foliage?

EISENBUD: No, that's not so. In fact, the cows graze on it; it's part of a grazing land, and there's enough grass on it.

WARREN: Very interesting.

DONALDSON: We've purposely omitted one of the prime areas of interest in the overall environment, and Bob has some data which we might bring in now, with your permission, on the whole-body burden of the Rongelap people.

CONARD: In yesterday's session I mentioned that after about six months, the urinary excretion of radionuclides of the Rongelap people dropped down to barely detectable levels, and by the time they were moving back to Rongelap we couldn't tell the difference between the body burden of unexposed people and exposed people. As soon as they got back to Rongelap, however, there was a rather sudden and marked increase in their body burden because of the residual contamination on the island. This came about primarily through eating pandanus and to a lesser extent from the coconut, and strangely enough the zinc-65 came from eating fish. As Lauren pointed out, fish was one of the mainstays in their diet. We were later able to get a whole body counter out to Rongelap to measure their body burdens. The first one was a big monster that weighed about 21 tons, and it was a real endeavor to get that thing out on Rongelap Island. We have recently been using a "shadow-shield" type of whole body counter consisting of lead bricks. Figure 40 is a photograph of one.

Figure 40. Shadow-shield whole-body counter (gamma spectroscopy). From R. Conard.

UPTON: Were the fish levels higher in the Rongelap area than in the area to which the natives had been evacuated?

DONALDSON: Yes. There was no fallout down at Majuro.

CONARD: They were in a relatively clean area. They were down at Majuro, 400 miles to the south.

UPTON: The fish then continued to be more active in the Rongelap area over the passage of years?

CONARD: Right. That was a 3-year period up until their return.

UPTON: Yes.

CONARD: And the fish were still quite active with zinc-65.

UPTON: These are marine fish?

CONARD: Yes.

DONALDSON: There's no fresh water.

UPTON: The lagoon is a marine lagoon?

CONARD: Yes. It's salt water.

FREMONT-SMITH: These are fish that stay in the lagoon. They were not going in and out of the ocean?

DONALDSON: Both populations exist there. The people eat the fish that live in the lagoon and also tuna fish and other open-ocean types of fish.

FREMONT-SMITH: Which were the ones that were primarily responsible for the increased body burden, do you know?

CONARD: I really don't. They ate all kinds of fish.

FREMONT-SMITH: I mean do you presume that the ocean fish in that area still carried the heavy. . .

CONARD: Lauren, it was probably lagoon fish, wasn't it?

DONALDSON: The ocean fish are essentially carnivores, and the lagoon fish are herbivores. You immediately fractionate on this basis alone, that is, the food chain is different. As you move up the food chain to the carnivores the radioactivity is less.

FREMONT-SMITH: So it was the herbivore that was responsible.

DONALDSON: The herbivores are obviously the best concentrators.

CONARD: Figure 41 is a spectrograph of what you get from the whole-body count, showing the comparison of the count in 1957 before, and in 1959 after, moving back to Rongelap. In March 1958, shortly after they had come back, they showed an increase, the first peak being cesium-137 and the second peak zinc-65. We carried out these whole-body counts over the years since they've been back on the island and I can now review very briefly what's happened in the way of the body burden of these isotopes.

Figure 42 is a histogram which shows the changes over the years. The first 1954 data there show the higher levels, of course, connected with the initial contamination, and then up until 1957 their body burdens

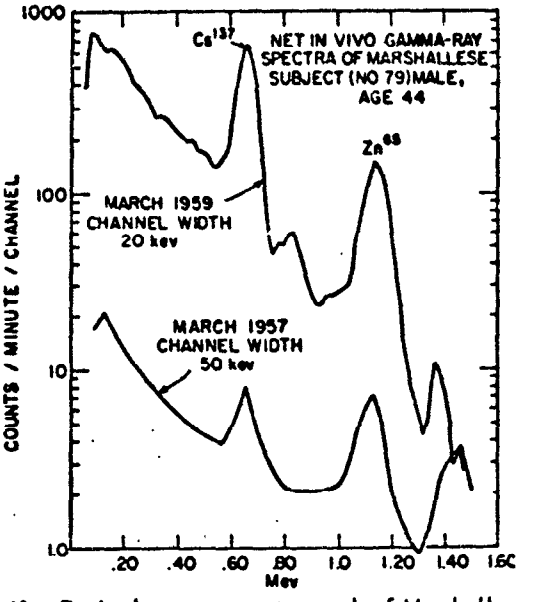

Figure 41. Typical gamma spectrograph of Marshallese subject before (March 1957) and after (March 1959) moving back to Rongelap Island. From R. Conard.

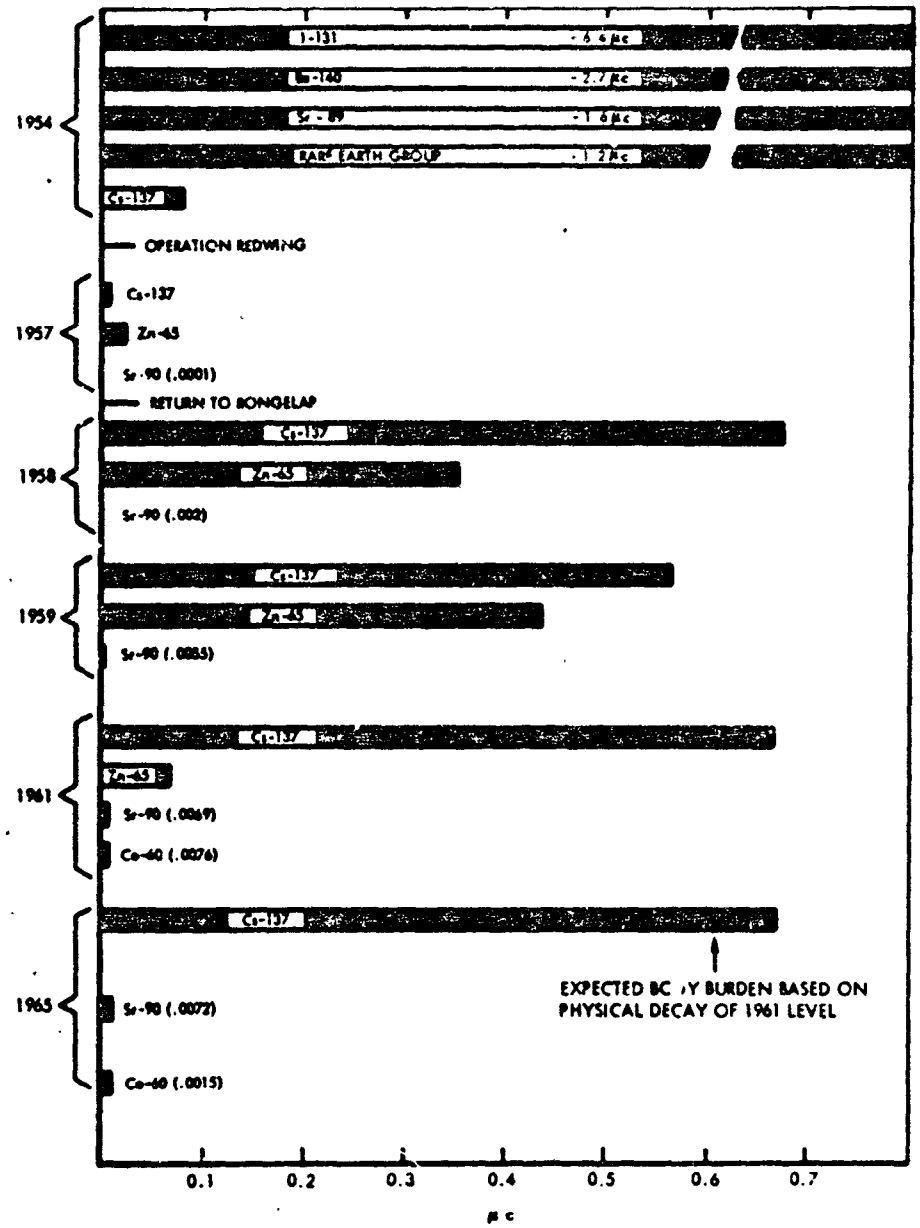

Figure 42. Estimated body burdens of isotopes in Rongelap people at various times since 1954. Values obtained either by gamma spectroscopy or by radiochemical urinanalyses. From R. Conard.

reduced practically to zero. You see on their return to Rongelap, the increase in cesium and zinc. Strontium-90 also began to appear, and this had to be detected not by whole-body counting but by radiochemical analyses of the urine.

The levels reached a peak about 1961 or so and during the past year a drop in urinary excretion has occurred, indicating lowering of body burdens. Cesium peaked at a little less than 1 microcurie of body burden, which is not high, but it represents about 300 times the level in those of us in the medical team that were counted.

In regard to zinc, it reached a peak at about the same time that the cesium did but suddenly within one year's time it dropped to about 1/10 of the previous year's value. I wonder, Lauren, do you have any comment on that, as to why we had this sudden drop in zinc-65 in the people? Was something happening to the fish then that caused this sudden change?

DONALDSON: When did they get rice?

CONARD: They had been eating rice pretty much all along.

DONALDSON: Their food habits can change radically.

DUNHAM: There wasn't a difference in your counter at that point?

FREMONT-SMITH: Don't suggest that!

EISENBUD: What is the half-life of zinc-65? I should know, but I don't remember.

DONALDSON: Less than a year. Two hundred and fifty days, I believe.

CONARD: But that wouldn't account for a sudden change?

DONALDSON: If I remember the data correctly—and I would have to look them up. I have them here. Essentially there has been no drastic change in the usually expected declines. If they have changed their habits not only in eating fish but also in eating birds, and if they've had expeditions to the north island and come back with lots of birds, that would increase it.

FREMONT-SMITH: Did you do any cultures of white cells on these people?

CONARD: Yes, at 10 years we did about 40 cultures for chromosomal studies.

FREMONT-SMITH: Did they show anything out of the usual?

CONARD: They showed persisting aberrations, low levels of aberration.

FREMONT-SMITH: More than other people would have?

CONARD: The exposed people showed a greater incidence of these aberrations than did the unexposed.

EISENBUD: I think it should be emphasized that those doses that you show in Figure 42, when translated into dose units, are just a couple of hundred milligrams.

CONARD: I was going to get around to that in a minute.

EISENBUD: Sorry, I didn't mean to anticipate.

CONARD: Another isotope that was found was cobalt-60, to a lesser extent, which is about 1/10 the zinc level. We haven't seen any iron-55 in the people but we haven't done any specific studies.

EISENBUD: Any what? Have you looked for it?

CONARD: Not specifically, no, but we haven't had whole-body counts now in a couple of years.

EISENBUD: You can't do it with whole-body counting. It decays by internal conversion and gives you an electron . . .

CONARD: Maybe we'll pick it up in the urine.

EISENBUD: No. Sample blood. Maybe you have some in your laboratory. What you do is separate out the iron-55 and look at it with a thin crystal. It should be very interesting in that group to see what the iron-55 level is. Iron-55 is an interesting isotope. It has been neglected until now because the emission is a 6 KeV electron which has a range of only one micron in tissue; it has generally been

ignored. But iron goes to very small volumes of tissue. Specifically, it tends to concentrate in little globules and you get a very high dose there because essentially all of the range of the iron-55 electron is comparable with the diameter of the globule.

MILLET: May I ask if the unexposed population showed chromosomal changes, too?

CONARD: They showed some peculiar chromosomal changes that we haven't been able to understand yet, chromosomal breakages. They show about as many breakages of chromosomes as do the exposed people. But I was referring to the more specific radiation-induced types of aberrations, such as dicentrics and ring forms that occurred.

AYRES: May I ask about the zinc? How is that taken up and where is it stored in the body? Is it taken up as zinc or is it a surrogate for something else?

CONARD: I really don't know. I know it gets into the body and is fairly well distributed, as I recall.

LANGHAM: It's concentrated in the epithelial tissues. The hair is very high, the skin is high.

CONARD: The prostate I believe is fairly high.

LANGHAM: The prostate and pancreas. Percentage wise, the greatest amount is in the skin and hair.

BRUES: It looked to me as if the cesium levels were remaining rather constant in these people. I think that's remarkable. It turns over with a half-time of three months or so in man. So they must be in essentially a closed environment without cesium drifting or blowing out of it.

CONARD: That's so. And I think, as Lauren pointed out, the fact that this material is sticking in the upper layer of the soil and not being dispersed, being diluted in soil, so to speak, means that for a long time we'll have levels that can be detectable.*

*Since this symposium, results on the latest radiochemical urine analyses (March 1967 urines) reveal that the excretion of these radionuclides

WARREN: It's interesting that the tropical rains don't leach it downwards. The tropical rains should produce quite a bit of water to leach this down into the soil. Is it complexed or fixed?

DONALDSON: It doesn't leach to any degree. It stays pretty well fixed.

WOLFE: It's accumulated in the soil algae in that upper layer, isn't it? That is what the radioautograph shows.

DONALDSON: Yes.

WOLFE: And the algae are only in about the upper couple of inches. Below that it's apparently too dark.

TAYLOR: Is there any animal study that correlates with the observation of malformations of human children that were in the fetal state two or three months or so when the irradiation took place? Is there any animal counterpart of that that's been seen in any of the bomb test irradiation?

MILLER: Not in the wild state, but in laboratory animals certainly.

TAYLOR: How about fish, for example? When the fish are irradiated when they are developing eggs, do the eggs lose their fertility like that?

DONALDSON: You can go the whole gamut. If you take chronic exposure over long periods of time and pick a level, say a half roentgen per day for a 90-day total exposure of 45 roentgens, for example, and follow the fish through several generations, instead of getting a damaging effect you find a stimulating effect. Double the dose, and the same sort of things happens. Or double it again and I'll give you the answer in part tomorrow. I'll be a midwife tomorrow while you're enjoying yourselves here. We'll have several hundred fish coming back from the sea that were exposed to 2.5 r from a cobalt source as eggs.

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*(Cont'd) has dropped to about one-half that of the previous rate of excretion. This may be due to the fact that the people are buying and eating more commercial food.

FREMONT-SMITH: These are salmon?

DONALDSON: Yes.

FREMONT-SMITH: I thought there might be somebody that would know it.

DONALDSON: In these experiments the exposure starts the instant the eggs are fertilized and continues until the yolk is absorbed and the fish are free-swimming, a period of 90 to 100 days.

FREMONT-SMITH: They are bigger and better as a result of the radiation?

DONALDSON: Yes.

DUNHAM: Are they all or are they selected? You still are losing 90 or 99 percent of them.

DONALDSON: Yes. Survival of the irradiated fish is better than that of the controls.

DUNHAM: It's the ones that come back that are bigger and better.

FREMONT-SMITH: Do you lose 90 or 99 percent? Is that right?

DONALDSON: Actually the normal expected mortality of salmon in the sea is in excess of 90 percent. We release an equal number of controls and irradiated fish. For fish exposed to 0.5 r or 1.3 r as alevins, the survival, rate of growth, number and hatch of eggs produced are equal or superior in the irradiated group.

FREMONT-SMITH: So that is really as if you had benefited the fish by radiation.

DONALDSON: Yes.

UPTON: How about the hatchery?

DONALDSON: Better, though barely significant.

EISENBUD: Do the salmon say the university is always there? [Laughter]

DONALDSON: They don't make mistakes! I wish I had students as smart as those fish.

WARREN: I think that this point which Lauren has found is of great significance in this whole story of radiation exposure and yet it's been sort of ignored.

FREMONT-SMITH: It's against the dogma. And not just ignored. It's submerged, it's suppressed.

WARREN: I've examined this with great interest for years since he fir.. had this finding.

DONALDSON: Let's get the record straight. I'm still under . . .

WARREN: He's still exploring.

DONALDSON: . . .under the initial directive that I received from Dr. Warren back in 1943, during the days of the Manhattan Engineering Project. The experiment must be carried out over many years and it must be done in the complete environment, not just in the laboratory. In other words, the fish must be exposed during the time that they would be, say, living in the effluent of the Hanford Works, and we must be able to get our hands on them again when they return from the sea in order to evaluate the effects, if any, from their exposure to radiation. In other words, we must not simply say, because they didn't die in the first 90 days or 20 days or the first year, that there was no effect. I naively told Dr. Warren, let's see, 24 years ago, that yes, we could do this experiment. I didn't realize that it would take me 24 years to get the facilities and develop a salmon run that would return to the University campus. Each exposure level takes a minimum of five years to evaluate

FREMONT-SMITH: You're going to telephone tomorrow afternoon and tell us what the answer is?

DONALDSON: One step toward the answer.

WARREN: I think this is very significant and I think a great deal of credit is owed to the AEC Division of Medicine and Biology for continuing to support this work over the years, 20-odd years, with such a small yield in return for a few percentage of fish. This has been maintained over the years and you're now in what, 26 generations?

This ought to be of interest to the geneticists here. Some of the original exposures were in 1943 or 1944.

DONALDSON: Those are the experiments with rainbow trout which started in 1943.

WARREN: But here has been the longest, to my knowledge, single set of observations on one or more species of fish that have been exposed to relatively small amounts of radiation. I think this ought to be continued as long as necessary to get the final answers. I agree with Lauren. He's got some initial answers which look very spectacular and interesting and he's properly modest in not claiming too much too early. But I think this is as important as following the Nagasaki situation, where the dosages are not so well controlled.

FREMONT-SMITH: It would seem to me that nature has taken advantage of all of the physical properties of nature and used them to an advantage. On the other hand, it has been sort of assumed that radiation was always bad and that any radiation was going to be harmful. Now it seems to me there is some evidence to believe that there was a higher radiation in the past than there is today and that it's entirely possible that there is an optimum radiation for some species, or maybe for many species, and that we shouldn't assume that every radiation is bad. It seems to me that Lauren's temporary answer supports this position, that it may be that salmon, maybe other fish, and maybe other species are benefited by an appropriate radiation. I know this statement is contrary to official position, but I'm contrary to official position.

WARREN: I've been looking into this, as you know, with some interest of late and I'm not willing to say that radiation is universally harmful because we have a continuous background of naturally occurring radiation and cosmic radiation. The former could have been considerably higher in the past, but I don't think I'm in any position to go any further in that discussion. But I point to Lauren's experiment as being significant in this direction.

BRUES: There's recent evidence that the earth's magnetic field flops over every so often, letting in meteoric material and cosmic radiation (References 42 and 43). I believe the last time that this is supposed to have occurred coincides more or less with the time when man first appeared on earth. That is rather speculative, of course.

DONALDSON: I cringed just a little bit, Dr. Warren, when you said small in numbers, because I've made the grandiose statement that this is probably the biggest radiation experiment, numerically speaking, that's been carried out with vertebrate animals. We normally use in excess of 100,000 exposed and 100,000 controls, making 200,000 animals in each experiment. Then we have to carry another population along, so we always have reserve stocks.

WARREN: But the salmon gives a small percentage of return, as you indicated.

DONALDSON: Yes. Even if we get only a 1 to 3 percent return, we have somewhere between 2,000 and 6,000 salmon coming back to the University pond, which is just slightly larger than this room. When you have that great a number of adult salmon—the average weight last fall was 18.6 pounds—coming to a small place like this in a two-week interval, you have a tremendous mass of experimental material. So your statistical problems are astronomical. This return would produce at least 5 million offspring each year. To evaluate 5 million offspring, follow them step by step all through their incubation period, determine the number of anomalies, the rates of growth, individual variations between some 1,000 to 1,200 lots, you need more than a computer; you need a group of trained monkey technicians.

FREMONT-SMITH: How large a staff do they provide for you to help with this?

DONALDSON: That was the question a group of Russian geneticists asked me last week.

FREMONT-SMITH: I'm asking it now.

DONALDSON: Ask John.

FREMONT-SMITH: Let's get it on the record. How large a staff? They've been supporting it for 24 years, but how large a staff do you have?

WOLFE: It depends upon the season of the year. When those fish are coming back, he's got 25 or 30 students and assistants out there catching them out of the pond and going through all these ablutions that Lauren has described. During the off-season I don't know how many people there are.

FREMONT-SMITH: What I'm trying to bring out is, does he have enough staff to do the job?

WOLFE: Nobody ever has enough staff.

FREMONT-SMITH: Okay. I just wanted to bring it out. He hasn't got enough staff.

DONALDSON: This is one of the tricks one learns being a school teacher, Dr. Fremont-Smith. The fish usually come between August and September. The return runs usually begin around the 25th day of September. This year it was the 26th, but it's close enough. Then I have the 25 or 30 students who can help me.

WARREN: He orders the fish to return on that date! [Laughter]

FREMONT-SMITH: I think you ought to go to the University to start on that.

DONALDSON: Young salmon normally go to the sea during July, maybe as late as August, but that's inconvenient because school lets out in June. We speed up their growth and get them out into the bay the first day of May on their way to the sea. Then the students have time to prepare for their examinations and everything goes along nicely.

WARREN: It was very cute of him to turn nature to his time schedule.

FREMONT-SMITH: Forgive my remarks. I just wanted to get it on the record that maybe he could have a little more help.

EISENBUD: What is the radiation pattern? I don't know if you gave that. If you did give it, I missed it. What dose do you give them over what period of time?

DONALDSON: We have increased the dose year by year. We started out at 0.5 r per day, went to 1.3 r per day, then to 2.5, and this year we're going up to 5 r per day.

EISENBUD: For how many days does this go on?

DONALDSON: Approximately 90 to 100 days, during the entire incubation period. This is one advantage of this sort of experiment.

You have a built-in food supply and you can put the eggs in a chamber and expose them to a cobalt-60 source—expose them for 90 to 100 days. At the end of the exposure period they are ready to start to feed; then you can take them out to the troughs. During the 90 to 100 days they have gone through their entire embryological development. They're fully formed little fish.

WARREN: Lauren ought to also tell you that he has been studying all of the abnormalities that can be produced in these fish with irradiation and that there's a certain mortality from this, depending upon the dose rate. You get all of the abnormalities that have been ascribed to this in other species and the large lethals are included in this list. But at this dose rate your abnormalities and your lethal effects are pretty low, aren't they?

DONALDSON: There's no significant difference in the number of anomalies between the irradiated fish and the controls at the levels we have used so far.

FREMONT-SMITH: No increase?

DONALDSON: No significant increase.

TAYLOR: What is the LD-50 dose for a salmon?

DONALDSON: An acute dose is between 450 and 500 r.

TAYLOR: You're giving them about 500 r, aren't you?

DONALDSON: Chronic exposure.

WARREN: Daily.

DONALDSON: We'll give about 400 to 500 r this year, spread over a 90- to 100-day period.

AYRES: That's a time when cell reproduction is rather rapid, though.

DONALDSON: That's right.

WARREN: At their maximum rate of growth and change. Presumably this should be the most sensitive period, shouldn't it?

FREMONT-SMITH: The most vulnerable period.

AYRES: On the other hand, recovery can be more rapid.

TAYLOR: Why don't they all die, is what I'm asking.

LANGHAM: It's the dose rate. There's a lot of difference in giving a dose in five minutes and over a hundred days.

TAYLOR: Is it a factor of 2?

DUNHAM: Your monkeys all had lethal doses, as you showed yesterday.

LANGHAM: Yes, if given in an hour or so. The prompt lethal dose of the monkey is about 550 r.

FREMONT-SMITH: Please, gentlemen, don't have a private conversation because it makes it impossible.

UPTON: I think a similar experiment has been performed on the mouse. I think I recall that Russell and associates (Reference 44) could detect defects in mice exposed to dose levels of 25 r in the embryonic period whereas if they administered something like 13 r per day, continuously given over a 24-hour period throughout embryogenesis, they observed no effects, due presumably to the lower dose rate.

DONALDSON: The most unusual aspect of this research is that the fish are tested over the entire life cycle. A major portion of their life span, of course, is spent in the highly competitive environment of the open sea. After the exposure period during incubation, the young fish are fed for about three months, until they have grown to migrating size and have developed the ability to change from fresh to salt water. Then they are released to go to sea. After two or three years of active feeding in the sea they must be able to retrace their migratory pattern and "home" to the small pond on the University campus.

There are surely many response patterns involved in the complex life cycle of the salmon. The final pinpointing of the "home stream" is now fairly well documented as a memorized olfactory response.

FREMONT-SMITH: Do you want to tell us briefly that fascinating story about the olfaction and how they find their way?

DONALDSON: This is the work of Dr. Gorbman and associates. Dr. Gorbman is the chap who worked on the iodine uptake in the fish at Bikini (Reference 38). He has been doing memory pattern responses by taking the return salmon and immobilizing them, lifting the skull case off, putting probes in the olfactory lobes and then dropping water on the olfactory nares. The water may be from the "home stream," which gives a positive response, or a foreign stream, with a negative response. Water samples collected from down river along the migratory path bring a positive response.

FREMONT-SMITH: Down the river.

DONALDSON: Yes, samples of water from down the river or up the river or some other place, or even tap water, and recording their memory response for this particular environmental stimulus.

FREMONT-SMITH: The electrical activity to the environmental water.

DONALDSON: Yes.

AYRES: Is it an encephalogram technique?

DONALDSON: Yes.

FREMONT-SMITH: What happens?

DONALDSON: The olfactory nares are sensitive to infinitesimally small amounts of "home" water. Dilutions of the "home" water continue to give positive responses. If, on the other hand, samples of water are obtained from 100 yards on up the watershed where the salmon haven't been, there's no response.

TAYLOR: What happens if he takes them out of the water and gives them upstream water and downstream water and some mixed stream water?

DONALDSON: This can be done.

FREMONT-SMITH: It makes them very angry!

DONALDSON: May we come back to the subject at hand for the moment? Before leaving this environmental area that we've been talking about in the mid-Pacific, I think it's germane that we include a word or two about the change in our relationship with Japan since 1954 and how these environmental problems were handled on a different basis.

In the 1958 series, we obtained permission from the Division of Biology and Medicine, Dr. Wolfe and Dr. Dunham, to do a sort of undercover operation. This operation involved one of our good friends in Japan, one of the leaders in the Shunkotsu Maru expedition, which caused so much of a problem in 1954. This chap agreed to collect and evaluate samples of tuna fish that were caught by the Japanese fleet. He collected some 2,000 samples, sent us half of the samples, and kept half. We made our evaluations, they made theirs and we compared them. However, he couldn't get his data published in Japan, but that didn't necessarily matter; they were available for the scientific record. Since they were not the sort of exciting things that would make a good news story, they are not a part of the popular record.

In 1962, during the high-altitude tests at Christmas Island, this program was again repeated and Dr. Toshiharu Kawabata* again collected the samples and sent half of them to us. Under some very real pressures on the part of the hysteria-minded group in Japan, an expedition, made up of a group of reliable Japanese scientists, was sent to evaluate the radiation hazard. The ship was equipped and sent out and we were asked to meet it in Honolulu in June of 1962. We had long conversations with these scientists as to what we had found in the Pacific and, most important I think, for this record at least, we more or less "held their hand" during this operation, because, frankly, they did not expect to return home. They were perfectly willing to give their lives to the cause, many of them.

FREMONT-SMITH: They expected to be killed by the blast?

DONALDSON: They expected to be, at the very minimum, greatly affected by radioactive fallout. It seems fantastic again or incredible, but they had the most elaborate air-conditioning system I've ever seen. Every porthole was plugged. They had long filters installed.

*Kawabata, Toshiharu. Department of Food Research, National Institute of Health, Tokyo.

The ship was equipped so that it could be operated entirely without anyone being on deck, with almost a periscopic peephole. They wanted assurance from us that they could go into the area and possibly survive. But what would be the best way to proceed?

In all of our discussions with the Japanese we were very frank and tried to be as helpful as possible. We reviewed our program, showed them the areas we had worked in and the levels of radiation we had encountered. We assured them we were perfectly healthy and were returning to the test site on Christmas Island to continue the program. I think it was a great relief to them to have a chance to talk with us —and to see that our health was good.

Well, the ship left Honolulu; they made their stations, they went home and we arranged a second meeting, in Tokyo, again through the Division of Biology and Medicine. The Commission sent Dr. Gordun Dunning* over to chair the meetings, where we brought all the data together, their data and ours. We did a correlation study eventually and found that we had essentially the same results—fallout could be detected but at very low levels.

FREMONT-SMITH: Were they awfully surprised to come back alive?

DONALDSON: They were tremendously pleased, I guess, to be alive.

DUNHAM: You said they were very sophisticated, knowledgeable scientists.

DONALDSON: They were sophisticated, knowledgeable scientists. They also had fishermen on board who had been exposed to the popular Japanese beliefs of radiation damage. But the precautions that they had taken and the facilities they had were completely out of keeping with anything we had available to us or had ever actually seen.

EISENBUD. How close in did they go?

DONALDSON: The exclusion area was 200 miles. So they were that close at least.

*Dunning, Gordon. Technical Advisor, Division of Operational Safety, U.S. Atomic Energy Commission.

EISENBUD: The Shunkotsu Maru came that close in 1954 and they didn't seem to be too concerned about it. You may have noticed in the Saturday Evening Post picture that shows me on the deck of the ship, that I was the only one that didn't have a mask; the Japanese accused me of being a little too cavalier about radioactivity. They thought I really ought to take care of myself.

DUNHAM: You were grandstanding! [Laughter]

EISENBUD: There was nothing I could do about it. I didn't bring any along and they didn't have any for me.

DONALDSON: I'm about at the punch line of my story, I hope. At the conclusion of the meetings there was to be a press announcement and the place was swarming with newspaper people. They had television cameras, newspaper photographers all around us; the place just buzzed. The prepared statement, which was handed to the newspaper people, stated that we were in complete agreement and that the levels of radiation were very low—or non-detectable. You should have seen the expressions on these men's faces. "But these are not great amounts," they said. "No. These are the findings of the joint report." When we searched the papers the next day we could find only about an inch and a half of print devoted to this news, and I don't think any of the footage was used on television.

CONARD: I had one final statement I wanted to make. In regard to the Rongelap body burden situation, it turns out that none of these isotopes exceeded 5 to 10 percent of the MPC (maximum permissible concentration) in the people. The children had slightly higher values for the strontium-90, to 20 percent of the MPC in some cases. It was estimated that the total body dose from all of these internally desposited isotopes only amounted to several hundred milliroentgens per year, and, as you know, our MPC levels are based on peacetime limits and are very conservative, with a safety factor of about 10 which is usually cranked in. So, in the aftermath of a nuclear war it would seem to me that this Marshallese experience does tend to indicate that after the first week or so one can live in a contaminated area without too much radiation hazard.

FREMONT-SMITH: With that degree of contamination.

CONARD: Yes. But even extrapolating back to larger amounts, judging by the smaller dosage they received, it would seem that it would be a minimal hazard.

ROOT: You mean if you hadn't moved them off at all it would have been a minimal hazard?

CONARD: I would say that it probably would. I don't think that I want to stick my neck out that far because I really haven't calculated what the total dose would be if they had remained on the Island continuously, but certainly it's not anywhere near the range of the acute immediate hazard.

ROOT: That's a good shelter hypothesis—if you can get them all under shelter during the actual fallout, they could emerge the next day perhaps without danger?

CONARD: I wouldn't say the next day.

AYRES: That's a standard Civil Defense notion that if you shelter for a couple of weeks, during that time the activity drops by a factor of 100 and then you're probably all right.

CONARD: Most of the radioiodine by that time has decayed.

EISENBUD: Things had quieted down in the summer of 1954 and the then, I guess we forgot to mention yesterday, the Russians started a test series in September and the fallout levels to Japan were actually heavier in September than they had been during the period when we were testing the previous spring. But things had quieted down, anyway, which led many of us to believe that the commotion in Japan in that time was at least in part motivated by Communist propagandists.

Well, one of the things that happened in the early fall, particularly I think motivated in part by the Russian test, was that the Japanese decided that they didn't get the most out of the visits that some of us had made the previous spring. They wanted to have a radiobiology conference and they invited the Atomic Energy Commission to send a group over. About a dozen of us went over in November and sat with our counterparts in Japan and had two weeks of very worthwhile discussion with them.

Interestingly, and apropos of the remarks I made yesterday about the schism in Japanese medicine, there were no Japanese physicians in their delegation and we were discreetly asked not to include any in ours so that they wouldn't have to pick or choose between Tsuzuki and his opponents. So the conference included geneticists, physicists,

and biologists of various kinds but we never did get to see the physicians. This is very interesting.

But, out of that conference we saw some Japanese data, from their Shunkotsu Maru expedition, which I think was right in the middle of the test, wasn't it, Lauren?

DONALDSON: Yes.

EISENBUD: Do you remember the date of the Shunkotsu Maru expedition?

DONALDSON: They left May 24th.

EISENBUD: They sailed into the equatorial current just west of Bikini and took profile measurements which indicated that about 200,000 curies a day were drifting out of the lagoon into the equatorial current. This is while the other tests were going on. This information was given to me in a little packet. It wasn't discussed very much. I read it on the way back and I got interested in it. As a result of that and the fact that it was a simple extrapolation to show that this would go into the Kuroshiro Current in the Philippines and then head north to the Japanese coast, it seemed prudent to get out and get some measurements. This was done through an operation control which was carried on jointly between the Coast Guard and Dr. Donaldson's laboratory and ours, and that took place, I believe, in March, about a year after the 1954 event.

DONALDSON: This expedition left on February 25, 1955.

EISENBUD: And got some very good data on the distribution of radioactivity in the Western Pacific as a result of that test.

FREMONT-SMITH: Was it appreciable?

EISENBUD: Yes. The radioactivity was detected everywhere that the expedition went. It started essentially from the Marshall Islands and proceeded west to Guam and then north in the Kuroshiro Current to Japan, where they put in and exchanged data with the Japanese. Then, as I recall, Lauren you correct me—I'm just reconstructing this—they came back in the Alaska Current and went down the West Coast of the United States and completed a cruise of some three and a half or four months during which time they actually followed the current all the way around.

FREMONT-SMITH: Were the fish getting this and accumulating it?

EISENBUD: Yes, but very, very small amounts. We said it was high enough to be interesting but low enough so that we didn't really have to worry about it.

DOBSON: I should like to ask Lauren Donaldson something. It is a broad question which I hope I can put sharply enough. Extrapolating from the experiences that you have had over the years with your ecological studies, what kind of situation would you visualize, let us say in the western part of the United States, in the Washington State region, if a sizable number of nuclear devices were exploded there? I am thinking of the aquatic biota, the river systems, the terrestrial fauna and flora, and so forth. Not an overwhelming number of bombs; choose the number yourself.

DONALDSON: I find it most difficult to answer your question, except in the most general terms. The number, size, and composition of the devices detonated would have to be stated in your problem. The conditions under which the energy was released, whether in air, on the ground, over land or water, or even under the water in a harbor, for instance, are important in outlining even a general answer.

If we assume the device is exploded in the air out of contact with land or water, the near-in fallout would be of little consequence to aquatic animals. If, on the other hand, it is detonated underwater, especially in salt water, the radiation problem would be maximal in its effect on aquatic animals. In the aquatic environment radioactive materials are carried by the currents; some settle out on the bottom or go into the deeper non-mixing layers; some are selectively concentrated in the food web and passed along the food chain, following many possible pathways.

CONARD: Did you say that over land it would not be of consequence?

DONALDSON: It would be of little consequence.

CONARD: I don't see why you wouldn't have a big fallout problem with the fireball if it was close enough to the surface to draw up and incinerate tremendous quantities of earth into the cloud.

DONALDSON: I assumed that it was a high burst for illustration.

ROOT: A high burst.

DONALDSON: I assumed a high burst, not in contact.

DUNHAM: I would like Dr. Wolfe to comment on this question because I think I know what Lauren is driving at, and that is that the earth is so different on the atoll than in the State of Washington, in terms of radiosensitivity, with the tremendous amount of pine forests, that maybe there would be a difference.

WOLFE: I would think that in the coniferous forests of the Northwest there would be widespread damage in the areas of heavy fallout, damage to the extent that the forests might be totally killed in areas. I don't know whether I'm talking to your question or not. This is one important thing that we know of differential sensitivity, that conifers are more sensitive and it would take a lot less radiation to kill the forests in the Northwest than it would to take them out in the Appalachians. In the Appalachians I think maybe fire would be the sole killer except in the pine regions to the southeast and along the coast. In the Northwest both radiation and fire in the coniferous forests can be rather disastrous. I know that there have been those who speak lightly of fire as a factor in nuclear war, but I noticed in the last fires, the fires in the Northwest, that there was available manpower and they couldn't do anything about them until they had run their course. In a time of nuclear war you won't have any manpower and you won't have any equipment. So I think fire and radiation would cause considerable damage in the Northwest over the land.

Fire is a striking example of a subject about which participants are all speaking truthful things but are not speaking to the same subject. The effects of fire will vary widely if we only consider geography and phenology as the variables. There are at least a dozen others. We can discuss fire storms in cities, but what we should be discussing is what cities are susceptible to fire storms.

DONALDSON: The problems are so different in the two environments. May we again use an example from Rongelap. When we first visited the northern islets, the area of heaviest fallout, we hurried to finish our essential tasks there and then waded out into the lagoon to wait for the boat to transport us back to the ship. On the islands we would have been in a high radiation field, but in the adjacent lagoon we were perfectly safe, except for possible shark attacks.

ROOT: Are you, Dr. Donaldson, referring to irradiated particulate fallout matter in the water which goes into the food chain and Dr. Wolfe referring to direct radiation?

WOLFE: I'm talking about the radiation that gets there, whether it's from fallout or any other source.

ROOT: But a high burst wouldn't be so damaging would it? There wouldn't be anything to come down.

WOLFE: I don't think it would. But this illustrates a question that has been put to the Division by the Joint Committee. They want to know, since we're conducting radiation studies at Oak Ridge and at Brookhaven, why we have to do them at the test site, for example. And the problem, I think, is answered in part here with the Rongelap study, that neither Oak Ridge nor Brookhaven nor Argonne nor anybody else could have predicted accurately, nor could have discovered, the thyroid difficulties that Bob Conard has reported on. You've got to go where the action is.

I don't know how I can put it into language for you, and I don't know whether we could put it on paper for the Joint Committee, Chuck. We've got a different environment; it involves different biota and different meteorology and different climates and different relationships altogether. That is just the way ecology is. It involves geography.

TAYLOR: Aren't there two very significant differences, at least, between the exposures at Bikini and Hiroshima and what you do at Oak Ridge and at Brookhaven? That is, the close-in dose rate phenomena are not producible on a large scale. You can't irradiate a group of trees in a very short time.

WOLFE: We do have a cesium source in a forest at Brookhaven.

TAYLOR: Yes, but some of the irradiations are in milliseconds, as I understand it. The dose rate phenomena . . .

UPTON: One can tend to simulate this with a fast burst reactor.

TAYLOR: Are these ecological studies?

UPTON: Yes. From a tower.

TAYLOR: Then let me mention what may not be a difficulty. Some of the significant effects, at least in the Marshall Islands, were due to fallout, literally to fallout, to material falling on the community that is being irradiated, and that has at least two effects that are different from what you get with a gamma source. One is that chemistry is involved, biochemistry, and the other is that there are things like beta burns which are not produced with a cesium source.

Now, in connection with this last thing, I have heard many people say that deciduous forests are relatively radiation resistant. Is it really clear that they are also resistant to beta and alpha activity distributed on the surface of the soil trickling down through the trees, particularly in the wintertime? The state of ecological complexity right near the surface is considerable and it would appear to me that you don't produce a lot of effects by irradiating to very high dose levels the first few millimeters of soil.

WOLFE: You just kill everything at very high levels.

TAYLOR: Yes. The question is, will that kill the trees?

WOLFE: Deciduous trees?

TAYLOR: Yes.

WOLFE: No.

TAYLOR: You say all of the transfer between bacteria and fungi and nematodes and all these things that go on in the upper foot are not affected by the fires?

WOLFE: I would doubt it.

[At this point in the session a film "Return to Bikini" was shown. The following discussion pertains to the film.]

DUNHAM: Lauren, this isn't the way I heard the story. There was a movie I saw a few years ago that was announced to the public by Ian Fleming with a 4-page spread in the London Sunday Times which showed little fish that had become disoriented, losing their way, trying to climb trees, which showed sea turtles who tried to find where to lay their eggs. They laid great quantities of eggs which were sterile and then couldn't find their way back to the sea. It showed piles

and piles of tern eggs, which were also sterile, and very few terns. Now, which is the true story, sir?

DONALDSON: Dr. Dunham, during the period from 1946 to 1964 we were at Bikini and Eniwetok for several months most years. We made a total of 23 separate expeditions. No matter how hard we looked we could not find a mudskipper "trying to climb trees." In fact, there are no records of mudskippers at either atoll nor are there any mangrove swamps, the preferred habitat for mudskippers.

DUNHAM: This was supposed to be an authentic movie of the aftermath of the atomic bomb in Bikini. Maybe you selected different parts of the atoll.

DONALDSON: I think one would have to do more than select a different part of the atoll, in this particular case. I think even John Wolfe with his great accomplishments in environmental control couldn't build a mangrove swamp out in Bikini without an outflow of fresh water. This sort of completely falsified popular release is nothing but disgusting.

TAYLOR: Who made that particular movie, do you remember?

DUNHAM: It was an Italian movie. It had a lot of other stuff in it. There were beautiful pictures, though. I must admit there were beautiful pictures of wildlife. As Lauren says, undoubtedly the ones of these mudskippers, as they call them, were taken in the mangrove swamps somewhere and there were lovely pictures of giant sea turtles laying eggs. Again they're apparently authentic pictures.

FREMONT-SMITH: Maybe it was the photographer that was disoriented; thought he was in Bikini but wasn't.

DUNHAM: That could be quite possible.

BUSTAD: Are there any natives now on Eniwetok and Bikini; are there any residents there?

DONALDSON: There are no residents on Bikini. The place is delightfully deserted; one can be completely isolated from the outside world here. The native Bikini people were evacuated to Rongerik Atoll in the spring of 1946. Rongerik was downwind from the tests, you recall, on the chart that Bob had on the board. When we visited this atoll in the summer of 1947, we found that the natives were hard

put to obtain enough food, inasmuch as a fire had burned over much of the atoll and because Rongerik is much smaller than their home atoll of Bikini. We reported this situation as forcefully as possible. The Navy had the responsibility for the Marshall Islands at that time. They moved these people to Kwajalein and then to Kili, a small island south of Kwajalein, and there they remained.

The people from Eniwetok, on the other hand, were evacuated to Ujelang, another island. They are not happy at Ujelang, again because it's not their ancestral home. It's smaller than Eniwetok and they would like very much to go home. However, they would find their former home greatly changed. To restore one of these atolls to its pre-test site condition would require Herculean effort. It isn't that life cannot go on there but that the very basis of their economy, the coconut, has been largely destroyed. It would take maybe 10 or 12 years to replant these areas with coconuts and make them productive.

We who have worked there have many friends among these people. We hope that it will be possible to get them back home again. I think this is a blight on our national record not to have done so.

CONARD: We certainly are trying to, aren't we?

DONALDSON: I have no knowledge of it.

TAYLOR: One gets the impression that the ebb and flow of the sea plays at least a major role in restoring the islands, restoring the atolls to their original states. Do you want to say anything about the relevance to this, to a similar situation on land, for example, in Nevada?

DONALDSON: The ebb and flow of the sea must play a very important role. The atolls are built from material extracted from the sea, and as they erode and weather the material is returned to the sea. During the years we have worked in the Marshalls we have seen some reefs form and others wash away.

SESSION V
THE SPANISH INCIDENT

INTRODUCTION

LANGHAM: As a proper beginning I would like to invite Merril to speak up with "incredible!" at any time he feels the urge.

FREMONT-SMITH: Or even with "credible!"

BUSTAD: Are you restricting it to Merril?

LANGHAM: Maybe I'm intimidating Merril. I'm sure I haven't the rest of you.

In listening to the discussion yesterday with regard to the socio-psychological reactions among the Japanese, I was thinking about how the reporting of this incident is so different from the things that Merril was saying. That was why I was saying "incredible," because my experience has been quite different from Merril's. Perhaps the problem we faced was not nearly as great, but I am sure that one cannot help but wonder why the reactions to these two situations were so different. I have eliminated a few pictures that deal with the details of the health physics and how we handled the contamination and related matters in order to concentrate on those things which I think have some bearing on the subject of this meeting. I feel the differences in Merril's experiences and mine in Spain may, in part, lie in the psychological conditioning of the people.

A pertinent question may be why the psychological reaction was so much different, because many of the problems were quite the same. There was delay by both nations involved in admitting there had been an accident that involved radioactive material, just exactly as there was in the other case. There was a serious economic problem insofar as the people in this limited area were concerned.

FREMONT-SMITH: You mean the nations didn't admit that there had been an accident? How long?

LANGHAM: Quite some time.

FREMONT-SMITH: How long was it? Are you going to come to that?

LANGHAM: Yes. I think it will be brought out in the discussion rather strongly.

FREMONT-SMITH: Physically.

LANGHAM: I had none of the problems Merril had, and I don't think it's that I'm that much better than he is! [Laughter] Of course, I think the problem was not nearly as great either. I don't intend to make any long speech, but I want to set the stage and see you wrestle with why the problems I faced were different from the ones that Merril had.

DESCRIPTION OF THE ACCIDENT

LANGHAM: As all of you know, there was an incident involving the loss of four nuclear weapons, each rated in the megaton class, and it has attracted its share of publicity. Here are three examples. The Saturday Review gave it a great play (Reference 45). There were two books written on it in this country (References 46 and 47), and an Englishman came out with a paperback (Reference 48) within two months after the incident was over. The two American books are rather good, and they're not bad accounts of the incident if you'll give, of course, the author's privilege of introducing a little trauma here and there.

Flora Lewis' book, One of our H-Bombs is Missing (Reference 46), concentrated more on the sea search for the one that was lost for weeks. Tad Szulc's book, The Bombs of Palomares (Reference 47), concentrated more on the land operation and went a little more deeply into the philosophy and where and why than did Flora Lewis. They both are relatively good books. There are mistakes, of course, like calling a scintillation counter an oscillation counter. The English book (Reference 48) is absolutely abominable. It gives everything wrong, and it's the type of unfortunate thing that so frequently occurs. The Reader's Digest carried a very nice article (Reference 49) on the incident, also.

The incident occurred about 40 kilometers from Granada, about 80 miles up the Mediterranean coast from Gibraltar, about 70 miles

west of Cartagena, in a very remote area right on the Mediterranean shore.

The incident involved the refueling operation of one of the Strategic Air Command B-52s as part of Operation Chrome Dome. I imagine most of you know about Operation Chrome Dome.

FREMONT-SMITH: No.

LANGHAM: It's given in great detail in Miss Lewis' book, as well as in Mr. Szulc's. Since about 1962 a certain percentage of the SAC B-52s have been airborne at all times carrying weapons, this being part of the deterring philosophy. This means that if SAC were entirely wiped out, still a certain percentage of the SAC force would be able to zero in on its prescribed targets. These flights were being made constantly, and there were elaborate pains, of course, taken to see that Dr. Strangelove's philosophy could not predominate, that some person could not take the war into his own hands by proceeding to target on his own volition.

The final act of fusing the bomb requires the word of the President of the United States, so there was no chance of a mishap of that kind.

Flying nuclear weapons of the megaton class over people's heads is serious business, of course, and so these bombs have built into them safeguards which make the probability of one of them giving a critical yield in an accidental situation like a plane crash about 10^{-7}. In other words, there is not one chance in 10 million that a criticality could actually occur, and this is because of combinations of interlocks, and so forth, which would have to be thrown in the right sequence before you would have an armed weapon.

The United States has no agreement which allows it to land a nuclear-carrying aircraft in any country. These aircraft must take off from the United States, fly their route and return to the United States without landing. This means refueling operations in the air at various points along the route. We had a refueling operation agreement with the Spanish Government. The 16th Air Force was in charge of the refueling planes which would take off from Spanish territory, meet the bomber supposedly out over the Mediterranean and refuel it; the bomber would then continue on its way. These, of course, were always called practice flights. They could, of course, be changed from a practice flight into the real thing by the right combination of messages, including one from the President. This is Operation Chrome Dome.

As a result of the accident that occurred, Spain immediately withdrew, or requested that no more weapons be flown over Spain. Some feel that this operation has about outlived its usefulness, that the intercontinental ballistic missile has replaced it. And so, Operation Chrome Dome is being phased out.

TAYLOR: Excuse me. Are you equating Operation Chrome Dome with air alert? Do you mean it's decided to stop all air alerts?

LANGHAM: No. I am referring to the type of operation in which an armed bomber with a target in mind is flying a practice flight in the direction of that target and then is turned around. This operation has been going on since 1962. This particular refueling operation has been done 140,000 times with nuclear weapons aboard without a single accident.

According to an Englishman who had an 8-millimeter movie camera, he saw the vapor trail overhead and when he looked up he saw a big puff of smoke and fire.

Immediately in the path of the falling debris was the little village of Palomares, approximately 400 inhabitants. Palomares had been there since the time of the Romans. In Roman times they mined the nearby hills for lead, zinc and various other minerals. At the turn of the century the mines began to run out. Many of the people left Palomares, but a few of the hardy citizens stayed behind growing tomatoes, raising pigs, sheep, goats, alfalfa and other things, agricultural products of that variety.

When the planes exploded, the four weapons came tumbling out in all directions and pieces of airplane fell absolutely all over the village. Disappearance of the planes from the radar screens at the refueling station let them know that an accident had occurred. There were 2 bombers on this run and two fueling planes. The other bomber reported that the accident had occurred. So, the accident was known within a few minutes after it occurred.

Immediately, contact with the area was established by the 16th Air Force. The principal way to get there was to drive over a very narrow bad road or to fly in by helicopter.

The first thing, of course, that one should do in a situation of this kind is to look for any indication of a criticality yield and, indeed,

this was done. So, the first group that flew in by helicopter looked to see if there had been any indication of a criticality yield and then started searching for the injured and the dead. There were seven American Air Force people killed and three injured. The next effort was to find the weapons, primarily because they included a lot of secrets, so-called, of our weapon technology, and so they were to be found at all costs.

THE SEARCH FOR THE LOST H-BOMBS

LANGHAM: Within two or three days a base camp was organized on the shores of the Mediterranean which grew to house 850 people before the operation was over. Almost immediately a search was started on land with these people lining up finger tip to finger tip and walking across the countryside looking for something that looked like a nuclear weapon even though, of course, nobody in the crowd had ever seen a nuclear weapon. They searched 49 square miles three times by this technique and part of that 49 square miles they searched seven times trying to find one lost weapon. The Bureau of Mines flew out a team which even inspected all of the old mine shafts and all of the old wells.

It was obvious that some of the weapons, one or two of them, could have dropped in the sea. So, the Navy was brought in on the operation and within two or three weeks the Navy Task Force had grown to 14 ships. They brought in the Alvin and the Aluminaut and the experimental devices that are used for deep sea recovery; and this turned out probably to be the greatest Navy exercise in deep sea salvaging and recovery that has ever occurred.

The sandy beach was a part of the economic and psychological aspects of the incident. The mines having run out, this was a depressed region. It has eight miles of the most beautiful Mediterranean beach you will ever see. All one would have to do is clean up some of the slag dumps and things left by the miners and one would have a resort possibility that could actually rival the French Riviera. The Spanish Government had actually underway a developmental program to develop this into one of the tourist resorts which are doing so much now for the economy in Spain. One can imagine the great concern of the Spanish Government; here was this development, and if there was a hydrogen bomb lurking around somewhere just waiting to go off, the tourists might not come. And so, this incident could jeopardize their entire program to relieve this depressed area by making it a tourist area.

After some time, within several hours, three of the weapons were found. One was found in the dry river bed just to the east of the village, right where the dry river bed joins the Mediterranean. Palomares sits on the mesa about a mile from the beach. Between Palomares and the beach are the ruins of the smelters which have fallen into great decay. These ruins actually were used as political propaganda. Pictures of these wrecked and ruined smelters were run in the Iron Curtain country newspapers as part of the aftermath of the American accident. And not only was that of some international political flavor but the Nuclear Disarmament Conference was meeting in Geneva at the time of the accident; and when it became known, even though it was not announced officially by either the Spanish or the United States Governments, the Disarmament Conference was terminated.

EISENBUD: The Western blocs walked out?

LANGHAM: The Eastern blocs.

EISENBUD: How long after the accident was that?

LANGHAM: They walked out immediately upon hearing of the accident even though it was not reported that nuclear weapons were involved. The Russians walked out of the Disarmament Conference and so it was disrupted. So this had international political trauma as well, as much as did the Japanese incident.

I forgot to say that the first weapon, the one that fell in the dry river bed, sustained only a dented nose and lost one fin. It was picked up by the helicopter, put on a truck bed and rolled away.

The second weapon completely overshot the village and landed in the hills to the west of the community. The chute didn't deploy; it impacted at full velocity in the side of the mountain. When the high explosive charge in the warhead went off the plutonium therein, of course, was converted to the oxide. The fine dust of oxide was thrown up with the dirt and bits and pieces of bomb casing into the air; the wind was blowing down the valley toward the village at about 30 knots. So the plutonium cloud drifted down towards the little village of Palomares.

The third weapon to be found impacted right at the edge of the village, 100 yards from city hall. One motor pod and wing of the B-52 fell within 80 yards of a school yard where there were 70 or 80 children

playing. There were bits and pieces of airplane all over the village. It was just absolutely unbelievable that that much material could fall down in a populated area and not hit sombody. But nobody was hit. In this case the weapon impacted in a rock wall of a man's tomato patch, the high explosive charge detonated and the plutonium was thrown into a cloud which drifted away from the village but down across their principal agricultural area.

Their prime cash crop was tomatoes, and they get two crops a year. The last one they harvest about the middle of January. They were just waiting to get in to harvest their last crop of tomatoes. Incidentally, it happened to be a holiday for a patron saint of the village. So, religion enters into the situation. For those of you who like to think of the theological aspects, the statement was made, "The hand of God was out in Palomares."

FREMONT-SMITH: The hand of God protecting the village or punishing it?

LANGHAM: Protecting it, because this is the only village that's had over 4 megatons of weapons dropped on it with nobody being hurt. So it does look as if the hand of God was out; this was what the Spanish thought, the people that lived in this area.

In this case, the contamination went down across their principal cash crop, their vine-ripened tomatoes, and so economics are involved here. They have a fishing industry also, and there was a question about the fish as well as the tomatoes; exactly the same thing that Merril was talking about.

The inhabitants of the area are very friendly, nice people. They like to have their pictures taken. Burros and carts were their principal mode of transportation. Many of the tomato fields were fertilized with soil that had been brought in in baskets, and the soil has been brought in over the years to make the tomato fields.

RADIOACTIVE CONTAMINATION & DECONTAMINATION

LANGHAM: Plutonium counts of the order of probably two or three thousand per minute per alpha probe area could be measured in the front yard of a few homes and there was a count of 500 or so sometimes on the living room floor inside the house.

The Spanish AEC colleagues were extremely knowledgeable and extremely cooperative. Their attitude was, "We'll worry about the people; you worry about the tomatoes and the contaminated fields. We'll take care of the people." They did a beautiful job. Here was cooperation, as you see, that was unbelievable and knowledgeable. There were only three or four of these people, but the two principals had spent a year or two in this country at Rochester and at Brookhaven. So they knew something about what they were dealing with, and some people took it quite lightly.

Their tomato plants are trained gently by hand to grow up in a tripod of stocks. They'll grow seven feet high and they were just loaded with vine-ripened tomatoes. In January on the European market they bring a nice price. This income was what was going to keep them going until their next crop.

The tomato vines gave readings of 10,000 to 20,000 counts per minute. The first effort was to get the vines raked up in a pile so that the plutonium wouldn't blow around and create a further inhalation hazard. Enough plutonium taken into the lung or the liver or bone would produce cancer. We've done this hundreds of times in animals. Plutonium, if taken in systemically is, indeed, bad. There's no doubt about that. And some people have referred to it as the most toxic substance known to man. I think this is erroneous, but you can get that belief by looking into the industrial toxicology tables at the maximum tolerable levels of various materials; and when you get to plutonium you'll find that plutonium-239 has one-half of a microgram as the MPL. That's one-half of a millionth of a gram as the maximum permissible body burden. If you're worrying about the plutonium-238, it's 250 times lower.

BUSTAD: I think that you should point out that ingestion as such. . .

LANGHAM: I would, yes. But the whole idea, as I said, is it's systemic deposition. The reason I feel plutonium shouldn't be given this terrible reputation is that it is extremely difficult to get into the body and one can eat it and absorb only about 3/1,000 of 1 percent of what passes through the gastrointestinal tract. In the lung the absorption is a little bit higher perhaps.

EISENBUD: I've heard the statement made many times. I don't understand the basis for it since the maximum permissible body burden for radium is 1/10 of a microgram.

LANGHAM: If we put it on the microcurie basis, then it's 4/100 of a microcurie as compared with 1/10 for radium.

Tomatoes, vines, cane stocks and all were taken to the edges of the field and hauled away to decide on disposal later. Crop destruction brings up the picture comparable to the Japanese.

UPTON: These weren't killed by radiation?

LANGHAM: These were killed by the Americans.

FREMONT-SMITH: At this time was there any knowledge yet locally or internationally that . . .

LANGHAM: Yes, but neither country had admitted it.

FREMONT-SMITH: It was just known but not admitted?

LANGHAM: Yes. It leaked out very fast but neither country would admit it. They admitted there was an accident, an airplane accident, but they wouldn't admit that any nuclear materials or nuclear weapons were involved.

FREMONT-SMITH: Even though all of these was being swept up?

LANGHAM: That's right.

DOBSON: Wright, what were the local people told? In what detail and by whom were they informed of the nature of the operation?

LANGHAM: They were informed by the Civil Guard, who seem to crawl out of the woodwork in Spain any time something happens. They were told by the representatives of the Spanish AEC and by our own people to stay out of the fields until told to go in. And so they were excluded from going into their fields where their tomatoes were about ready to be picked. Obviously, this caused the usual bit of concern and talk and soon it began to get around that there was a radioactive substance in the field. So, one began to hear the villagers talking about radioactivity. Their knowledge of radiation effects stemmed from knowing that in Hiroshima and Nagasaki thousands of people died from an atomic bomb, and one was occasionally asked the question, as a matter of fact, are we going to die? When told no, this satisfied them so they went away. Naturally this was a big thing in

their life. It was a place where nothing had happened since the Romans and all of a sudden everything seemed to happen. Visitors came in from nearby villages. Even though there were only 300 people, approximately, living there, the Spanish ended up monitoring 1,800 people because it became quite a tourist attraction.

FREMONT-SMITH: With no restrictions on local travel?

LANGHAM: Not except right in certain areas where we posted the Civil Guard and told the Civil Guard not to enter.

A gentleman who owned the tomato patch on the edge of the village was standing in the door of his home. The blast from the explosion blew him down onto his living room floor, tore one door off the hinge and knocked out one of his windows. That was the closest we came to having a Spanish casualty. Seven Americans had already died and eight more were killed flying in supplies and equipment. So, 15 Americans lost their lives. Not a single Spanish life was lost.

EISENBUD: How soon after the event was it known to the local residents that their crops would be bought?

LANGHAM: Probably 24 to 48 hours. I mean, the first thing they knew of it was when they were restricted from going into their fields.

EISENBUD: They were sure they would get a good price for their crops.

LANGHAM: A Frenchman claimed he got a radiation burn on his knee from looking into the hole. He got down on one knee, looked into the crater and then his knee got sore after that and he said he had a radiation burn on his knee. Of course, this is alpha activity and it got on his pants but he could not have received a radiation burn. Later the highly contaminated area was delineated with red flags to warn people. They hardly knew what radioactivity was, you see. To us a red flag means danger, "Don't enter," but to them it means much more danger than it means to us, I guess.

FREMONT-SMITH: "Very dangerous. Don't enter at all."

LANGHAM: Yes, or, "Run the other way." I don't know, except that the red flag created enough commotion and our psychology friends can explain this, I think.

DUNHAM: I think Merril's point about it having political significance may be important.

EISENBUD: The red flag is what the Loyalists carried during the Spanish Revolution.

LANGHAM: Yes. Maybe that did it. All I know is that we had to get the red flags down fast for some reason or another. Scraping up the plutonium contamination where we felt it was dangerous was begun even though no agreements had been made with the Spanish Government as to the extent of cleanup. In other words, remedial action was started even before there was any agreement.

FREMONT-SMITH: Yes. Starting to occupy a bit of Spain, so to speak, by the soil.

LANGHAM: Yes. By the time we were through with the land operation, Palomares took on a different appearance. The houses had been hosed down in many places; some of them had been rewhitewashed. The fields had been plowed clean, with the exception of irrigation ditches, which we finally got the Spanish to agree to let us leave; the soil is so bad that it takes 10 years to stabilize an irrigation ditch, and if we had stripped the vegetation at the irrigation ditch we would have had a problem there. So, the Spanish agreed to let the irrigation ditches stay. Some of the fields were not stripped. In other words, we had agreement with the Spanish, finally, as to what we would strip and what we would plow and what we would compensate for, and so forth.

Great piles of contaminated soil and trash were collected at impact point number 2. The question was what do you do with it? We talked to the Spanish and asked them if we could bury it and started digging a trench. The Spanish became concerned and asked that their geologists and hydrologists look the situation over to see if there was any possibility that this could eventually get into the water shed. This action delayed things a week or so.

The size of the burial pit would resemble something approaching the length of a football field, half the width and 40 feet deep, depending on what kind of agreement we could get with the Spanish as to how much we had to remove. But we started digging anyway. Before digging anywhere, we used tank trucks and sprinkled, theoretically to hold down the inhalation hazard of the people working there.

UPTON: Did the people wear respirators doing this job?

LANGHAM: Some of them did, some didn't. Most of the respirators were surgical masks, and if it did something for your psychology to wear one, you were privileged to wear one. It wouldn't do you any good in the way of protection but if one felt better, we let them wear it. We ran into such psychological problems. The manual says you will dress up in coveralls, booties, cover your hair, wear a respirator, wear gloves. That's what the manual says. So, some people tried to do this where it was possible to find something that resembled this type of equipment, and before long this caused consternation in the village. "How come you dress up like that and you let us walk around in the village with our street clothes on?" So, even little things like that, that I had never even thought of before, became problems psychologically. Why shouldn't we be protecting them if we were doing all of this protection in the area? Most of the time the clean up operation would hardly meet the standards of the health physics manuals, but I think there was not anything wrong with this operation.

EISENBUD: How soon after did you arrive?

LANGHAM: I guess I got there at about noon on the third day, something like that.

FREMONT-SMITH: Where were you when you started?

LANGHAM: I was in Washington.

FREMONT-SMITH: A good place!

WARREN: Did you go home first?

LANGHAM: No, I didn't go home. They told me to proceed to Madrid, and I didn't even have a passport. I was in Spain all of this time without a passport, only a pair of pajamas and a shaving kit. They did give me a little gear. I stayed only five days and came home. I was home four days and they sent me back for five weeks. When I arrived it was obvious there was no real health problem. This was not a health problem. The psychology, economics, international agreement, these are things with which I claim no competence whatsoever. So, at the end of five days I came home only to be sent back, assigned to the American Embassy, when negotiations were started.

PSYCHOSOCIAL, ECONOMIC AND POLITICAL ASPECTS

LANGHAM: The Spanish wanted the burial pit finally lined with asphalt, so this was agreed to. Then they decided that they wanted a concrete slab put over it and a fence put around it, and the United States to take a lease on it. I kiddingly asked them if they wanted the lease to run for 5 half-lives—120,000 years. When the State Department heard we were contemplating building a monument to this unfortunate incident, we were told to take the material out of Spain. So a barrel factory was leased in Naples, put on 24-hour duty and in 2 weeks produced 5,000 steel drums which met the specifications. When the barreling operation was complete we had packed up 4,879 barrels of this material, hauled it down to the beach and put it on board a freighter out in the Mediterranean.

The next question to come up was, what do you do with it, now? The obvious thing to do was to haul it off a few miles into the Mediterranean and throw it overboard. You would be surprised how many people objected to this! [Laughter] Gen. De Gaulle's government was one, as did many others, whether or not they owned even remotely a shore on the Mediterranean. In fact, people objected who had no coastline whatsoever on the Mediterranean.

The decision was made to bring it home. You may think our problem ended there, but the Agricultural Department heard about it and said "That's Mediterranean fruit fly country and you can't bring it in!" [Laughter] I tell that partly as a joke. It so happens that the Agricultural Department did object and they did say that we would first have to sterilize it; and they suggested ways and means of doing this. After a while they did agree that if it was brought in and buried in the steel drums, there would be no possibility of fruit fly larvae, and so forth, getting to the surface.

FREMONT-SMITH: How about the Governor of the receiving state?

LANGHAM: He probably didn't like it too much, but it was deposited at Savannah River in the AEC's burial ground, and I guess the Governor felt he couldn't protest too strongly. But there were protests from that area about bringing this back into the United States.

Some statement was made about how the State Department's mode of operation once in a while causes trouble. The Ambassador, Angier Biddle Duke, is very well liked by the Spanish people and is a very competent person, but it was just traumatic to see him try to do

something, primarily because it seemed that even the Ambassador doesn't dare do anything, even give out a news release, without a check to Washington. I think this thing could be more simply done. I think Angier Biddle Duke could have been more effective if he had just been able to initiate a bit of action himself.

This is the story except for a lot of details and the psychology of the news releases, the many, many things that I would much rather hear discussed here than to have to continue talking about myself. In other words, I think the interesting thing here was something that had all of the qualities that were in the situation Merril was talking about. There was never a panic or anything resembling it. There were little flareups. There was a little demonstration for an hour or so at the University of Madrid which was nicely timed. They were allowed to demonstrate and then they were told to quit, and when they were told to quit, they did so. There were a few days when fish were not bought. There was a little rough time when any tomatoes from the south of Spain, whether they came from Palomares or not, were not being picked up by the distributor, the middle man.

In the high level meeting, in which the Vice-President of Spain participated, it was pointed out that their distributors were not buying the tomatoes from the south. You see, the distributor goes down and buys them and brings them to the city. He was afraid to buy them for fear that when he got to the city he wouldn't be able to sell them. So, they were more or less not buying tomatoes from that whole area. When the hint was dropped to the Vice-President, and since the government licenses these people, the government said, "Those tomatoes are all right," and indeed they were. In three days there was re-establishment of distribution channels.

The release of this information tells something, too, and that Miss Root is not going to like, I imagine. We had a bilateral piece of paper that we actually handed to Munoz Grandes. This was something that the State Department had agreed upon, and which was going to be bilaterally released, in which the two governments simultaneously admitted that the accident had involved nuclear weapons. The Vice-President looked at the piece of paper, which was a very benign little thing, and as you might expect, that was the last of it. Three days later, unilaterally, on the second page of their leading newspaper, a beautiful article came out written to Otera, head of the Spanish AEC, in which he told the details, what the situation was. It was a most magnificent bit of factual reporting. When I came into the Embassy the people were running up and down the halls and one gentleman

said, "We're having a meeting. Otera has blown his top. The whole thing is out in the newspapers." The cat was now out of the bag.

EISENBUD: On what day was this?

LANGHAM: This must have been the 1st of March, somewhere along in there. It had occurred the 16th of January, something like six weeks later.

Part of my job with the Embassy was to read all the newspapers. I could not read Spanish, but I would get translations of every little article on which I was to advise as to technical accuracy or as to whether it reflected in any way on the American image. When asked for my opinion of the Otera article I replied that it was excellent and that I wished I had written it myself. No one appreciated my humor. The article was a good one and I do wish I had written it myself. But it just seems that when the American image is involved, people have no sense of humor whatsoever.

FREMONT-SMITH: Did they send you home the next day?

LANGHAM: No, but I wasn't invited to go on Duke's swimming party! [Laughter] In fact, I wasn't even invited to advise him on that. I might have advised him not to do it because it drew adverse criticism. I've heard it criticized especially by the English.

FREMONT-SMITH: What was the criticism?

LANGHAM: The Ambassador and the Minister of Industry went down to Palomares and had a press conference on the beach, then went in swimming even though the temperature was 54 degrees, to show there was no hazard. This was considered by some to be a stunt to show people that this wasn't going to hurt the tourism. The Spanish Government's greatest concern seemed to be to find the lost weapon, because of the possible impact that this could presumably have on the development of this area as a tourist resort. It's going along fine, now. If anything, I think now it has received a little added push. The people seem to be back to normal. We're following them to see if they have any plutonium in them. So far it appears that they do not, and I think this is an incident which in terms of importance will not even be a grain of sand on the beaches in time.

FREMONT-SMITH: How long was it before the fourth weapon was dredged up from the ocean?

LANGHAM: It was about nine weeks or so. They had a terrible time finding it. Admiral Guest drew some criticism because of the Spaniard who said he knew right where it went down and, indeed, they found it right where he told them it would be. But Admiral Guest came in and started a systematic sea search in which he started from the beach with skin divers and then he went to hard hat divers as he was going out. He was making a systematic search of the entire bottom. They found old cannon balls, pieces of airplanes, etc. They literally searched the bottom of the sea systematically and then finally it got so deep that they had to get experimental equipment like the Alvin and the Aluminaut. With this kind of equipment, they could finally search the area where they were told the bomb went down, and that's where they found it. They got hold of it, lost it, and it slid down a little further; they got hold of it again and finally got the thing up. Of course, the criticism of the Admiral for not searching where the Spanish fisherman advised is unjust. The Admiral had no capability to search at the depth where the fisherman advised, and while waiting for deeper sea equipment, the Admiral and his staff felt it would be advisable to make a systematic search of the shallower water in the event the fisherman was wrong, so that the shallow areas would have been already searched.

Part of the good humor going on between the Navy and the Air Force was that the rule book says the person who has custody of the weapon is responsible for the clean up and the recovery in the event of an accident. So the question was did the Navy on the first try have hold of it long enough to establish custody? If so, they would have to pay the bill thereafter! [Laughter]

FREMONT-SMITH: Did they?

LANGHAM: No. You never put one over on the Navy, not even here! [Laughter] So I think the land operation probably cost of the order of $1,800,000 and the Navy charged the Air Force $5,200,000 for the sea search. There were x number of dollars in damaged weapons and three aircraft. According to the authors of the popular books on the subject, the taxpayer probably inherited a bill, counting the cost of the aircraft, approaching $30 million. Counting the plane that crashed in the mountain flying in supplies, perhaps $35,000,000.

FREMONT-SMITH: A little less than Vietnam for one day.

LANGHAM: Yes. Not even that.

WOLFE: The Spaniard that knew, was he a fisherman?

LANGHAM: He was a fisherman. He was the one that pulled the pilot out of the sea. When the plane broke up and the pilot and the bomb dropped out, they both popped their chutes immediately; this happened at about 30,000 feet and there was a strong wind blowing. So, one bomb and the pilot drifted almost five miles out to sea. The fisherman swore that he saw two chutes and that one of them, if it had a man on it, carried a dead man; and he kept telling them where he saw the second chute. He said the chute was different.

BUSTAD: Another interesting part of this is that he described it to them, the impact point, and on two successive days took them to the same spot.

LANGHAM: Yes. This was his fishing ground. They had him go to the spot two or three times. He went to the same spot within 200 feet.

MILLER: Why would the chutes open for two but not for four?

LANGHAM: The chutes were not supposed to open on any of them unless they were signaled to do so. When this plane fell apart, evidently these weapons got an impact in the chute cannister which popped the lid off a couple of them, and then it was a matter of aerodynamics whether the chute was dragged out. They found the tail plate off the chute cannister to the weapon that drifted out to sea. They found the tail cap to the chute cannister and this is all they could find anywhere.

DOBSON: Wright, in the early and less certain part of the whole episode, when you first arrived was it difficult to find out whether or not there was a health hazard?

LANGHAM: No. SAC has a response crew. Albuquerque has a response crew. These people were all arriving at about that time. There was the usual meter problem. At one time we had 12 alpha meters, one of which was working, and you can't do much monitoring with one instrument. But the Spanish, believe it or not, had four or five instruments. So the Spanish came in with their instruments and by the time I got there they and the response teams already had a crude outline of the levels.

DOBSON: The Spaniards had better monitoring data when you got there than the Americans had? Is that what you mean?

LANGHAM: Not necessarily better, but they had contributed to the fact that there was quite a bit of data of a preliminary nature by the time I got there. They could show crude contour plots and where the accident occurred and what way the wind was blowing and other useful data.

DOBSON: How did you find out whether anybody had a real snootful of this stuff?

LANGHAM: Largely on intuition. Within an hour after I was there I was completely relaxed. This was one of these situations where the circumstances were all just right. If we do this again, we may be in trouble because we have had all our luck on this one. The wind was blowing right, the people weren't in the field and pieces of the airplane fell beside people but not on them. It's just one of these things where everything broke right; there are, of course, the lasting effects, as you might expect.

From the psychological point of view it may interest you that here was a community in which there was no class distinction whatsoever and now there is class distinction—the man who got compensated as opposed to the man who didn't. The man who didn't is a forgotten kind of second-class citizen, at least he feels that way. So there's social stratification now where it didn't exist before. One woman has been deathly sick ever since and, of course, it's due to plutonium. This was the woman who was standing in her front yard when a burning American body fell right at her feet. She tried to put it out by scraping and putting sand on it and she's been sick ever since. I think if I had done that I would probably be sick, too. But, of course, they think the logical thing in this is that the plutonium is making her sick because she was down in the dust scooping it up and so she must be full of plutonium and she'll not return to her home. Every time an animal dies, of course, the question does come up. There have been agitators in and the population will flare up and there will be a little demonstration. Some of their own authorities come in and quiet it down. So this is a game, you see, and there are a lot of psychological implications to all of this that I would just like to hear you people speak about.

SPEAR: Was there any period when the farmers were looking out and seeing their prized cash crop being bulldozed into piles and not having any idea that they would be compensated for it?

LANGHAM: This was not the case. This is one of the things that I think has something to do with this difference in the reaction. The Spaniard, believe it or not, respects his officials and he believes them, and these Spanish AEC people came in and explained the situation to them.

My wife and I became great friends with a waiter. When we were ready to leave, I said to him, "You never asked why we were here. Has it ever occurred to you?" He said, "No, not particularly. A lot of people come here, a lot of nice people." I said, "Well, we've been here associated with Palomares, you know, the weapons accident." "Oh, yes," he replied, "Palomares. It's somewhere down on the coast. I saw something in the paper like this but we never get excited about anything we read in the papers!" So their whole attitude was fine. The only time there was some trouble was with the Red Duchess, who is three times a grandee. She was thrown into jail for 24 hours once; she's a real agitator. She showed up at Palomares with two doctors she had hired herself to give these people physical examinations; she was telling them that they had been mistreated, they might be sick, and maybe they were going to die. As a result there was a flare-up in the community, and so the Spanish officials and authorities had to go down again. The people will go up and down depending on how much they're agitated. If you can just keep the agitation low, the problem is low. This seemed not to be the situation in Japan.

EISENBUD: There were a lot of differences, I think, that are quite apparent. What about the press? How large a press corps did you have? Did you have the foreign press?

LANGHAM: You had the foreign press, a few wandered in and out.

EISENBUD: What was the total press corps at the height of the excitement?

LANGHAM: You never knew. They just wandered in and out. The people were too busy to give them any information. So they wandered away again and then, in fact, much of the criticism of the way this was handled has come from the press. I mean we were too busy. We didn't bother with them and they got tired and went away. Furthermore, the sea search stole the show, you see. I mean those H-bombs sitting out there just ready to go off, stole the show. So they really didn't bother with us so much.

WOLFE: The gentleman, the fisherman who knew where the bomb was, did he get a prize?

LANGHAM: He got money, he got a decoration. He got his boat painted. Then a lawyer got hold of him and he's suing the American Government for $5 million.

MILLER: What for?

LANGHAM: He says the value of the weapon was at least $5 million. Well, that's an inflated price. I happen to know that it didn't cost that much. The lawyers are trying to file a suit that he really saved the American government $5 million.

BUSTAD: There's another aspect of it that I heard and I wanted to check it out with you. I heard that there was a lot of discussion as to whether they should picture the weapon after they retrieved it, and that they decided for publicity purposes to have it pictured. It might be interesting to present the background on that.

LANGHAM: Well, of course, the international propaganda was that these Americans just might sneak in a dummy bomb on you and say, "See, we found it." So the question was how would you prove that indeed you found it? So they finally decided to let the photographers have a crack at it and take its picture as they were bringing it up, and so forth, and so this was done.

WOLFE: How did the accident start? Did the two planes collide during the refueling operation?

LANGHAM: Yes, evidently. I'm sure this has been investigated by the military and by the Air Force at great length; about the last word I think one heard was that of a man guiding the refueling pipe who yelled at the pilot and said he was approaching too fast. I don't know what happened, but the plane broke in two, the bomber broke in two right in the middle, and there was a spilling of fuel, the tanker exploded and everything exploded. I've heard that there's been a real serious investigation of this accident but I think the Air Force is not saying the specific details.

WOLFE: It's much too early for that. We won't get that until 1980!

CONARD: Where did you get all the top soil to replace what you scraped off?

LANGHAM: I don't know exactly myself where it did come from because I was in Madrid most of the time during that phase of it. But what they were going to do was to let the farmers themselves pick the area they wanted it brought in from. So they picked an area that wasn't too far away.

FREMONT-SMITH: You mean we don't have any USA soil over there?

LANGHAM: No, it wasn't shipped from this country. It was local soil that wasn't contaminated.

SPEAR: One of the disturbing things about it, if I read this correctly, is that apparently the suppression of news, the suppression of information, was a very helpful factor in holding down any kind of panic reaction; that if this had been a more sophisticated local population they would not have been as ready to accept the simple word that "You're going to be all right; you'll be taken care of." This I find disturbing.

ROOT: Well, they had had a controlled press in Spain for so long that even if it got decontrolled, people would take a long time before they would begin to read it. Newspapers in Spain are very rarely read because they are government handouts and have been known to be so for a great many years, whereas in Japan, as Dr. Schull pointed out, there's the most terrible competition for news. The newspapers themselves are so rich that Yomiuri, for instance, has a whole pool of automobiles and when a reporter was taking me out, he just commandeered a car with a chauffeur and took off. You don't even have that in New York. Also they send two or three reporters out on the same story so that they can cut each other's throats and get the best report possible.

DUNHAM: The best in what sense?

ROOT: The most detailed with the most intimate pictures. Reporters were piling in through the windows in the hospital, where you're not even supposed to enter without permission. They were climbing up the walls and falling through the windows to get pictures.

EISENBUD: They carry aluminum scaffolds with them. They will think nothing of just rigging a scaffold up to a second story building and looking in a window, and nobody stops them. [Laughter]

WARREN: With a camera, too.

EISENBUD: Yes, with a camera. They use them in crowds so that they can get up above the crowds and they just rig them up and take them down. Frankly, I see very few similarities between the two incidents. You have a situation in one case where nobody was hurt. In the other case, you have 23 sick people. You have a relatively unsophisticated country under strong essential control in one case. In the other case you've got a highly sophisticated scientific corps totally disorganized and all seeing in the Japanese incident the first opportunity that they have had post-war for any kind of self-recognition; they were jockeying for power and seeing who could say the strongest anti-American things because this was the kind of thing people wanted to hear at that particular time. You had an AEC in Spain which they didn't have in Japan. You had a Dr. Ramos, whom we all know, who was very friendly. The nearest counterpart in Japan would be, I suppose, Tsuzuki who at least by reason of age and long accomplishment, was recognized as a senior person, and he was fundamentally anti-American for reasons which maybe Staff Warren would want to expand on. He was a former Japanese admiral who, I think in his later years, came around for opportunistic reasons to be friendly to us, but I think under it all he was not. You had a situation in which the barber found that business was good. There must have been other people besides the barbers that maybe benefitted economically, whereas in Japan the bottom suddenly dropped out of one of their major industries.

FREMONT-SMITH: It did also in Spain because all the crops were lost.

EISENBUD: Within 24 hours after the accident, Wright said that they had agreed to buy the crop.

FREMONT-SMITH: Yes, that is true.

EISENBUD: Which is a good thing. The crop is sold; you don't have to worry about the spoiling, you don't have to worry about finding a market.

DOBSON: Isn't the most enormous difference the fact that the Japanese felt that something had become hazardous about fish, and at the same time they had to look to fish for their very sustenance?

EISENBUD: Yes.

DOBSON: While the crops in the Palomares episode were not so essential, even the tomato crop. This was not the Spanish tomato crop; it was a relatively few patches. So there was no national threat . . .

EISENBUD: Yes.

DOBSON: No imagined national threat.

WARREN: It wasn't there for a while until the word went down from headquarters that they'd better buy up the tomatoes in the south because there was nothing wrong with them. It was there for a few days or so.

LANGHAM: Yes. You saw elements.

WARREN: Of the possibilities.

LANGHAM: You saw the elements of such a development as were seen in Japan. That's the only thing I'll agree to. This was of great economic importance for the local area. I don't think it was of economic importance to the nation necessarily except for tourism, which was definitely concerning the high officials of the Government. But there was great economic hardship brought to bear on the whole area insofar as that goes. So that element was the same and you could see indications of this causing considerable trouble; but it was kept so localized that it never attracted any attention, particularly as compared to what the Japanese did.

MILLET: I think the question of the relation of the populace to the leaders is a terribly important thing here. I was very much struck by the statement that these persons believed in their leaders and so were not victims of panic. If the leader, whom everybody trusts, comes out before the panic gets started and says, "You're going to be all right, don't worry. Now you go about your business, we'll take care of the crop for you," that's one kind of thing. But, on the other hand, in the Japanese instance, you've got some criminals here to begin with and you've got a very dubious relationship between them and the governing group in Japan, to say the least. So there are a lot of psychological differences here that make it quite clear that there would be a different kind of reaction, I think.

FREMONT-SMITH: How did you mean criminals?

MILLET: These sailors who didn't want to come home because they were going to get in trouble.

FREMONT-SMITH: I see. You mean because they had been in jail before.

MILLET: Whereas, the Spaniards were all good virtuous peasants.

ROOT: I think another thing, you got very little information until the press became absorbed in the search. Then everybody was with the drama in Spain hoping and praying for a happy ending. In the Japanese incident you got no knowledge at all until burned bodies came home. Then, worldwide reports followed concentrating on the horror, with no explanation and no preparation. This I think had a lot to do with the global impact.

CASARETT: Certainly one large difference between the two incidents is the previous experience on the receiving end of nuclear weapons. I should imagine that such sensitization would be much greater in Japan.

FREMONT-SMITH: You mean if Hiroshima had been in Spain, you would have expected an entirely different response?

CASARETT: Yes.

MILLER: Dr. Schull called the Bikini experience "anaphylactic shock."

WOLFE: Is there any record of anybody but the United States dumping radioactive material or bombs or what not on other nations?

LANGHAM: No.

WARREN: The Russian, Chinese and French fallouts, that's all. That's not a weapon.

WOLFE: But there have been no large incidents. If they had one, we do not know about it yet. They would be slow in letting it loose.

LANGHAM: This situation was a bit unusual.

WOLFE: Yes. I just wondered why we're always getting in the unusual situation.

ROOT: Another thing was that this was so obviously sheer accident. An explosion in the air—seven men, pilots and crew, killed, all Americans. It was more likely to make people feel sad, than mad. Whereas the Bikini shot was a deliberately planned test. It was portrayed throughout the world as a cynical determination of the sorcerer's apprentices and their government to advance their weapons technology regardless of human cost. A terribly unfair judgment, of course, but these were the press repercussions and the impressions of people and governments.

LANGHAM: Some of our friends in other countries seemed to think that it was a bit of a deliberate act to be flying over people's heads with things of this nature and this came in for a great deal of international political harangue as you might expect.

ROOT: Yes, you would get that.

TAYLOR: "The Sword of Damocles" talk was revived for a while during the Palomares incident.

CONARD: Couldn't they have re-fueled over the Mediterranean rather than over this village?

LANGHAM: Yes. In all probability this was their instruction, but they had done this so many times that contact was made a little bit closer to shore than expected. I think this has been a big part of the investigation, on the part of the Air Force to quite an extent: wasn't there actual human negligence or error on the part of the crews? As far as I know, no action was taken. It was just an unfortunate accident, and to put two planes together at 30,000 feet is probably not something you always do right on a set spot each time. I'm sure it has all been gone over very, very thoroughly by the Air Force.

WOLFE: Does Russia maintain a Chrome Dome?

LANGHAM: Not that I know of. I think this is an American innovation.

MILLER: There is a manual which tells what to do in an event like this, but it mainly concerns threats to health, and I suppose

describes procedures to be followed, measurements to be made, contaminated areas to be demarcated, and techniques for decontamination. But shouldn't there also be devised a manual concerned with psychological problems, political problems and economic problems? Is this a matter that should get some thought on the basis of your experience?

LANGHAM: Well, I'm pretty sure it's getting thought. It's just almost impossible to sit down and write a manual that's going to fit a situation. You have to visualize the situation and then you write the manual to fit it.

FREMONT-SMITH: This means every potential situation the manual has to cover.

LANGHAM: Yes, and invariably it occurs where you don't expect or under conditions you don't expect.

FREMONT-SMITH: And the manual would have to say, "Use good judgment."

LANGHAM: That's right. There's no manual for a situation like this. For example, I finally ended up with some dear, dear friends amongst the Spanish people. I mean they are wonderful people, at least the ones I dealt with, and I have no reason to think that they all are not. But we started out bargaining. Now, how much would we clean up? And one of the gentlemen said, "Well, we think you should pick up every atom of plutonium you dropped in Spain and remove it," to which I replied, "You know, of course, that's an impossibility, don't you?" He said, "Yes, but it's a good position to start from!" [Laughter] We made a decision that if the contamination was above a certain level, the soil and crops would be removed; if it was between other levels the land would be plowed; and then at the lower levels it would be sprinkled; and at still lower levels nothing would be done with it. So we set a level at which we would plow. No one really thought about it. This was just part of our agreement. When we got into the mountainous area where the contaminated cloud had come down in the valley, it was just sheer rocky mountainsides that were contaminated above the level that we had agreed to plow.

FREMONT-SMITH: You can't plow a mountain.

LANGHAM: So how do you plow a rocky mountain? We went to the Spanish with another problem. This was a rather big crisis and the Spanish replied, "Well, as we recall, plowing was your idea, so plow!" [Laughter] It was finally agreed in this case that they would elevate their standards a bit and we would resurvey the whole area and if we found areas contaminated above this newly agreed limit we would actually work it with pick and shovel and stir it around, the idea being to get the plutonium beneath the ground so that when it blew it wouldn't become an inhalation hazard by resuspension, and some of these hillsides were pick-and-shovel-worked into the soil instead of being plowed. They were reasonable people.

Now, this doesn't mean they don't drive a hard bargain. They took the recommendations that were first proposed and essentially divided them by two and made us go to one-half the level we had proposed. That's all right. If you're writing manuals—and I've written a few—you decide on what the proper standards of cleanup are. But it comes as a bit of a shock to find out that if it's the other fellow's backyard that you've dirtied up, maybe he has something to say about the standards. You don't come in and tell him you'll clean it up according to your standards. You clean it up according to his, and these people drove a hard bargain, but they were nevertheless reasonable. I had a great respect for the Spanish AEC group. I think they have some highly competent people. The whole Spanish AEC isn't as big as the group Dr. Dunham used to have in the Division of Biology and Medicine alone. I mean that's their whole AEC and yet in it they have a few highly competent people.

WOLFE: Wouldn't it have been much cheaper to haul soil in there and cover it over than to carry it out?

LANGHAM: Of course. It was carried out and then fresh soil replaced on top of it. I think it's just as well, right around the crater areas, that that was removed. At least you know it's no longer there. They let us plow under, you see, a lot of it and they asked us to give them a soil followup program because we had plowed something with a 24,000-year half-life into their soil and I'm sure they would have objected if you had just buried it there, too. They felt happier about having it removed, and so did I, where the levels were high.

UPTON: Will there be a followup of some kind?

LANGHAM: There is a followup program.. This was part of the bargain, that we would set up a followup program.

UPTON: What are the objectives and the scope of this program?

LANGHAM: They've got a people's program, a soil program, a vegetation or a produce program, an air sampling program, and they were extremely clever in the way they approached us on this. They said, "Now, we've taken your advice and we're sure that you have given us the right advice. Will you please set us up with a program and equipment so we can prove to our authorities that we were right in following your advice." So they have a followup program, yes.

UPTON: Under their own auspices, or do they furnish advisers?

LANGHAM: This was very strong in their minds. They wanted this to be their program with us providing the backup, giving them the equipment, teaching them the techniques, which we've done, and occasionally advising them and letting them send people to this country. I've had two or three of them at Los Alamos already.

I think you'll find that Spain wants to get back into the swing of things; they want, above all, to use this to maintain a contact, and they want contacts. I would bet that if you counted the number of friends we have got in Spain as compared to what we had before this accident, we would count more friends there than we had before. This was an opportunity for them to get outside contacts.

FREMONT-SMITH: That's why you got your medal!

LANGHAM: Well, I never quite figured out why I got a medal, because this is a rather sober thing within itself. What you find is that circumstances place you in a position that you can't get out of and you are the focal point of the effort of an awful lot of people. Winning medals is just being in the right place at the right time.

FREMONT-SMITH: I think making friends was the crucial thing. That's why we have more friends. Anyway, I think there's a very interesting comment, because I think we don't have more friends probably, I'm not sure I am right, in Japan as a result of the thing there.

EISENBUD: I think that we had the same reaction in the scientific community. There are a lot of opportunists among them. It was quite common during those first few weeks for the younger people to sidle up to me and ask how they could go about getting fellowships in the States or ask if I could, when I went back, send them some reprints, or ask how they could learn about a certain piece of equipment. As Chuck will recall, starting with Tsuzuki's visit to the States in May, which was precipitated by this accident which had occurred two months before, there was a long series of exchanges. We had that radiobiology conference in the fall. The Division of Biology and Medicine began to support research in Japan and any number of the young people began to come to this country as the result of that incident.

FREMONT-SMITH: So it was comparable in a way.

EISENBUD: I think we really have had the same types of ties but this, I think, is a form of opportunism. I presented to them the first sodium iodide crystal that they had ever seen and they appreciated it very much. But I'm sure that we couldn't say that the same was true at the level of the people, where I think there are some scars.

There was one other difference. At the height of the Japanese furor which was, say, a week or two after the boat got into Japan —I think it was the 26th of the month but it might have been a few days later—the AEC resumed testing in the Pacific. All through that spring until the end of May there was a series of tests and each one of those, of course, precipitated new rumors and new concern. All through that spring there were rumors of fishing boats that had been heavily irradiated, apart from the question of contaminated fish. They were concerned, too, about the health of their fishermen.

MILLER: Merril, when you went to Japan, what kind of experts did you wish you had with you who were not available? I can think of two who might have helped you. One would have been a public relations man experienced in this sort of thing, and another might have been a person who knew Japanese culture exceedingly well.

EISENBUD: The Embassy presumably had this. I think Mr. Allison had a good feel for Japanese culture. As I recall, he spoke the language. As far as public relations were concerned, this was controlled out of Washington. We held an off-the-record briefing

for the American press and this was helpful in the way the news was reported in the States. But we were not permitted to meet the Japanese press until the following November when we had some very successful news conferences in which a lot of this was rehashed, and I think it did some good. But all through the period that I was in Japan, neither Ambassador Allison nor I met with the press. The only direct announcements from the Americans were from people who were just passing through and who had no relationship with the thing, but felt that they would like to be spokesmen. All they did was muddy the water.

MILLET: There is a very high level of comraderie between the American psychiatrists and the Japanese profession, too. We went over there for a short conference, then brought them back the next year and hosted them to go down to Mexico for an international congress with the Mexicans. That's been a very profitable experience for everybody.

EISENBUD: When it was all over, John Morton and I decided to go to Eniwetok because he was interested in finding out what he could about the natives there. Bob Conard and Chuck Dunham and I, and others, thought it was a secret that we were leaving. We learned in retrospect there really weren't any secrets all through there, that almost every move we made was pretty well known to the Japanese. When I got to the airport, the whole scientific corps turned out to say good-bye to us. My house is decorated from one end to the other with lovely presents that were given to us, and I think it was quite sincere.

SCHULL: One has to be careful in placing too much emphasis upon events of that kind in Japan. Courtesy requires that individuals of prominence be welcomed and sent off, even when they may not be liked. To do otherwise is a reflection upon oneself.

WARREN: Is one supposed to give presents in return?

SCHULL: Not necessarily. It seems to me that if there are answers to be found to situations like the ones we have been discussing, they must be sought in the culture of the country, and possibly years of historical events which may have preceded the "affair." Japan is certainly a marvelous illustration of this; her reaction to the Fukuryu Maru incident is not to be explained in such simple terms as the response of a defeated nation to her defeator. Japan's image of herself

had been shaken by the war to an extent not shared by any other contemporary country save possibly Germany. This was a nation which prided itself on the fact that it had never been successfully invaded, had never lost a war. The Japanese had seen themselves as strong, virile, and so on; whereas in the post-war years they had been constantly groping toward some form of national identity. The Peace Treaty had, of course, been signed somewhat less than three years before the Lucky Dragon affair, but to the average Japanese the change in Japan's status could hardly have been apparent. There were still as many foreign troops in Japan in 1954 as there had probably been at the height of the Occupation. It was a staging area for Korea, it will be recalled. Most Japanese did not have then (and quite possibly still do not have) a strong and positive image of themselves as a nation, and more than one observer has felt that they lack self-assurance in their dealing with others. I believe that some of this is visible in Japan's foreign policy. Certainly she does not play the role in foreign affairs today that she should in view of the size of her population, her wealth, and industrial power. Japan is still hiding behind the skirts of the United States to a considerable extent. This may be a convenient stance, but it is hardly a worthy one.

To try to put the remarks of the last several minutes into some focus we might ask ourselves what the reaction in France, say, would have been to an event such as the recent one in Spain. And would knowledge of the Japanese and Spanish responses be of much help to us in predicting those of the French, or would we have to seek all of the answers in the French and French culture?

FREMONT-SMITH: Right. Very nice statement because I think one of the things we are weakest on is seeking answers in cultures.

LANGHAM: I wholeheartedly agree with that. I am convinced that if this had happened not far away in France, we would be on our knees in front of de Gaulle, even right now. I think what is found in the culture as well as the national philosophy of these places is important. The Spaniard is a person of great pride. I think probably part of their failure to make the progress they desire is that they can rely on this great pride and do so perhaps too much. I think you'll find that Spain is changing and I think you're going to find Spain bidding once more to become somebody in the family of nations. It's coming slowly but definitely; they are progressing and tourism is one of their great commodities now. It's absolutely impossible to get tourist accomodations during the season in the vicinity of Madrid.

They are developing this as one of their commodities, so to speak. It's why Palomares was a rather important factor; the Vice-President of the country, once he heard that there was no real health problem, next asked, "Are you going to find that lost bomb?"

SCHULL: There's at least one other important difference between the Japanese and the Spanish situations which has not as yet been mentioned, and that is the racial overtones which can be read into the former. In Spain, both of the nations which were involved were Caucasian, but in the case of the Fukuryu Maru it was white against yellow in the minds of some. It may have been solely fortuitous, but it was at about this time that I recall encountering the first of what has since become a much larger number of places in Tokyo catering only to Japanese.

ROOT: I think that's corroborated by the lack of furor when the Chinese dropped their bomb. I was being taken around and introduced by top Japanese science writers who had been our guests at the Overseas Press Club the previous year and who were handsomely returning the courtesy. This established a rather special relationship. I began increasingly to get anti-American sentiments, arguments from the press and from some of the professors, officials and doctors. Just at that time the Chinese bomb was exploded and I felt a distinct, though concealed, elation behind the expressed fears of fallout. That definitely was a racial thing, I think.

FREMONT-SMITH: The notion that another yellow race had gotten a bomb?

ROOT: Right. A really great identification exists with China. Actually a basic bond exists and is recognized, despite all the unhappy years of the past.

MILLET: It seems to me there is a renascence of pride in Japan. I think there has been tremendous pride in the culture of Japan in the past. Think of their walking out of the League of Nations meeting, for example, in the militaristic days. It would seem to me rather that the state of Germany is one in which there is a complete lack of identification at the present time. What was once a single nation is now split into two nations. They don't know how they can get back into one. That's a genuine fear of loss of identity, I think, there. I don't think there's any fear of loss of identity among the Japanese as far as I know.

SCHULL: I believe that you are wrong, but I will admit that the loss of identity is of a different kind than one sees in Europe. It seems to me that there is more of an isolation of the young people in Japan from their elders, on a relative scale, than there is even in our own country where, as we all know, there has been a substantial rejection of the values of former generations by the youth of today.

MILLET: Is that the same as identity? Would you say that we have no identity as Americans?

SCHULL: Perhaps I'm using the word identity in a different sense from that of psychiatrists, but a nation's image is not apt to be created by its youth, and in Japan now, as in many other lands, the young are not identifying with their elders who create the image.

It is almost foolhardy on my part to attempt to describe the directions that Japanese youths are apt to take. They pick up one fad after another; I'm sure they even have hippies now. But the role of students in Japan in the years since the war has been a particularly interesting development. The Communist movement on the campus, for example, ranges from the shuryū or "main stream" Zengakuren to lesser and lesser streams, some of which even find the Chinese much too liberal. These latter extremists have isolated themselves from all of the Communist currents except their own. Unlike the prewar years, the Emperor doesn't seem to be a focus of student interest nor even concern, although the younger members of the royal family often are, albeit in a somewhat romantic way.

About two years ago we lived on a small island in the western part of Kyushu known as Hirado. It has a population of about 40,000 individuals, and we were the only Caucasians. We managed to establish reasonably close relations with several of the families and could observe some of the emotional difficulties their college-age children were experiencing. Many were torn between the needs of a modern, rapidly paced society and the traditional ways of their parents. They were attempting to retain some sense of parent-child relationship but often could not bring themselves to actually accept the values which motivated their parents. As a consequence, they are confused, and perhaps some of this confusion is reflected in their extremely high rate of suicide—almost a third of all of Japan's suicides involve individuals of ages 20 to 29, and a surprising number involve teenagers, that is 15-19 year-olds.

MILLET: How much need to migrate is there in Japan? How much desire to live in other countries and become citizens of other countries?

SCHULL: The thrust today certainly is toward the big cities, but there is some migration elsewhere. There has been recruitment throughout Japan of people, principally farmers, to go to Brazil, Colombia, and several other countries in South America; the quotas are usually over-subscribed. But the major movement, as I've said, is to the large cities, and particularly those with newly developing industrial areas, notably light industry. Hirado, for example, between the national census of 1960 and our census in 1964 had lost about 10 percent of its population. It is mostly the young people who are leaving the island; they find the urban areas more attractive or at least offering more opportunities. Since the number of farms on Hirado has not diminished as a result of this migration, it is obviously no longer essential for the young to remain on the land. Presumably this, in turn, reflects changing agricultural practices, and especially increasing mechanization. The fact that the young can leave without necessarily impairing the productivity of the family farm, coupled with the active recruitment of labor in the rural areas by a number of Japan's industries, contributes to the urge to move. Recent migration from Hirado has been chiefly to Fukuoka, the nearest city of a half a million inhabitants or more, to Osaka, to Nagoya, and to Tokyo.

It seems to me, to return to the major issue before us, that the Japanese experience with and attitude toward ionizing radiation is a hazardous base from which to extrapolate to what might happen in Spain or most any other country. However, careful study of the reactions of Japan can make an extremely important contribution to the methodology of evaluation, and particularly to the task of appraisal of the role of a complex cultural fabric in the overall response to a "nuclear happening."

FREMONT-SMITH: Did you feel that the alienation of the young people had started several years earlier in Japan before it became evident over here?

SCHULL: That's my impression, yes. At a time, let's say, when our students at the University of Michigan were still primarily interested in panty raids, the Japanese students had begun to be more active politically, but then they have a long tradition of political activity which doesn't exist in the United States.

ROOT: Think of the students who protested Eisenhower's visit. That was pretty far back. They overturned Jim Haggerty's automobile and demonstrated so violently at the airport that it was considered inadvisable for the President of the United States to visit Japan; this is in great contrast to the classic image of the Japanese that we had.

MILLER: Merril, Dr. Langham has said that there was a leader who could issue a statement which pacified the people. You said that you talked to Dr. Tsuzuki because he was the leader, and he wasn't leader enough. In retrospect to whom could you have talked to to obtain more effective results?

EISENBUD: I didn't talk to Dr. Tsuzuki because I thought he was the head man. He clearly wasn't. By the time I arrived, there had already been constituted a committee which was headed up by Kobayashi, who was, as I recall, a microbiologist and statistician from the National Institute of Health. There were two physicians on the committee. There was the head of Toyko Hospital, whose name escapes me, and Dr. Maki Asumi, the radiologist, and the others were geneticists and physicists and marine biologists. It was agreed between our Embassy and the Japanese Foreign Office that all communications to the people would be through this committee. This would have worked all right. We stuck to our part of the bargain, which was made so easy that later on when we wanted to hold press conferences even the Ambassador was not permitted to hold one. But while we were coming to agreement as to what the facts were, the individual Japanese scientists were going out on their own and vying for public attention, and Tsuzuki in particular, who was not a member of the committee, was using his very prestigious position in Japan to get to the press. There was just no way that it could be done because this was obviously something that was going on, which I never understood, between Tsuzuki and the rest of the medical community in Japan. He finally left Japan and went to Geneva in the middle of the furor, for which he was criticized.

MILLER: But the reason for containing unjustified fears, or even justified fears, was that there was someone to reassure the people in Spain and there was no one to reassure them in Japan. Apparently the situation was out of control and could not possibly have been brought under control under any circumstances, even in retrospect.

EISENBUD: There are some things that a man with political sensitivity just can't say. Just like during the Korean war, if Truman

had tried to settle the Korean war on the terms it was finally settled on, I think he would have been impeached, but I think Eisenhower looked very good at the time. It was the right time to say something.

FREMONT-SMITH: Politically the right time.

EISENBUD: Yes, and I think that if Kobayashi, for example, had tried to make a statement at that time which was reassuring, they would have found another chairman for the committee. It's as simple as that. Now, who they are, I don't know, it might have been the Foreign Office.

MILLER: What I was trying to get around to is, what happens if there is another such incident, either here or someplace else? It would seem to me that one rule of thumb would be to try to get someone who can reassure the people as to what the real circumstances are. Is this not right? Is this not the big difference between the reactions in Spain in Japan?

EISENBUD: That's right. That's why I think it's important in the nuclear field that we maintain good contacts with our counterparts overseas, and there are innumerable instances where potential difficulties have been aborted by mere letters or short visits either from government to government or by representatives. I could enumerate half a dozen. But in Japan there was no organization. Japanese science at that point was a pretty amorphous structure. You didn't have an Atomic Energy Commission. Dr. Tsukamoto—I don't know where he was in those days. He is now the head of the biological part of the AEC over there and if this incident came up, he would be the man they would listen to, but I don't know. There was no such person in those days.

FREMONT-SMITH: Isn't the implication of Dr. Miller's question, which I think is a very good one, that the State Department should have a very detailed study of cultural anthropology of the cultures of all the different countries and make this a primary concern of the State Department? They have done this to some extent, but not really. Part of the difficulty has been that our cultural attachés all over the world are isolated in the embassies and in the little enclaves and do not move with the people. We don't have a suggestion that had been made at a conference for the State Department back in 1946 and 1947, that there should be a systematic effort to put students in cultural anthropology. These students would be writing theses, doing field

work in association with embassies in different countries but also living among the people and with a liaison to the State Department, both to their university and back to the local embassy. Hopefully, there would be a constant feedback of cultural understanding which would flow back to the university and the State Department. These students would then be good candidates for cultural attachés some years later. Actually, I believe the Foreign Service Institute does make some effort to give some cultural anthropology to the Foreign Service people, but in actuality the cultural attachés who are supposed to be the people to do this are, by and large, almost completely isolated from the community in which they serve.

I think that the question raised is, in a very broad sense, if we are concerned with a variety of incidents, (and we are going to have incidents, not all nuclear, but we are going to have incidents with other countries all over the world), if we're going to meet these incidents appropriately, we've got to have a great deal of cultural insight with respect to every other country that we can bring to the fore. How do you meet this situation if it has to do with Thailand and their culture, which is going to be quite different from meeting it in Spain?

MILLER: Yes. I think that, for one thing, the adviser, an expert, might be able to indicate the person who can influence the people or can advise that there is no such leader, that the situation requires a second line of defense and what it should be.

FREMONT-SMITH: But at least there should be a current awareness of the cultural attitudes with respect to a variety of things in any country with which we have any dealings at all.

EISENBUD: It might be worth noting that shortly after that Japanese episode both the State Department and AEC had a scientific liaison in Toyko Embassy. Of course, this was done in other parts of the world as well. I don't know whether we have anybody over there now. I presume we do as a scientific attaché.

FREMONT-SMITH: Yet a scientific attaché is not a cultural anthropologist. This is a different story. He'll be an expert in physics, you see, or possibly in biochemistry; in the social sciences I think they are very, very rare. I think we had one in India and a couple of other places for a short time and then this was caput. But the concept of using social science insights and especially cultural anthropology, which I think ought to be one of the key ones, I don't think it has penetrated.

WARREN: You are aware of the upset in the anthropologists association, aren't you, about their being used as tools by the CIA?

FREMONT-SMITH: Yes, I know, and the story that was in Peru, what was it called, Camelot, which raised an awful mess. But there was also not a great deal of wisdom used, I would think.

WARREN: That's right.

BRUES: You mean that even the cultural anthropologists can have a colonial attitude when they go somewhere?

WARREN: I think the anthropologists, too, have calmed down about this. It wasn't quite as bad as they thought at first.

SCHULL: To return to Wright's case, it appears as though in Spain all of the unpredictable elements contrived to get together in a very happy sort of way. In Japan, exactly the opposite seems to have occurred. The one organization which could conceivably have made the statement Merril suggested was the National Institute of Health; the National Institute of Radiological Sciences was not yet in existence. But even if Dr. Kobayashi had made a statement and a forceful one, it's questionable whether it would have had a significant effect upon the Japanese public. The National Institute of Health of Japan, though established in 1939, was more closely identified with the occupation than many other groups, and I, for one, am not convinced that it had either the stature or sufficient public acceptance to stem the tide even if so motivated.

EISENBUD: The medical schools were under the Ministry of Education, the hospitals were under the Ministry of Welfare, and they were jockeying between the politicians in those two groups. It was a mess.

DUNHAM: Frank, you made a statement to the effect that the cultural attachés were isolated. Is this by job or simply by the type of people that had been appointed?

DUNHAM: I've seen this happen to science attachés.

FREMONT-SMITH: I get the impression that the whole embassy group, the cultural attachés and the science attachés, all live together, all speak English. They live in special housing arrangements for

them. I think this was true in Germany. They are not systematically organized to live with the local people or even talk their language. and this is talked about a lot, this isolation. I'm not in a position to know that it is true, but I have no reason to believe it isn't true. The children go to American schools to a large extent, that are set up especially for them. So I think that there is a failure to take advantage of the opportunity, and I believe that this has been pointed out to be quite in contrast to what the Soviet Union does; they send their people over to roll up their sleeves and speak the language and mix with the people and live at the level of the people. It would be very difficult for us to get Americans to go over there and live at the level of the community in which they are supposedly working.

DUNHAM: On the other hand, the British charge d'affaires in Peking conducted a seminar in Washington ten years ago when he came back, before he went to Harvard to do some special studies, and he pointed out that the Russians had isolated themselves from the people and they were not allowing their children to associate with the Chinese children. So their approach is not uniform across the board.

FREMONT-SMITH: That's a comfort.

DUNHAM: I think these things are uneven and a lot reflects, I think, the personality of the people involved. I know of a science attaché, and I won't say what country he was in, who almost deliberately isolated himself from the scientific community and expected it ought to come to him. If you have a cultural attaché of that type, he isn't going to learn anything. Even if he doesn't know the language, he should be outgoing.

FREMONT-SMITH: But there had been a policy here at the State Department with respect to this in order to encourage, in every possible way, a relationship of these particular attachés to the community.

TAYLOR: This is apropos of nuclear accidents or what?

FREMONT-SMITH: I'm talking apropos of international relations of which nuclear accident is only one. We spoke of what we would do in the future if we had an incident in France and I'm raising the issue, what would we do in the future with any kind of incident? We are bound to have conflict as we are having many right today. We are bound to have conflicts with nations, and the way to deal with these conflicts is to know as much as possible, at least, about the culture and the attitude and the mood of the people and not to be insensible.

EISENBUD: We've had many incidents of many kinds, mostly of considerably less severity in terms of hurt, but potentially of sensitivity, equal sensitivity in relation to people, and there was a period in the late 1950's when there was worldwide concern about fallout and the subject came up before parliaments all over the world. I had a number of opportunities to visit capitols on short notice, perhaps 10 or 15 of them around the world, to meet, and I found that the guidance that I was getting from the State Department was good, and I think it was good in Japan. I spent, I guess, 9 or 10 weeks there and I've had many opportunities over the years to just reminisce with Japanese friends now about this incident, and I've thought about it a great deal. I really can't think of a single bad lead that they gave me. I think that their appraisals of the people I would have to deal with were good. I think they seemed to have a very good understanding of the Japanese culture. A number of them had been there before the war and a number of them had learned the language and some of the senior people did live in the Japanese community.

SCHULL: I would like to support Merril in that general statement. Japan has been one of the few major embassies to which we've tried consistently to appoint professionals witnessed by the fact that all of our recent ambassadors to Japan have spoken Japanese.

EISENBUD: Reischauer has a Japanese wife.

FREMONT-SMITH: Isn't this somewhat of an exception, one of the few; right?

SCHULL: We even have been fortunate to have Japanese-speaking science attachés. I can think of one in particular, Otto Laporte, who is a physicist of competence, and probably one of the very few in the United States who speaks Japanese well enough to communicate effectively in that language. At the social sciences level we've had a procession of outstanding people. The competence was there on the State Department side, in my opinion.

BUSTAD: Isn't our criticism, Merril directed at the fact that the State Department expert in Japan should have been allowed to speak out?

EISENBUD: I think that if he had been allowed to work out his arrangements with the Prime Minister then—who incidentally told us he recalled saying, "Mr. Ambassador, it was you folks who thought

we ought to have a free press," when we were complaining about what the press was saying, which was a very good point! [Laughter] This was their first experience with a free press, at least their first decade of experience. I think that Katayama and Allison could have worked out an agreement which would have nipped this in the bud within the first few days and I think that it would literally have bought the good will of everybody from the fishermen on to the rest.

FREMONT-SMITH: But it was the State Department policy that prevented this?

EISENBUD: I can't speak for that.

FREMONT-SMITH: I assume it was.

EISENBUD: But I do know that there did seem to be the kind of latitude in the field that was required in order to work out the arrangements, presumably.

BUSTAD: I vote for decentralization! [Laughter]

WOLFE: Wright, when you were in Spain and you had to make the decisions, did you have to go to the Ambassador and then to Washington, and then all the way back before you decided to plow or not to plow or something like that?

LANGHAM: No. Insofar as those decisions to do things immediately were concerned, these were made by General Wilson, head of the 16th Air Force in whose territory this thing had occurred, and in dealing with him you begin to realize why he is a general. He certainly made decisions, and his way of making a decision was to get the people around him that he thought could advise him, listen to them, and when they were through talking he made the decision. That was the experience the first week in the field.

The second time I went back I was assigned to the American Embassy. Now, you found here that decisions had to be checked all the way back through Washington. I think if there is one thing that surprises me it's how dependent on Washington the Embassy seems to be when it starts to make a decision, and yet Mr. Duke was a highly respected man among the Spanish. As far as I know decisions must be stamped in Washington before action is taken. I just got the idea that there was too much centralization of opinion. In other words,

does it do you a great deal of good to have a fine man in the field that's respected if you give him no authority to do anything? And I rather sensed this. Now, I could be wrong. The thing that set me off on this was when Miss Root remarked about it, and this was really just what was bothering me when I was there. It seemed that there was a rather cumbersome chain of command insofar as the American Embassy was concerned in a decision-making way and I rather gathered, Merril, that you had said about the same thing.

EISENBUD: Yes. It was ridiculous. For example, if I wanted to send a cable to John Bugher just telling him that I was going to remain another week, this was a communication from the Ambassador to Secretary Dulles.

WOLFE: You don't just send one with a carbon copy?

EISENBUD: No.

TAYLOR: Isn't it true that every communication today from the State Department to an overseas post is from the Secretary of State, signed "Rusk"?

EISENBUD: It was when I was there.

TAYLOR: Every communication, even a transfer of a clerk from one office to another.

SPEAR: You can always look down at the lower left and find out who it really came from, but it's signed "Rusk."

TAYLOR: Why go through this charade, or whatever it was?

LANGHAM: I never sent a message. All of my messages were sent by Mr. Duke. Evidently that's his job.

WARREN: I can see a certain reason for this administratively. The Ambassador is playing the hand of the President, really, in his international relationships. So there should be appropriate consultation. But something should be allowed to the Ambassador for the use of his judgment in the situations. The trouble is that the minute it's a nuclear power, a sort of paralysis goes over everybody and particularly those who are not scientists and are politicians or people in the administrative hierarchy who are unfamiliar with the situation;

they just didn't dare move, and I imagine the President's office called up Mr. Seaborg and he was consulted on the question all the time, and, of course, the Department of Defense had to be consulted. So they had a small Cabinet meeting about this, and this took a long time. Not that I'm in favor of a complete block of responsibility; I am not. I think there's a time and a place for it and the local man ought to have enough sensitivity to his situation to be allowed to meet what is really an emergency situation. Now, if his judgment turns out to be wrong, then he should be jerked home and he does it at his own peril, but a good man knows where the perils are and what the goals are. Isn't that a beautiful thought! [Laughter] It just doesn't work out quite this way.

CONARD: I feel like I've been sitting in a State Department briefing! [Laughter] I wonder really how relevant some of this stuff is to nuclear warfare and the long-range effects? We've laid an awful lot of stress on incidents that have occurred in foreign countries and how we might handle those in the future. But what about what would happen in this country as an aftermath of the war and the psychosocial reactions here? I think that's the real point we have to get at.

SESSION VI
PSYCHOSOCIAL REACTIONS
(Initiator: Merril Eisenbud)

SESSION VI
PSYCHOSOCIAL REACTIONS

INTRODUCTION

EISENBUD: The interest in this morning's subject is made obvious by the way it has pervaded the discussion of the past two days. I was very happy when I learned that our discussion this morning would not, of necessity, be limited to the psychosocial implications of the three events that we have discussed so far in this series, namely, the two Japanese bombings, the Fukuryu Maru incident and the Marshallese incident, and the Spanish incident. Instead, we will try to roam the whole field of the psychosocial implications of nuclear warfare. I think it may perhaps be worth a moment to explore the meaning of at least the social part of "psychosocial."

I would consider this to include all aspects of man's social structure including economic, cultural, theological and any other of the "ologies" that may go into the society that he has constructed. I suggest that we look at these questions in three phases. I can think of designations for the first and third. I think of the first phase as the prologue, that portion of the discussion that would be concerned with what happens before war comes and the ways in which the war can be prevented or its effects mitigated. The third is the epilogue, being what the effects would be if the war came. But, for some reason, I couldn't think of what the middle section would be called. However, through my learned associates, namely John Wolfe and Austin Brues, I was made to realize that what happens in the middle is the logue, which had never occurred to me before! [Laughter] So, we will think of the prologue, the logue and the epilogue.

FREMONT-SMITH: The "logue" is a word.

ROOT: Yes. In the beginning was the "logue!" [Laughter]

EISENBUD: Let's go back in time, let's say before 1400. We can go back as far as you like and then go forward on to, say, infinity, but we are really talking about the next century or so, perhaps. This

is time, and I think we need some measure of progress; you could say that we can just take an arbitrary unit of man's feeling of well-being, of gross national product, or anything you want to take. There certainly has been a general increase, with it's ups and downs, which has perhaps been an exponential increase.

FREMONT-SMITH: A general increase in something.

EISENBUD: Something, whether you measure population, gross national product, well-being or leisure time in technically advanced countries, those countries that are going to be involved in the logue.

DUNHAM: Directly.

FREMONT-SMITH: Something is going up.

EISENBUD: Then something happens. What we have been talking about now is "pro." We start the logue with an all-out nuclear war which suddenly throws the society back, and I would certainly think that one of the things we would want to debate, when we get to this point, is how far it is going to decline. Is it going to retreat to where we were in 1400 or just to 1800, or will it go back 10,000 years? This is an important question of a social nature.

Equally important is the epilogue. In the epilogue the society continues to retrogress because the recovery mechanism fails to operate. There is a period of leveling off followed by the recovery of progress. We are interested in the rate of decline and the rate of recovery, and we can speculate on what happens as the epilogue continues through history.

We will bear in mind that it is this dive and the rate of recovery that are perhaps the subjects of most speculation and, I think, probably the aspects of the whole story about which we can do the least at this moment, in contrast with the prologue, where perhaps there are some things that we can do because we are all participating in one way or another in the efforts to forestall the "logue," to put it off to infinity. If we push it off to infinity, it doesn't happen.

ATTITUDES TOWARD NUCLEAR WARFARE AND DEFENSE

EISENBUD: There's been a good deal written on this subject, and some of you—notably Bob Ayres—have written extensively on the

psychosocial implications of nuclear war. The study that has impressed me most is one which I think might have been required reading for this conference. It is entitled Nuclear Disaster (Reference 50), by Tom Stonier, a rather unusual individual who, while he was on the microbiology staff at the Rockefeller University, was encouraged by his associates in a small group called "The Scientists' Committee of New York" to make a review of the whole subject of nuclear war and its social implications. He came out with a report which many of us were impressed with, although there are many aspects that many of us didn't agree with. It was ultimately published. I think he has attempted to analyze the effect of nuclear war in documentary fashion and he has considered all of the physical and biological factors that would enter into an evaluation of what the effect of an all-out nuclear war would be.

FREMONT-SMITH: It seems to me that during the time that he was writing this there was much more active interest and a good deal of writing going on and general activity about this. I am wondering what happened. Interest fell off. Did everybody concerned get tired and frustrated and give up because nobody seemed to be able to do anything about it? I don't know. I haven't seen anything. Has Stonier done anything more recently?

EISENBUD: I don't believe so. I think you told me, didn't you, Miss Root, that you had seen him last week?

ROOT: Yes. I saw him and he is just as interested but, this being a specific study on what would happen to New York City if a megaton-range bomb were dropped, he finished that project. I don't know whether he is writing another book, but he publishes articles and arranges lectures.

EISENBUD: I know he has done one thing which is very interesting. He is now over in Manhattan College and is a professor in the Biology Department. He has established what he calls a "pacem in terrem," peace on earth, institute. I don't know whether he's gotten the funds for it, but he's attempting to devise a greater curriculum toward peace. He feels that by the time students leave Manhattan College they should understand the implications of this. He's already held one 3-day seminar where he invited a number of people to present things to the undergraduates.

FREMONT-SMITH: So he's still active in the area.

TAYLOR: I think there's an important thing about his book and about what he was doing, at least two years ago. I think it's a fair statement to say that the book is essentially an anti-civil defense book; that the purpose of it is to decrease confidence in civil defense measures. The reason I'm saying that so emphatically is that there was a panel formed by the American Nuclear Society about two years ago to discuss civil defense (Reference 51). Eugene Wigner and I were on the side of civil defense and Stonier and someone in the Harvard Law School, whose name I've forgotten, were opposed to it. We had a very informative and worthwhile debate. He said that what he really has in mind in his writing now is to display the futility of civil defense. I think that's important because I think he would be the first to agree that he feels very strongly about this and gets emotionally involved in illustrating his point, namely, that the disaster, no matter what we do, will be so complete that we should not do anything which will indicate that people could get away with a nuclear war. I think that's his thesis.

FREMONT-SMITH: Isn't this his point, that one thing to do is to prevent it and that there is no use in trying to comfort ourselves by feeling that we can save ourselves with civil defense?

TAYLOR: I think his thesis is that if we fail to prevent nuclear war, all is lost.

AYRES: The one point where, in the last analysis, he thinks this system would fail is in the psychosocial realm.

DUNHAM: Let me back up to Frank's question. He asked why nobody's writing books on the subject anymore. I looked at the date of Stonier's book and it was copyrighted in 1963. Since then there has been no major nuclear testing to keep people stirred up. The fallout, even the rate of fallout, is such that you are actually getting a decrease in fission products on the ground now, and the Vietnam thing has overshadowed everything.

ROOT: I think, too, maybe the detente is a barometer of how sensitive people are to the overall situation. This great change really runs current to political detente. People are so hopeful it will continue, they would rather focus on that than on disaster.

FREMONT-SMITH: Are we relaxing over the fact that China has entered into the conflict with Russia?

ROOT: Yes, consequently Russia seems to be more approachable.

DUNHAM: Another commentary on the general attitude right now is that I was invited by Senator Clark, a member of a group on the Hill, Peace Through Law I think they call themselves, to view and to comment and to ask questions, first for the Senate members and then for the House members, on a movie entitled "War Games." The BBC at one time was not allowed to show it on BBC but it did go the rounds; and it was interesting. Only one other Senator showed up and practically no Congressmen showed up. It was almost exclusively staff of both groups. It was at a favorable time; it was late in the afternoon. There were no interruptions for voting or anything like that. But this shows the lack of interest.

DUNHAM: They were preoccupied with other matters.

FREMONT-SMITH: I had a feeling as you were talking that perhaps something had happened to us psychosocially in this respect.

SPEAR: I think also that the tone from the White House has something to do with it. In the early years of the Kennedy Administration there evolved quite clearly the attitude that nuclear war simply must not be permitted to happen; that it is unthinkable. This was supported by the outcome of the Cuban missile crisis. I think this has been an important thing which has been reflected, as far as I can see from the outside, in program emphasis within civil defense. I haven't been close to it, but lately there certainly has not been much impact of the fallout shelter program on the public. Kennedy gave this a great impetus and then very quickly regretted having done so, I believe. This is bound to be reflected in the public attitude.

FREMONT-SMITH: It seems to me that there's a legitimate conflict here. On the one hand, a strong point can be made for the fact that civil defense, appropriate civil defense, would save a great many lives; on the other hand, one can also make a strong case that any civil defense, appropriate or otherwise, may increase the danger of a war because people will feel that they don't have to avoid it because they have civil defense.

It seems to me that these two positions are somewhat in conflict in a way. One might say if there's going to have to be a war, of course, we want civil defense; on the other hand, if a war is going to be so bad that it really needs to be prevented, and if not having

civil defense and showing how bad the war is going to be might help prevent a war, then maybe this is the way the orientation should be.

EISENBUD: I wonder if it wouldn't be worthwhile, for the purposes of documentation, to go around the room to see what other studies have been undertaken and by whom. I know, Bob, that you have one I've seen which is very good. I don't remember the exact title of it, do you want to put it into the record?

AYRES: I don't remember what we used to call it. Its present title is Environmental Effects of Nuclear Weapons (Reference 52).

EISENBUD: There are a few RAND reports. The one that I think may be most applicable to this discussion is one I've seen by Harold Mitchell on either the biological or medical or ecological effects of nuclear war. Do you remember the exact title?

DUNHAM: There are about twenty of these and they are available in non-classified form. I don't know how many copies were run off, but they are available. What you are referring to is the one where they took a look at the plague and black death. I think that was strictly related to looking at the black death, how people reacted to it and how fast the population came back after realizing that it decimated many millions of people.

EISENBUD: There was one that was more general than that.

DUNHAM: That was H. H. Mitchell's Survey of the Infectious Disease Problem as It Relates to the Post-Attack Environment (Reference 53). I am talking about the more recent ones, within the last 6 or 8 months.

EISENBUD: I haven't seen one.

FREMONT-SMITH: Could we ask each of you, as the transcript comes to you, to put them in the record? I think it would be very valuable to have this bibliography in the record, and we do this rather systematically anyway.

EISENBUD: I would like, if you would, to confine ourselves for a while to the prologue phase.

ROOT: Dr. Ayres, you were not referring to the publication that Hudson calls The Year 2000?

AYRES: Good heavens, no!

DUNHAM: It might be! [Laughter]

ROOT: Then let me throw that into the hopper. Hudson Institute has a fat volume, called The Year 2000 (Reference 54) coming out in a couple of months which also deals with what there will be left. It had a section on nuclear war when I saw it in manuscript form. Also limited wars. It has a lot of scenarios but it also talks about limited warfare and limited nuclear weapons.

MILLET: If I may speak to the prologue for a minute, we started a series of round tables at the American Psychiatric on transcultural reactions of various sorts, and the first one we had was on civilian tensions in the atomic age. This was dated 1961. It has never been published. It contained rather an interesting conglomeration of ideas on the subject. Perhaps it might be timely to have that copied and sent around.

FREMONT-SMITH: It would be fine if you could get a copy to this group.

MILLET: I tried to get opinions from different people in different countries, and so on. We didn't get much out of anybody except the Americans, and from people who had immigrated to America from different countries, India and Holland, for example.

EISENBUD: The group that could be most influential in pushing that point, nuclear war on a large scale, off until infinity, of course, would be the decision-makers of our society, people that influence what is done. These are the people, mainly, that we elect to office, influenced by civilians of a variety of types, such as people from academic institutions, heads of large corporations, and religious leaders. I gathered from the discussion of the past two days that some of us think that perhaps the decision-makers in some cases haven't been well-informed and in other cases haven't been well-motivated.

I often think of something that Oppenheimer said in one of the last public appearances before he got sick, in which he pointed out that one of the greatest dangers is that man would go to nuclear war for trivial reasons because the consequences of the nuclear war wouldn't really be appreciated by the people that had to make the decision.

UPTON: Merril, could I interrupt, please? When you say decision-makers in our society, do you mean the world scene or the U.S.? Isn't it quite conceivable that the initiative will come from outside the United States?

EISENBUD: I'm thinking, of course, of the world scene, but the ones that we can influence are the ones in this country. I think the most impressive demonstration of ignorance at that level that I've seen occurred about 3 or 4 years ago. I can date it exactly as it was about the time that the DOD came out with their little yellow book or pamphlet on fallout shelters. It was at a time when there was a good deal of interest in the subject of civil defense and nuclear war generally. For almost a year I had had an invitation to address a very exclusive women's club in Washington that some of you may be familiar with. It is probably the most sophisticated group of women in the world. They call themselves something like World Forum or Women's World Forum, I don't remember exactly, but they are the wives of the cabinet members, senators, congressmen and diplomatic corps who are banded together for a monthly luncheon. They invite people to talk about something of interest to them at these monthly sessions and they asked me to talk about fallout. I talked about it in terms of nuclear war rather than the thing they were really interested in, i.e., what they should do about the strontium-90 in their children's milk. I was impressed, first of all, by the fact that although this yellow pamphlet had been widely publicized and had already been out for about a month, when I held it up out of curiosity to see if anyone knew what it was, no one in the audience knew what it was. From the questions that were asked from the audience, although these were the wives of the decision-makers, they really hadn't given the subject the depth of thought that you would expect women in that position to have done. I think that many of us have seen similar examples in the men themselves. I can cite many examples and I'm sure that many of you can, too. What can be done to upgrade the level of education of the people that have to make the decisions and the people that influence the decision-makers, namely, people like ourselves? I wonder if this wouldn't be worth some discussion. I think, Chuck, you've had a good deal of experience with this. You might have some views.

DUNHAM: Well, of course, I don't know that it's fair to take Cabinet members' wives as spending all their time preoccupied with high-level policy problems of state. They obviously hate to go to so many of these functions, including the Forun.. It's a prestige item to be a

member of this thing, and very often they do not participate. They don't have the time to participate; they do what your wife or my wife might be doing through the PTA and this sort of thing. So I think it's a little unfair to point this out as something fearful and wonderful. I think it's just natural that it would be there to a certain extent. I am amazed that not one of them had read the pamphlet, but I certainly wouldn't have expected many of them to have read it.

ROOT: I would like to know the nucleus in Congress that's interested. Perhaps that's a starting base.

DUNHAM: There's a group within Congress—I think it's a national organization—called Peace Through Law. I still can't recall the exact title of the group. Apparently Senator Clark is a principal member of it in the Senate. They look at the problems of avoiding war, and I gather their main approach is through legal means. I never saw a law stop an international conflagration, but maybe it could. However, that's beside the point. This movie produced by the BBC is about a nuclear war that takes place primarily in England. The attack is from the East and it's a very graphic presentation of, first, the disbelief and, then, the inadequacies of civil defense. The film then shows the public turning on their local leaders, which happens occassionally in peacetime, too. I thought it was very effective. Apparently Senator Clark saw it, too, and was impressed by it and thought it would be interesting to have his colleagues on the Hill see it. He got in touch with the Joint Committee on Atomic Energy which got in touch with us and asked if a couple of us would be there in case there were questions and comment. So he had two showings, one for the House and one for the Senate, one right after the other. There weren't more than two or three Congressmen there and I think he was the only Senator who showed up. It was a very vivid motion picture. It showed in local theaters, first in New York and it opened in Washington a day or two after I saw it; that's how we happened to have a print of it available. But it was forbidden for public showing on British TV because it was considered bad medicine.

ROOT: I think I know the organization in New York and I think if there is any interest here in seeing the film, we might be able to get it for the next meeting.

DUNHAM: It's a commercially available film. It was shown in the Penn Theater. I don't know where it was shown in New York.

CONARD: Were you able to get anything from the questions as to the depth of understanding of the situation?

DUNHAM: There were one or two staff members who felt very strongly and there was obviously one disciple of Stonier's, for instance. The idea just shows that you shouldn't even think in terms of Civil Defense. I think there were specific questions about how reliable such a statement is, etc.; it was generally quite accurate.

TAYLOR: It seems to me that it is very difficult to get dispassionate accounts of what might happen because there is a strong tendency to choose sides in this. There is a labelling of people. People who examine various types of Civil Defense measures and promote them tend to be in the hawkish column. And they tend to be for ABM, etc. This tendency for many of us to get polarized by statements about Civil Defense, is, I think, one of the reasons why it looks as though many people are not interested. I find that when I think someone is not interested in the subject, it's very often the case that he is not interested in what I have to say about the subject, but he has his own ideas. If he is in an argumentative mood, he will talk about them. If he's not, he acts as though he has not thought about it and is not willing to.

FREMONT-SMITH: Isn't this a very human factor, that people find it very uncomfortable to stay on the fence about anything that is important? I wish Jack would speak to this.

MILLET: We're stuck with the possibility of being at one end or the other of the spectrum; either "Yes, it's going to happen" or "No, it can't possibly happen," while in the middle are all the mugwumps. The mugwumps have the hardest time of all because that involves a lot of thinking and the use of imagination. I'm trying to come up with something new in the way of solution or at least something half way logical as far as one's experience can show it. It seems to me that that is fundamentally the situation here from a psychological standpoint. The same was true when we were trying to sort out the prospects for good government in Germany after the war. There were at one end the people who you knew would repeat the Nazi thing while at the other were people who couldn't possibly do it as far as we could tell. Then in the middle were those calling for judgment as to who could be trusted to handle this or that level of government . Fundamentally, the same psychological situation of obsessional horror at one end or the other existed, you see.

TAYLOR: I mentioned that because I think there is a difficulty in answering your question as to what one should do to make sure that the people who are charting our courses are well informed, because a definition of "good information" or "bad information" or lack of information is based so much on what one's own idea is of what these people should do.

EISENBUD: I think that the government has done a good job on the various editions of the Effects of Nuclear Weapons, which describes the technical facts, and certainly information of this form should be useful to people that have the intellect of our decision-makers. They should not have to read it second hand, they should not have to trust taking it out of the newspapers or out of the polarized journals.

FREMONT-SMITH: Which information is this that's available?

EISENBUD: There are several editions, three or four, of a volume that was first put out in 1947. At that time it was called The Effects of Atomic Weapons, and is now The Effects of Nuclear Weapons. It's several hundred pages thick and is put out jointly by the DOD and AEC. It's a fairly authoritative and grim account of the technical facts.

FREMONT-SMITH: Yes. But, wouldn't it be based on certain assumptions, either that you had a very big or that you had a smaller war? It wouldn't give all of the possible alternatives that might happen.

EISENBUD: No. It doesn't go into the overall consequences but it does tell you that if you explode a 20-megaton bomb, the blast damage will kill people x-thousand yards away, you see, and from that you get the picture. Then following that, in addition to that, there was a very thorough examination of the subject in some hearings of the Joint Committee on Atomic Energy in 1958. At these hearings many of the people around the table here took the opportunity to go to Washington to give their point of view within their specialties on just what the implications of nuclear war would be. So there is a large body of information that is available.

I must say that I think that one of the problems is that there has been a cultural change in this country which makes any discussion about anything that's unpleasant unpopular at any dinner party or cocktail party. When I was a child, my recollection of the house

parties my folks would give on a Saturday night was that they would generally break up into some pretty hot arguments. We don't see that any more. The unpleasant subjects are dodged. People don't like to take issue. I think we are much more polite than people were a generation or two ago in discussion with each other and I think also we tend not to show our cards. This, I think, may be the result of some of the polarization that took place, for example, during the McCarthy period where people weren't willing to say, at least they would like to avoid saying, where they stood on anything because they didn't know what the meaning of this might be, you see, in the way things were going. But, Chuck, and this is one of the few times I find myself in disagreement with you, I think that a group of women generally will reflect what their husbands are interested in, and if these men were obsessed with the importance of this subject in the way I think they should be, their wives would certainly know more about it. To me this was a very significant experience.

TAYLOR: I think you put your finger on the reason. Their husbands were not interested in the subject.

FREMONT-SMITH: Exactly.

EISENBUD: That's the point I was trying to make. I'm sorry I didn't make it originally.

DE BOER: The documents you are talking about are well-known and reasonably good. In fact, in some ways they are exceptionally good, but they also have their faults. For one thing, they are not updated. So many people, when asked to support current research and who have read those documents, will say, "Were not those problems answered in document so and so, published in the late forties and early fifties?" As a relative newcomer in this field of radiation biology, I am often called upon to define our current programs. Having done so with a great deal of zeal and enthusiasm, I am often asked, "Well, wasn't all that done somewhere around 1946? Don't you people ever give up? Do you always need more dollars?" Talk about discouragement. Talk about ignorance. Talk about what still needs to be done in all fields. This is part of the struggle which faces us because not all is answered, nor will it ever be. Yes, those are good documents, no doubt about it, but I think we are foolish if we try to tell ourselves that all the questions were answered in those documents.

AYRES: I must say that I agree with that.

SPEAR: I can support your point here by an incident that was rather shocking to me at the time. The under-secretary of one of the major departments of government, who shall be nameless, one who had been more than once to the test site in Nevada, who had been briefed many times on the effects of nuclear weapons in the latter part of the fifties, asked the administrator of Civil Defense to have someone come down and explain to him what was involved in the fallout shelter proposal, that is, what were the facts on shielding, on the strength of gamma radiation, and so on. I got tagged with the job. I was a little surprised because I felt that at this point in time he should have had some understanding of this. At any rate, I went down armed with the effects of nuclear weapons data, the test data, the attenuation factors involved, and for about twenty minutes held forth on the basic facts. When I finished he shook his head and said "Well, I'm afraid I couldn't go for this. I have a little trouble with claustrophobia and I think I would rather seek protection, even if it is hot and uncomfortable, in some of those yellow coveralls such as see some of them wearing out at the test site in Nevada!"

It was utterly shattering to me that I failed to get across the essential point there. This is a man who was very high up and very much involved in the total decision-making process and subsequently was to go higher in the pecking order.

FREMONT-SMITH: We should choose decision-makers who don't have claustrophobia!

SPEAR: It was to me a most shocking experience to realize that at that level in our government there was someone who was so completely innocent of any pertinent knowledge on a question like this.

UPTON: I live in a town where people, by and large, understand radiation pretty well, and we have a number of shelters in the plant areas as well as in the community. From time to time there are tests of these shelters; Boy Scouts, as a rule, are selected to spend a weekend in the shelter, and so on. I can't name a single acquaintance of mine who has a shelter in his basement although there are in the town several home shelters that are very extensive. I've heard about them; I've never seen one.

I think the prevailing attitude is one of a sense of hopelessness; it's a feeling that the problem is so tremendous that one trusts that the need for shelters will never arise; that if it should arise the extent

of devastation and the dislocation that would be associated with the use of this shelter would be so great that one might just as well trust to luck as to try to take any constructive preventive action. This is a very discouraging thing. I think in general that everyone at Oak Ridge hopes somehow that the decision-makers will avert the calamity, although one recognizes that it may not be up to Washington in the last analysis to do so. So I think that this points up the feeling of hopelessness and a feeling that the individual really can exercise no meaningful co[illegible]ctive action that may prevail throughout the country. I re[illegible] believe that people are so ignorant of the dimensions of t[illegible]. I prefer to think that instead they don't see any const[illegible]ourse which they can take.

DE BOER: Are you talking about yourself now or are you talking about the community you live in?

UPTON: I'm talking about the community and about people in general with whom I've discussed the problem.

ROOT: How many people here have shelters?

FREMONT-SMITH: What?

ROOT: Does anybody here have a shelter?

LANGHAM: I have.

EISENBUD: I would quite agree with you except in one respect. You said that these people who give up any hope of solution beyond this point hope that their decision-makers will prevent the war in the prologue and, of course, this is what we are addressing ourselves to. But I think there have been a couple of stripes running through the discussion in the last two days that are very worrisome. I think somebody raised somewhere along the line the question of the motivation of the politicians. I think it's very depressing that many of the columnists, as they talk about the Vietnam war, more or less assume that there's going to be some change in national policy because there's an election coming and that the positions that the politicians are going to take in the national election, on Vietnam, are going to be influenced by the fact that they want to be re-elected. I think this is terrible.

MILLET: There's a lot of evidence for this view.

FREMONT-SMITH: But, you see, they say the only way they can serve the country, which they know they can do, is to get re-elected, so the most important thing to save the world is to get re-elected. I think this is a perfectly understandable rationalization to justify almost any change of policy or any change at all that will get them re-elected. Then they can really make the decisions, at least until the next election looms up! [Laughter]

I'd like to go back to the earlier situation. I live on Long Island and I work in New York City, and there are a great many other people who move in and out of New York City every day who leave their families at home in the suburbs and go into New York City and then go back to their families. Well, to face what happens in a nuclear war for people like myself is extremely difficult because you might be on the train, you might be at home where there's no possibility of shelter because Long Island is so flat, at least on the South Shore where I live, and, on the otherhand, if you get into New York, you've left your family and children out there where they have no hope and you have no hope of seeing them again. It is so ghastly to contemplate what you would, in fact, be faced with if a bomb fell on Long Island, New York City, in the harbor, and each one of these gives you entirely different kinds of problems to face, that I think it's very easy, after having become involved in it and seeing the children in kindergarten hiding under a table with their heads up, which also has its absurdity, to just put it out of mind and say, "There's nothing we can do about it;" I'm sure this enters into a good many people's thinking about it.

TAYLOR: I think it's very important to distinguish two different kinds of hopelessness and I think I know the kind you had in mind, Arthur, although I'm not sure. One kind of hopelessness comes from the idea that no matter what we do as a country or as a world, any kind of a nuclear war will destroy us all in some sense; will ruin us, no matter what we do. There's another kind of hopelessness that is the kind I feel and that is that it's of no use for me to do something all by myself, and that's very different.

I'm for Civil Defense. I don't have a shelter because I can't see how to use the shelter if there aren't any other plans around my home or around the place where I work. It's a little bit like saying one is against pollution of the air by automobiles and then is given the option of spending $100 to put something on one's car that will make it completely pure. No one will do that by himself because it doesn't even begin to solve the problem. That doesn't mean he's for pollution or

against anti-pollution measures. I think it's very important to distinguish those two.

AYRES: As a matter of fact, there's a nice illustration. People understand this point so poorly that one of the petroleum companies —I'm not sure which one—published an ad in a national magazine saying, in effect, "You can stop pollution. Stop driving!" [Laughter]

HEMLER: There's another point that should be brought up here with respect to the new types of hopelessness that you are talking about. I think a good example that brings this out occurred in 1961, during the Berlin crisis, when at the national level it was brought out that a nuclear war was very possible. The Administration, the Department of Defense, Civil Defense, all brought this point out very quickly and very abruptly in, I believe, September 1961. People all over the country then began to have meetings and to build fallout shelters; construction companies and builders became involved. There was a mass movement toward the fallout shelter which points out that there was not the hopelessness of total destruction involved. At least it points that out in my mind.

You will also recall, however, that the movement was random in all directions, a multi-directional movement, because after having made the statement that nuclear war was possible, no instructions were immediately given as to how people should prepare; consequently, people began to prepare in their own different ways. There were people I know in my area that began to prepare their own personal family fallout shelters and there were a number of them built. There were some cities that began to build public-type shelters. I remember I was living in El Paso at the time. Do you remember a nationally publicized case which concerned the small city of Artesia, New Mexico, a city of about 25,000 people, which had plans for building a public school? They built as part of that school an underground public shelter for the school children and the people in the small town, although Artesia was far from being an important area. This mood continued for four or five months, but again no definitive instructions were coming from Washington or from the people that the public thought should provide them with the information on what should go into fallout shelters, what the fallout shelters should do or would not do. In fact, if you look at the record I think you will find that Civil Defense was caught badly off-guard by the statement made. They frantically tried to prepare instructions and tried to prepare the yellow pamphlet that you were talking about, but that came out afterwards. So there was

a five or six-month period in which no instructions were forthcoming, no information was forthcoming. The people were trying to do something and it ended up with pretty much chaos. By the time the government acted and was able to get the instructions out, the interest had died off.

Also during that time I remember meetings. I know I attended one PTA meeting where a speaker had been asked to talk on nuclear effects. Unfortunately, he was one of those experts who become expert by hearing and reading such things as On The Beach, and he managed to terrorize the audience. I got up about half-way during his talk and started trying to ask him questions that I hoped would bring the thing back into perspective, but it was a losing battle all the way. Later on in our area we did put out speakers, people who could talk about the effects to some degree. But we were talking earlier in this conference about not giving out any information. This was a case of not telling the people the problem or the facts early and in detail. This was a case where we gave out the basic fact that a nuclear war could occur too early and without any information to provide guidance for people.

Since that time there has been apathy with respect to the Civil Defense program even though the Government in fact, as I said, came out with instructions later on. It just died off. I worked with the Illinois Civil Defense people after that and it was very difficult to bring the people, the public, back to the point again because the impetus was no longer coming from Washington. The instructions were there and had been put out, but there was no push to use the instructions.

FREMONT-SMITH: Did any of you see the article in Progressive magazine at about that time entitled, "It's a Problem," a satire?

TAYLOR: Yes, I did.

FREMONT-SMITH: I think it's one of the best I've ever seen and I really think it points up exactly every single issue that came up. A man put up a Civil Defense thing in this small town and a friend of his learned of it through a plumber because nobody else knew about it, and he took his 11-year-old-boy over to see what his friend had made and then he asked him a series of questions. He said, "What do you do about air and what do you do about a filter and what do you do about your neighbors coming in?" At each question the man who was building the defense thing contemplated it and ended up, "Well," he said, "It's a problem!"

SPEAR: Before we get away from John's account of what happened in late 1961, I would like to point out that there was a kind of a significant footnote to the push. I believe it was in November of 1961 or thereabouts that Life magazine, which has a very substantial circulation, came out with one issue that was largely devoted to the fallout shelter question. This gave it a great push. The Life presentation did have some instructions. Obviously this was an inspired piece and the administration was with it. At that same time, in a confused setting when Civil Defense was in the process of transfer from the Executive Office to the Department of Defense, there came the drive to get out the little yellow pamphlet. Well, obviously, the administration was strongly behind the fallout shelter idea at that time and there was great drive to get the pamphlet out. But, as one can imagine if you have been in Government, the process of clearing its details was no fast matter. People in the State Department would worry about the effect on other countries of the mere fact that the Government put it out. Others worried about this impact on people: "We shouldn't say it that way!" Others had other questions. This took a long time, and along about the time—and I forget the sequence—the pamphlet finally did come out, the Congress was busy on some appropriation bills involving federal construction. The lack of national consensus was rather dramatically pointed out by a rider that went in many of these appropriation bills forbidding the use of any of the funds involved in making fallout shelter preparations in federal buildings.

The administration was also putting on a big drive for the owners of commercial and residential buildings to make such provisions. All of these things contributed to taking the steam out of the home shelter effort.

One other thing, too, was that many of the people in Civil Defense themselves were not sympathetic to the idea of the individual fallout shelter. They felt that this was no real answer; that community shelters were required where you could have large numbers of people together. So they didn't themselves take any steps in this direction. They were waiting for the public appropriations that would bring about community shelters. Those, of course, never came.

One other point I would like to make, going back to the effects of nuclear weapons. Merril, I agree that there is a mass of completely useful information there and that the government has done very well in making this available. But I think it takes more than simply making it available. The information is there but this has

never been translated into useful working knowledge on the part of most of the people of this country who need to know it, and that's almost everyone. Somwhere in here there's got to be some push, some motivation, some real incentive for people to assimilate that information and to make it working knowledge. It has never been done. I don't know how you would do it.

FREMONT-SMITH: In contrast, there is Sweden. Isn't there an entirely different situation there?

SPEAR: Of course; in Sweden you had the willingness of the government to dedicate substantial funds to underground shelter purposes for industry and for government.

FREMONT-SMITH: And for people.

SPEAR: Yes. And I suppose one could say that Sweden may have spent on civil defense what we have been spending on our military budget.

FREMONT-SMITH: I wonder whether psychosocially this contrast between two different nations isn't worth contemplating, because we had this surge of interest and then apathy following, and it raises a question of whether in a democracy such as ours or in a country built the way we are, one can keep up a pitch of interest which fall-out shelters require to keep them functioning when the war doesn't come. On the other hand, what has Sweden done? I think this is worth discussing.

SPEAR: I wonder whether the traditional stance of neutrality may in some way that I don't understand be responsible for this? You have much the same thing in Switzerland.

FREMONT-SMITH: And they are nearer to Russia, too.

SPEAR: The Swiss have a similar tradition of neutrality and I've always wondered how they can keep it alive over the centuries, in which they don't fight, the tradition of military responsibility of each adult male. Arms are regularly issued to them and when the church bells ring they assemble at certain points. They rehearse this periodically. How they do this, how they keep alive that interest, that sense of obligation, without ever being called upon to really use it, I don't understand.

WARREN: Just enough wars and their children are brain-washed. Isn't this one of the elements where we have fallen down a bit? Although I think the schools have done very well. Perhaps the Civil Defense exercises they go through as part of their fire drill and other things may be foolish to some extent, but it's an image which has been built up now in our children. It's partly responsible for some of the fear and hopelessness that the present college students voice, for instance, at least in my opinion, because they were indoctrinated in the hazards and the dangers but there was no solution offered. This gives then a sense of instability and frustration and hopelessness and so they are anti-social. "Let's throw the oldsters out and let's start a better world." But they have no solution for that either.

But don't we create our image for the next generation in the home and in the school, and without conviction at the home level? Isn't this as important as conviction at the high administrative policy level? In fact, it's more so because it furnishes the base on which the whole direction of thinking rests. A lot of thought has been given to this over the years, as you all know, but we haven't come to any clear crystallized policy on our own side, have we, as to what the facts are and how they should be related to our society?

FREMONT-SMITH: Is there a national policy in schools now? Do all schools in every state have the same kind of drills?

WARREN: Pretty much. It's a general policy that is now like osmosis. I think you would have a difficult time in taking it out of the school system. It's just like a fire drill.

UPTON: I get the impression that the Office of Civil Defense is treated as a poor cousin, and that we really don't have a firm, positive, well-implemented national policy in this direction. I may be quite misinformed on this. I would welcome some discussion on it.

MILLET: There's something I wanted to say at this point in connection with what Upton said a little while back about hopelessness. I think there are a couple of things that haven't been brought out here very much yet. One of the possible reasons why there's so much of this—I quite agree there is a great deal of that feeling around—is because people are so, let's say, spoiled. We have become so spoiled in this country about our way of life that it is not uncommon to hear such things as "Well, if it happens, who would want to live under those conditions?" I think there is a great deal of that.

Now, as far as Sweden is concerned, it occurs to me that they are always expecting that somebody is not going to respect their neutrality. "For some reason or another we had better be on the lookout because this one is going to hit us or that one is going to hit us."

I quite agree with what Dr. Warren has said about Switzerland. It's a phenomenon that's extremely hard to understand. It could be extremely important strategically, one would think, from the standpoint of Central European wars. Perhaps it has something to do with a multi-national composition which makes for a certain stability in terms of international relations. "Don't hit us because possibly you would hit somebody who is your first cousin if you do this."

HEMLER: I think maybe you hit on the point when you said that Sweden might be afraid that someone may not respect their neutrality. They have seen this happen in Belgium, Holland, Denmark. These countries have all declared their neutrality at one time or another and have had it overrun.

ROOT: I think Sweden is a little special. They started because they knew in the next war, being so close to the Soviet Union, their borders would not be respected, they would be overrun. But, in addition they had a technological incentive. The engineers developed a very simple and cheap way of drilling into stone with no danger of silicosis or splintering: a one-man drill. So they started drilling into their own rock and found it cheaper to build a room by drilling a hole than by enclosing a space in air—heating is cheaper, maintenance simpler, no windows. Their fallout shelters pay for themselves. Saab is located underground entirely. Big oil deposits are all underground. Atomic energy is underground. They have an underground hotel which can be converted into a hospital and lots of extra garage space.

So actually they don't need to budget for something which may or may not be used. They have a magnificent civil defense network which is a useful and paying proposition at the same time.

MILLET: Maybe if we could learn how to burrow economically into our rock in Manhattan, instead of going up into the air, we would be quite all right.

TAYLOR: I think there are two other reasons that are quite important. One of them is that Sweden, being a neutral country, is not

subjected to the idea that developing a simple defense program is provocative, that it will provoke the Russians. That's a very strong force in this country.

The second point I think is that they are probably unlikely, at least the way things are developing internationally now, to be subjected to some all-out massive attack. Therefore, they can answer the questions "What is the world going to be like; is the world going to be worth living in afterwards in Sweden?" They can answer a little bit more affirmatively, "Yes." But I think the strong point is that there's been no anti-civil defense.

ROOT: I was there at the height of the program and they were worried about the Russian reaction. And Finland was worried. It's very important for that area for Sweden to stay neutral to help the actual border countries.

They also did what they consider a possible provocative thing. They reorganized the military structure instituting compulsory service for all citizens, to provide a constantly changing but constantly standing army, which they didn't have before.

UPTON: Of course, there's the other element, too, which you brought out, I think, on the first day, Ted, and that is that we have relied on the old adage "The best defense is a strong offense," and we have more or less thrust into the background the notion that something might happen even though we were able to deter it. With change in the distribution of plutonium throughout the world, this philosophy becomes increasingly precarious.

TAYLOR: I must just say that as far as I'm concerned I have had some doubts about whether we should have a civil defense program in the past. I have no doubt whatsoever now, for this reason, that I've seen ways in which the deterrent forces can fail to hold things off, so that no matter what our national leaders do, criminal organizations, what have you, groups of people over which we have no control whatsoever, can threaten other groups of people.

UPTON: This deterrent philosophy wouldn't be valid at all for Sweden or Switzerland or smaller powers.

SPEAR: I must say that I've never been able to understand the reaction to which you refer. I know it exists, the feeling that a

meaningful civil defense program is somehow provocative. When you think of the building up first of our SAC force, then of the vast array of missiles, the nuclear subs that are on location at all times, these would represent to me, if I were sitting in a potential target country, a hell of a lot more provocative posture than building some shelters. Why this remains an influential argument I don't know; however, it does persist.

BRUES: I must say that I am becoming very disturbed to learn that the whole question of civil defense rests on these guessing games like that of the three prisoners. "If I do this, what will somebody else think I am thinking?" [Laughter]

HEMLER: I'm not sure that it was all that, although this may be what it has resulted in—this guessing game back and forth. I suspect from some of my readings and from some of the speeches made in Congress and by the Secretary of Defense, that this might have grown out of an original economy requirement. This was true back in the early sixties when we were first talking about a massive ICBM deployment coupled with a massive civil defense construction. We were talking in terms of multi-billion dollar programs and just as late as last year or six months ago, if you recall, we were talking in terms of $40 to $50 billion for defense missile deployment. These figures thrown out at the public were pictured as being something that was entirely impossible for economic reasons. Perhaps some of these other arguments that we're talking about here have come about to assist in rationalizing the basic fact that we just didn't want to spend the money or can't afford to spend the money.

SPEAR: I think that's undoubtedly true and I recall that in the late fifties—I forget the exact date—the Administrator of Federal Civil Defense, having beaten his head against the wall many times on this, finally delivered on the administration's doorstep a package of blast and fallout shelter programs with a price tag estimated between $30 and $50 billion. Not long after that he was appointed our Ambassador to Denmark! [Laughter]

FREMONT-SMITH: Isn't there another element in this thought of provocation with civil defense, and that is that if the government backed a strong program of civil defense, this could be misinterpreted as indicating a government policy which expected atomic warfare and therefore was probably going to be involved in one? It seems to me that this could be seen as provocative in that sense, not directly provocative but implying a behind-the-scenes policy of expectation which

meant that the world situation was getting worse and that we might be going to do something. Therefore, we might even try to knock out somebody else's atomic capabilities.

AYRES: Unquestionably so with the doves.

DUNHAM: Yes, but didn't somebody point out that the powers that be on the other side, as it were, wouldn't be so stupid as to assume that a strong civil defense was such a great threat to them and meant more in terms of provoking war than all these other things? The same people who are yelling about the provocation of having shelters and things, they yell some but not nearly as loud about these other things.

EISENBUD: What about the argument that was used against the Harbor report, for example? This report came out a year or two ago under NAS auspices.

AYRES: 1963.

EISENBUD: Time flies. A committee of rather distinguished people, headed up by Eugene Vigner, drew up a report on civil defense. One of the arguments that was made against the report and its recommendations was that it was less costly to add to the offensive power enough to overcome the civil defense than it is to build additional civil defense to counter the offensive power. So that if we invested, for example, $30 billion in a civil defense program, perhaps its value could be offset by, let's say, a $10 million increase in the offensive power of a potential enemy. Is this a valid argument?

HEMLER: It's an argument that's been used.

TAYLOR: No, this statement has been made by the Secretary of Defense over and over again. I've looked very hard for any backup in terms of any American analysis that demonstrates that this is a fact and I've never seen any such argument.

AYRES: The Kent Study (Reference 55) did show that, didn't it?

TAYLOR: No. The question is whether it's cheaper to build up an offensive force which will kill as many people in a situation where they now have a blast and a post-fallout shelter system, not an individual fallout shelter.

AYRES: They have an additional argument which gets linked with this, that it's cheaper to knock out the missile before it's launched than it is to do it the other way.

TAYLOR: No, but that's not the argument.

ROOT: Am I wrong in assuming that if we do have an ABM, that makes a fallout shelter program absolutely imperative?

TAYLOR: That's what the Secretary of Defense has said. However, he has not to my knowledge said anything about this in the last few months. I'm very curious to see what he's going to say.

EISENBUD: I felt this way at the time and I still do. I think that the government really passed the buck down to the individual family in a very unfair way. I had a good opportunity to look into this for probably the No. 1 target, namely, New York, when I chaired a committee appointed by the Mayor to look at the problem of civil defense in New York, and, frankly, we came up with a recommendation that ultimately led to the disbandment of the local civil defense organization, a transfer of its functions to the police and fire departments.

Now, the basic reason was that we could get no backup from the federal government. New York is the headquarters for the First Army, which has some very important missions. Mainly, it's charged with the movement of materiel and personnel through the metropolitan New York area in time of war, and yet we found that the First Army, in order to implement its mission, had really not considered how they would get through the city streets. They were expecting the city police department to do it and this police department had four channels of radio; the fire department had four channels, the civil defense had another four. There were twelve voice channels. In other words, in New York City, knock out the existing communications system and they can only carry on twelve voice conversations at the present time and, as I said to somebody yesterday, it would take all twelve people to find out where the Mayor is and whether he's still alive.

AYRES: I think the telephone company probably has quite a number of additional channels. Did you check into that?

EISENBUD: They may have it now. They didn't have it at that time.

UPTON: Independent of existing wires?

EISENBUD: If they made it available to people who contracted with the city. We didn't look into that. The point is that the channels available to the City of New York numbered twelve.

I'm familiar with what they did in New York itself, which is a communications network. They have a fine building and they, themselves, have a system that can support itself. But the point is that New York City then only had twelve channels and there was no easy way in which they could get additional ones without taking the channels away from people, who wanted to hang on to them, and most of them were government controlled.

The other thing was that the intelligence network at the time was so inadequate that it would be impossible for the local civil defense people to find out how big the bomb was, exactly where it was detonated and how high off the ground, which are three fundamental things you would want to have. This information could not be made available from the outside at that time.

The organization simply didn't exist with which it was possible to integrate the civil defense requirements of New York City with those of the surrounding communities. So, some of the evacuation plans that some of the New York suburbs had were completely opposite to what New York was trying to do, and vice versa. But the point is that, just as happened now, when everyone talks about civil defense one tends to focus on the family shelter, which I think is a dreadful thing. I think that if the federal government, through its military organization, can't provide the ultimate amount of capability to deal with these problems, I don't see how the individual family can nor do I see how the individual community can.

WARREN: The point of view of the military is, isn't it, Merril, that they have their own assignments? They can take on this additional assignment and their own assignment. At least when I was active in this field—I can't speak for the last four years—their own assignment had to do with defending the local situation against invasion, which is a pretty outmoded policy.

EISENBUD: Well, we ended up just discussing these things with the military and we just threw up our hands and decided that there was just nothing that the police department, the fire department and the civil defense organization could do.

This, of course, may be a more complicated problem than, let's say, you would have at Oak Ridge, you see, and that others might have. But your situation on Long Island would be dreadful because there would be millions of people trying to get out there and now, of course, they couldn't get out but they would try to get out there because it's the one place that they can go if they can't get across to the other side of town.

I wonder—this may seem logical, but I think perhaps it isn't—if what we are trying to do is to focus on some of the things that should be done without really having perhaps an adequate discussion of what the problem is.

ATTACK DAMAGE AND PROBLEMS OF POST-ATTACK RECOVERY

EISENBUD: One of the difficulties that one always faces in a discussion of this kind is that there are almost an infinite number of permutations and combinations that could result in a nuclear war of any dimension and any pattern, and I thought it might be worthwhile to take a few minutes to discuss the kinds of situations that have been assumed in the past in discussions of this kind. I think it would be unfair to call on anybody on short notice. I tried to stick Dr. Dunham with it but he didn't exactly bite. So I would like to run down, from my own recollections, what the assumptions were at the last Joint Committee Hearings on Nuclear War.

The size of the attack was on the order of 10,000 megatons. Maybe take off a factor of 2. It doesn't matter for purposes of discussion. A megaton is a big number.

AYRES: The attack in question involved 1,450 MT on the U.S. (Reference 56).

EISENBUD: What was the whole war?

AYRES: I think it was about twice as much.

EISENBUD: 3,000, then?

AYRES: Yes.

EISENBUD: All right, we'll say 3,000. I want to put the size of this into perspective by pointing out that 20 megatons of TNT—within a factor of 2—would be a block of TNT 10 feet high and 10 feet wide and

1,000 miles long, and these are the individual bombs that we are talking about when we say 20-megaton bombs.

UPTON: This, Merril, is just on the Continental United States?

EISENBUD: It's really half of that on the U.S.

UPTON: Half here and half on the U.S.S.R?

EISENBUD: Yes. As I recall it, this was in the United States. I think there were 200 targets. Is that about right?

AYRES: That sounds right, yes. Something like that.

EISENBUD: And the casualties, does somebody remember the number of deaths? I though it was about 70 million.

HEMLER: Sixty to seventy.

EISENBUD: We'll say 65 million; and how many injured? Another, we'll say 75 million.

TAYLOR: You might as well say everybody is injured.

AYRES: That seems a little bit too high.

EISENBUD: This figure is about right.

CASARETT: Merril, is this qualified by shelter or no shelter or anything of this sort?

EISENBUD: This is in the open, with the economy as it is now.

AYRES: It's not necessarily true that there's one hurt man for every dead man.

EISENBUD: All right. We'll put a question mark there. What are the livestock figures, John? Do you remember? What fraction of the livestock in the United States were killed? The figure I have in mind . . .

AYRES: I don't think that they ever gave it.

EISENBUD: There was a statement given by somebody, I think it was around 50 percent.

TAYLOR: It's probably reasonable.

BUSTAD: I would say that's too high, much too high acutely because most of them are away from the target areas.

AYRES: This was 50 percent counterforce, 50 percent countervalue. A lot of military targets are in agricultural areas.

BUSTAD We just said that at this kind of dose you're going to have two kinds of casualties. This was just entered into the computer. I'm just going by intuition now, and the location of the majority of the livestock.

EISENBUD: The thing that I do remember is that on the maps I saw, all of the dairy cattle in our part of the country, in the Northeast, would have died, all of them.

AYRES: Yes, but that's only a small fraction.

EISENBUD: Yes. Still that's where we get our milk.

BUSTAD: Let's try 25 or 30 percent.

FREMONT-SMITH: And then injury will be another equal number at least?

AYRES: Not necessarily.

FREMONT-SMITH: Doesn't injury count? Injured badly enough to be dead?

BUSTAD: Are you talking of the acute situation or what the casualties are after six months? This is rather critical, I think.

EISENBUD. I think six months would be the fair one. Of course, that's hard to evaluate. I think the hearings count was, as I recall, a situation at about 30 days.

AYRES: Yes, but their criterion was, I think, that you would either recover or you would not. It was about a 30-day period.

EISENBUD: Yes. That's right. These are the basic statistics. There were also statistics that I don't recall about the number of individual homes that would be destroyed, the number that would be

damaged beyond repair and would have to be evacuated. I don't recall the figures. I wonder if any of you do. I would certainly hope that they could be in the record of this discussion when the transcript finally comes up.

TAYLOR: They must have been approximately in the ratio of the rural to the urban population, which is maybe a third. That's a third of the houses which survive, but they would be almost all rural. That must be within a factor of two.

EISENBUD: Yes.

TAYLOR: Would survive?

DUNHAM: It would be a third of the suburban houses that would survive.

EISENBUD: These are the casualties that resulted from the initial strike. They do not or did not include, for example, casualties from starvation resulting from the fact that perhaps the crops are standing in the field when the attack comes and nobody harvests them. So all the crops rot, warehouses burn; there might be a lack of water, not so much because the water is contaminated but simply because the distribution systems are destroyed. It did not contemplate the effects of disease, the fact that medical facilities would be inadequate to deal with disease.

Then one has to consider what happens afterwards, because surely there will be some period which might be only six months or it might be six centuries while there would be a continued attrition in society due to the fact that society hadn't quite recovered enough to deal with the raft of explosions, the insects, the viruses and God knows what.

UPTON: The absence of a major effect on other continents?

EISENBUD: You would have to assume that there was about equal devastation or more in Europe.

DUNHAM: What about Canada?

AYRES: I think the attack covered Canada as well.

EISENBUD: Yes.

UPTON: What about Latin America, Australia?

EISENBUD: No, they weren't included and it would seem to me they ought not be included.

UPTON: Can one imagine a six-century-long dislocation if Latin America and Australia survived practically intact?

EISENBUD: I didn't say it was six centuries. It could be six months or presumably six centuries. I would say it would be longer than six months.

AYRES: You have to remember that you cannot expect much help from places like South America because even the surviving production in the first year is perhaps more than they have on a per capita basis. The pattern would probably be very unbalanced and distorted compared to pre-war.

UPTON: But in the presence of flourishing civilizations on other continents one can't imagine a Dark Ages that would last centuries.

AYRES: I agree with that. I'm saying that our surviving wealth would be considerably greater than the existing wealth in most parts of the world.

WARREN: But would it be enough to purchase food from other countries?

AYRES: It depends.

WARREN: We've got a lot of gold in Fort Knox.

AYRES: We've got a lot of gold in Fort Knox; we have very much more overseas investments which can always be disinvested. We have something like $50 billion in overseas investments.

WARREN: Unless they are appropriated by local powers and they thumb their noses at us.

AYRES: This is one of the interesting questions. One reason that they might not want to appropriate your surviving overseas wealth is, first of all, that we hold a lot of their gold which, of course, is true, and secondly, we would still have a very, very impressive surviving military force.

WARREN: Which we couldn't resupply, though, once it was used up. I don't mean to be pessimistic, I just wondered what the situation was.

AYRES: I don't know that they could or couldn't.

EISENBUD: I wonder how long our ships could continue to operate if they couldn't get spare parts? This is where you get into trouble because no one has enough knowledge about the details of our economy. You can imagine how important this is. There will be some small plant on Long Island that makes something about as big as your fist and because it's destroyed the ships can't go to sea. You see that in strikes. A company will go on strike and its ramifications are enormous.

AYRES: An example might be the General Motors strike, where the Fisher Body Plant was shut down and everybody else went out.

EISENBUD: In this case it was the suppliers, you see, that went out. But I'm thinking now about people who make small electrical parts.

AYRES: There have been studies of this problem (Reference 52). For example, the National Academy of Sciences Committee on Emergency Planning has made quite a point out of this. They have had a number of meetings involving representatives from industry and they have tried to think of specific vulnerabilities of this kind—bottlenecks—where, for lack of a nail, the shoe is lost, etc. And depending on your attitude towards these things, you may think they came up with a lot of a little. My own feeling is they found surprisingly little. One of the things that's often mentioned in this context is tetraethyl lead. It happens to be made in two or three plants. On the other hand, we don't need it. We could operate quite well without it if you're willing to let your engines operate inefficiently. Certain things are crucial, but you can usually find a way around them, and it's very difficult to find something that you could be confident would really knock you out provided you can look for alternatives, and I think probably the lack of transportation is more crucial than the other situation.

EISENBUD: I'll wager that there isn't one or two companies in the United States making valves for pipelines; you know, these big valves that go into pipelines.

UPTON: What about Latin America, Australia?

EISENBUD: No, they weren't included and it would seem to me they ought not be included.

UPTON: Can one imagine a six-century-long dislocation if Latin America and Australia survived practically intact?

EISENBUD: I didn't say it was six centuries. It could be six months or presumably six centuries. I would say it would be longer than six months.

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EISENBUD: I'll wager that there isn't one or two companies in the United States making valves for pipelines; you know, these big valves that go into pipelines.

AYRES: How many do you need in the first year, is the point? Let's say a pipe plant is put out of commission?

EISENBUD: You might need hundreds of them. They are big items.

AYRES: Probably you'll cannibalize.

SPEAR: Back in 1951 the Federal Civil Defense Administration conducted a rather intensive study of the items produced in Cleveland, Ohio. As I recall, we found 6 or 8 quite important products that weren't produced anywhere else in the country. The Industry Evaluation Board of the Department of Commerce routinely addresses itself to the critical essentiality of industrial and other products and facilities. These are rated according to their essentiality. Unfortunately, both of these efforts were classified, and the result of the Industry Evaluation Board's work has never been declassified as far as I know.

AYRES: But can you say what would be the impact of the loss of those 6 items?

SPEAR This was the object of the Federal Civil Defense study that I mentioned. There were various studies by ODM prior to the amalgamation of FCDA and ODM, taking the result of the I. E. B.'s work and studying the impact on the nation of the loss of various products and facilities.

AYRES: The point I am making is that one might conclude from what you have just said that the whole economy grinds to a halt. I think that would not be the case.

SPEAR: No, but there are some critical vulnerabilities.

AYRES: Critical, but how critical?

SPEAR: Well, I don't know how to generalize on how critical they might be. The delivery of oil by pipeline, gas by pipeline, electric power transmission are some of the things, as well as industrial products, that I. E. B. has been dealing with. As you conduct war-game attacks on this country, you can see the effects of taking out some of those things. Over the years, some of these vulnerabilities have been lessened. Communications is one good example where there has been a hardening of facilities.

AYRES: I think it's fair to say that when you look at these things in detail, you can usually find an alternative way of solving the problem. If it's a valve for a pipeline, you might have to close down one pipeline but there are other pipelines. If there are no pipelines available there are barges and unitrains and ships. There are other ways of solving almost any electrical problem by avoiding the particular device. But it's usually more expensive, and in that case it is critical.

EISENBUD: Bob, let me give you one example, though, at the present time. If you want to build a power plant, you can't do it in less than about 5-1/2 years because the delivery time on turbines is close to six years. This is in a normal, healthy economy. Just think about what would happen if a large number of power plants needed to have their turbines replaced.

AYRES: It's more likely that other things, such as transformers, would go first.

EISENBUD: This may be, but you are going to need turbines. You are going to need them in large numbers.

AYRES: Turbines are extremely hard, you know. That's one thing that would be very hard to destroy unless you had a direct hit.

EISENBUD: Of course, this again is part of the unknown.

DUNHAM: You've got a much smaller population there.

TAYLOR: I think there's an important question. Where is it one is trying to head? We've gone through societal retrogression, and it seems to me it's worth trying to be a little bit quantitative in asking how far we have dropped back. Have we, for example, dropped to a point where the productivity very quickly can be brought to the same scale as it is in the United Kingdom or in France or in India or in Mexico or whatever, because there are factors of ten or twenty involved there, and if you just look at numbers like that, there aren't factors of ten or twenty immediately visible. In other words, there's a temptation to say the drop has been to a standard of productivity which is still considerably higher than all of the underdeveloped countries in the world today.

AYRES: That's in fact the indication when you look at it. It may not be so, but the reasons have to be rather subtle ones.

TAYLOR: Having dropped by an amount which is very important, then the next question is, is the aim to get back up to where we were before the attack, which I think is foolish, or is it to . . .

HEMLER: Survive.

TAYLOR: Survive and move in new directions, perhaps. Let me give one example. If half the power plants had been knocked out or two-thirds had been knocked out, I don't see that it should become an objective to go back to the energy standards that we had before the attack, because that's lavish, luxurious. We may be much more interested in raising the food productivity level than raising the power level.

AYRES: Incidentally, there have been unclassified studies (Reference 52) done on this question of energy, which suggest that it will be in oversupply.

TAYLOR: It is now.

EISENBUD: I was in Rio de Janeiro last January and there was a bad storm which washed some mud down the mountainside and in through the race of a hydroelectric plant; when the water receded, the mud caked and the plant was knocked out for an estimated year and a half.

DUNHAM: If the bomb landed on the city and not on the power plant, you would have no power problem.

EISENBUD: Let me say what happened as a result of the plant being knocked out. Who would have thought, for example, that in the middle of January, just before Carnival, the sewage pumps could not be operated? Sewage was pumped into Copacabana Beach.

TAYLOR: Isn't that standard in all seasons in the world today?

EISENBUD: No, but just think of the implications of the loss of power supply which resulted in the required discharge of the normal amounts of sewage in untreated fashion into the surrounding waterways. Then superimpose on that the additional biological material that's going to have to be disposed of, recognizing that in most of these estuaries, or many of them anyway, the biological demand is just a little bit less than the dissolved oxygen.

AYRES: You've just given one of my nasty scenarios (Reference 57).

EISENBUD: Here is just one example, you see, that comes to mind. Of course, the basic problem is that the more you think about it the more complicated it gets and so you go back to your policy of not thinking about it.

DE BOER: Who is going to provide the leadership? Two years ago I was part of a Civil Defense exercise in Albuquerque. Here you have a population of 300,000 sitting in the desert, so everything has to be trucked in. The first question I asked was about food supplies. No one had an answer. They, the Civil Defense officials, had made two basic assumptions, i.e., there would be adequate reserves and a functioning distribution system, and food would be continuously brought in. Well, I don't believe that anybody would be crazy enough to make a 200- to 300-mile trip across the desert under these circumstances, even if the trucker had sufficient gasoline. In case of disaster there, our best estimate was that we might last six days to a week. With possible hoarding, this would be considerably less. Most people don't have anything to fall back on and those who do, more often than not, depend upon their freezers. Electricity under these circumstances may be out. Food and oil supplies would be gone first and these were the very things most of them had not given enough thought to. The assumption had been that supplies at the worst would be temporarily discontinued. I am also sure that people would not come out of the valley for weeks. This valley is nearby and there are a few things which are grown in the Rio Grande Valley near Albuquerque, such as apples, potatoes, and tomatoes, but these people would not come to the city to sell. They would sit on what they had, preserve it and use it themselves under the circumstances. They surely would not be interested in commerce.

AYRES: That may or may not be true. I would probably disagree with you about the amount of food in Albuquerque. I don't know if you made a direct survey or if this is based on casual inquiry. The Department of Agriculture has done surveys.

DE BOER: This is the point. They were never confronted with that thought. So when I asked the question, there was no one who could give me the answers.

AYRES: You see, most of the food in the city is not in private houses; it's in stores and in the warehouses.

DE BOER: No, these were Civil Defense officials. They should have known, but they didn't, and, again, my question of responsibility arises.

AYRES: The average supply in the United States is roughly thirty days, not four or five days. There is usually a ten day supply in the stores and a twenty day supply in the warehouses.

DE BOER: Yes, but you're looking at the ideal conditions, if you could distribute it nicely.

AYRES: I mean at the present time that's the pipeline.

FREMONT-SMITH: They would still have to be distributed.

DE BOER: You're still looking at the working distribution system.

AYRES: Yes, but I mean the food in the warehouses is not going to to be eaten.

DE BOER: If the man says, "You come and get it" that's different.

AYRES: What can he do with it? He can't eat it all. In general, I think that the problem you describe is a very real one. I wanted to quarrel with the specific number because I don't think it applies.

DE BOER: I had never been confronted with this sort of question and it intrigued me.

AYRES: I think you are quite right, that Albuquerque is very isolated and it's rather hard to imagine people bringing in food during the first six-month period.

WOLFE: I'm more worried about water, not for Albuquerque but for any of the targets. Before you get all the turbines whirling again and all the material redistributed, it does seem to me that you ought to consider what the world is going to be like, the local world for each individual as he comes up out of his shelter or gets up off the ground if he's able to, or whatever the situation is. What is he walking into? I raised some question about these shelters in 1962, and I had to conclude then that the shelter would probably be a means of slow death instead of quick death. After all what do you come out of the shelter to?

WARREN: You're not talking about fallout; you're talking about what resources are available when you emerge from your shelter.

WOLFE: Well, they may or may not be available. If they are available, they might be some distance. The way I visualize this is that suddenly the population of all ages becomes sort of senile. Nobody is going to grab the ball and run a hundred yards. This sort of thing must be some sort of shock and I haven't been able to find out what they did at Nagasaki and Hiroshima? What did those people do?

WARREN: Those that were left escaped very shortly after the blast into the surrounding countryside, generally with an amnesia. They didn't know how they got there. The catastrophe was so great that very few of them did any useful work for quite some time. Those who wandered back to start building again or who tried to erect some kind of a shack were mostly squatters who didn't own the land in the first place. But the farmers and the fishermen were still just the same—those who were out in the periphery and who hadn't been touched by this.

FREMONT-SMITH: There won't be so much periphery according to this.

WARREN: There was no periphery there, although the other part of the war had touched the other big cities which were similarly badly damaged.

WOLFF: If there are 200 targets, I suppose we have to assume that they are all going to be hit or nearly so. Just wander out in the country, this would be a psychological thing that most of you people know more about than I do, but I can't imagine that we're going to get in there and start to rebuild right tomorrow or next month or maybe for months to come.

FREMONT-SMITH: Aren't people going to be very much concerned as to what they're going to have to face the next few weeks?

WOLFE: I think they're going to have to be concerned about where they're going to get their next drink of water.

FREMONT-SMITH: Yes. Some of them will have a little water, but at least it will be the most immediate thing. It is not what's going to happen next month but what's going to happen in 6 days or 60 days

that's going to occupy the attention of the people as they come out of their shelters, isn't it?

WOLFE: Yes, I think they have to eat and sleep.

TAYLOR: It seems to me one difficulty with this discussion is that it's a little bit like asking what happens in either of two cases. Case number one: a person is shipwrecked and winds up on a desert island with nothing on the island. You're asked what does he do? He dies. Case number two: a person who is shipwrecked lands on the same island with a survival kit and something out of which he can make distilled sea water and some seeds, etc., and he sets about the business of trying to survive in an organized way.

HEMLER: Then maybe he does.

TAYLOR: Maybe so. But I think it's important to separate the two questions. One is someone who suddenly emerges in a situation with no plan of any kind. He's put into a disorganized context altogether.

FREMONT-SMITH: People are going to have a great variety. Some people are going to come out and find nothing.

TAYLOR: I'm asking, I guess, what are we thinking about? Are we thinking about the U.S. today, right now? Or are we thinking about what it might be for the U.S. at some later time? You say he comes out of his shelter. The chances are he doesn't come out of the shelter because he doesn't have one.

WOLFE: That's true.

AYRES: If it's a surprise attack today he's not in a shelter.

TAYLOR: All right. So I think it's important to distinguish.

FREMONT-SMITH: We have to decide what assumptions we are going to start off with.

TAYLOR: Is it a prepared situation or is it a non-prepared situation, because they are vastly different, almost like night an day.

FREMONT-SMITH: Even in a prepared situation there will be an enormous variety of experiences to be faced when people come out,

and I was talking as if it were a prepared situation. But again, some will find nothing when they come out, depending on where they are. Some will find everything very nicely arranged for ten days or so when they come out and there will be an in-between. I would assume that a very important consideration will be how we get into communication with other areas to find out how bad the situation really is. What do we expect? Is there another bomb coming down, and, if so, when and where? What kind of social organization can be expected the next ten days or so?

TAYLOR: Are you putting that in the prepared situation?

FREMONT-SMITH: Yes. I am talking about a prepared situation. I would think that if we were going to try to face this, we ought to say, "Well, let's take a prepared situtation, let's take an unprepared situation, let's take a semi-prepared situation and take each one separately and try to follow each one through."

TAYLOR: Yes. So long as we understand what we are talking about. Otherwise, one person is walking into an unprepared world and another one is walking into one that has a $100 billion civil defense program prior to the attack.

FREMONT-SMITH: Which one would you like to start with?

TAYLOR: Maybe the real one.

AYRES: I'd like to interject a statement about the water supply. In some cases, as, for example, New York and a good many others, there would be plenty of water unless the Kensico Dam in White Plains happened to be destroyed. The pipes are deep and under very high pressure. Although some of the surface pipes would be broken, you would have water. In cases like the cities along the Great Lakes, which get their water from the Lakes, the water supply would depend upon the electric pumps. To the extent that they had reserve power, they would have water. To the extent that they didn't, it would depend on whether the main power supply was operating or not. In a lot of cases it would be operating because, as I mentioned earlier, generating equipment tends to be relatively hard compared to other facilities. Many other cities get water from wells, and again it depends on whether they have reserve pumping capacity and whether they have to depend on the main power. In some cases the main power would survive or it would be possible to get power from another area through

inter-ties. In some cases it would not survive. So you would get a whole spectrum of situations and certainly in some cases there would be no public water supply and then you would have the conditions for a disaster.

DUNHAM: Wouldn't it be true, though that in practically all cases the water would be greatly reduced?

AYRES: Yes.

DUNHAM: So that in an emergency situation we can get along with ten percent of the normal water requirements. I include bathing and everything else.

AYRES: Industrial water use would be out immediately.

EISENBUD: Again it depends on your assumptions. If, for example, New York had fires, they would have to fight the fires.

AYRES: That's true.

EISENBUD: And in most cities the water supply provides the drinking water and it's the same water supply with which you fight fires. Thus, I think it's possible to visualize a situation in which a city like New York would be without an effective water supply.

DUNHAM: Yes, but you don't have any other requirements for the 48 hours you're fighting the fires. You're not going to fight fires for 10 weeks.

AYRES: There's enough water in the pipes in most places to last for quite awhile if you're not using it normally.

FREMONT-SMITH: Are we planning this for the spring or winter?

AYRES: This depends, you see, on whether you have warning and whether you've been properly told what to do. One of the first things you should do if you have warning that there may be an attack within the next few hours is to get buckets and tubs and fill them up and make sure you have fresh water. Of course, all of these civil defense shelters are supposedly stocked.

DE BOER: This is really the first point that I would like to stay with. Where is the machinery which will enforce this and how well are they informed on this?

CASARETT: Wouldn't it be true that in an unprepared situation the best source for general communication and control, governmental control, would be the Armed Forces? In other words, military takeover of government?

DE BOER: That's precisely the way I look at it. As I look at the military, where I work there is no plan to take care of this. At least, there is no published plan that I know of.

CASARETT: That's what I meant by an unprepared situation. The only sensible thing would be to use that organization which is distributed and organized in such a way as to take over; that seems to me to be the only one which could probably handle the situation in an unprepared state.

DE BOER: All right, what guide lines do they have?

CASARETT: Are they ill-prepared?

HEMLER: They have some fairly strong guide lines with respect to takeover. This happened, if you recall, in Hawaii right after Pearl Harbor. There was a complete takeover and then a later movement down from complete martial law to partial martial law.

EISENBUD: Somebody isn't thinking it through, though, because when the power went out in the Northeast 2 years ago I was really shocked to find that most of the radio stations didn't have their own power supply and Kennedy Airport was closed down because they could not operate their lights and equipment, which shows that either the thing hadn't been thought through or that even the most superficial precautions have still not been taken despite the fact that in 1967, some 22 years after we first began to talk about this. . .

AYRES: The electric utilities did not think that such a power failure was possible. After it's happened they still don't think it's possible!

WARREN: Let's go back to this martial law business for a while. I don't know whether you remember the discussion and the bitterness that was created by the unwillingness of the General there to stop and give up his martial law authority. He had fairly good reasons, I suppose, for maintaining his control of the situation.

FREMONT-SMITH: Is this Hawaii?

WARREN: This is Hawaii. This led to tremendous discussions in the first Civil Defense program we had about the whole matter of the declaration of martial law at zero hour and what was involved and the struggle by the states, the legislatures and others in the public group to avoid martial law. Yet it is inescapable that there has to be some kind of an organized control that will assure that the mayor's functions, the supervisor's functions and the governor's functions will continue.

One of the primary needs is the police power required to handle the distribution of the food and all the public functions that our society needs in its regulation. This argument held back the acceptance of civil defense for several years finally, and I don't think that the matter has completely been lost sight of. It's a covert factor even now because the control of a military type has got to be dominant in the first period anyway when chaos and hysteria have to be reduced to bring about orderly reaction to this problem. This comes right down to our behavior again. What will the criminal element, or the groups that go on their own and do what they please, do about their own activities?

HEMLER: As a result of the conflict you are talking about, the feelings that occurred after the Pearl Harbor situation, the government did a great deal of studying and came up with some definitive conditions whereby martial law can be declared by a commander down to the local level. These are pretty clear cut now. There are several conditions, and I can't cite them offhand. One that I do recall is that if communication is not possible, then he does have the authority—and this has been tested out in the Supreme Court, as a matter of fact—to declare martial law. It has been clearly defined now, or much more clearly defined than it was at that time or later on during the Detroit riots, during World War II, and several other incidents that have occurred since then.

TAYLOR: Does anybody know if there has been any indication of more difficulty in the population doing what they are told by the authorities in, let's say, Britain or London during the blitz compared, say, to Hamburg; one a city in a dictatorship presumably more used to iron authority, the other a city in a democracy, not so used to iron authority? Are there any others in which the reaction of the population to the authority is any different, any cultural or political inertia that carries through to the point where the disaster actually takes place?

6 OF 7

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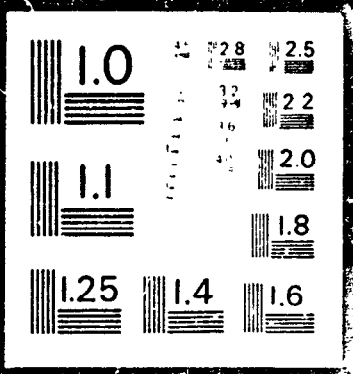

WARREN: One of the things that came out of many of the early discussions—and Ralph recalls this probably more vividly than I do—was the agreement from the studies made on these populations after the war. If there was prompt leadership the situation could be quickly brought under control, but with the lack of this leadership then chaos was apt to occur. The leadership could be indicated by the Civil Defense arm bands or some insignia, a visor cap or something that would indicate that the individual who took the authority under his wing did have the right to do so. There are many illustrations of this even today. You've been in a traffic jam after a football game and there will be a crossroads with no signal lights and the cars get all fouled up. But if somebody, a civilian, happens to step out in the center and begins to put a little order in it, in a little while he's able to unsnarl it just because he did something logical and had shown some leadership. If this is built into the Civil Defense program all the way through, you have some chance of a normal reaction. So we need some plans. At one time we had a very good plan and organization with widespread community participation in comprehensive detail at all levels of community and government. Probably the peak of it was at the end of Mr. Peterson's tenure as Civil Defense Director. At that time I think we had the best capacity to mount a Civil Defense effort nationwide with a pretty good organization and thinking through of the problems of transportation and communication and food and medicines and what kind of shelters would be useful. Of course, computations of the fallout and the number of megatons used were very much less than now. Nevertheless, the principles and policies are still pretty sound. They need improvement and extensions. But the community participation has fallen away.

AYRES: I would not argue with the implication that these have gone downhill. The amount of money available and morale may have, but I think there have been continuous improvements in the planning.

WARREN: On the planning, yes, I would agree with that because the planning, after all, can be done by a small group. But in a disseminated net of civil defense. . .

TAYLOR: I'm confused about one thing because during those days—I may have been in an unusual situation at Los Alamos—we had a civil defense program. There were block wardens and they had hats and arm bands and I had the impression that was true in the rest of the country.

WARREN: Yes.

TAYLOR: There were specific individuals called block wardens in most of the cities in the United States. I believe that's no longer true.

WARREN: That was true.

FREMONT-SMITH: It died out.

TAYLOR: So, I would question whether we've gone up. I think we've gone up with respect to the staff. But that's important because in those days, even though he may not have had any equipment, at least there was a fellow with a tin hat and an armband who knew he was supposed to take charge.

AYRES: On the other hand, I'm thinking of these specific vulnerabilities, many of which are less now than they used to be and also the doctrine which up until very recently has been very unrealistic.

TAYLOR: The doctrines are in the report and nobody knows what is in the report. That is the trouble.

WOLFE: You've got to remember that 2 out of 3 of these people for all the missions, the wardens, are unavailable for the jobs. Any committee that is set up in the community is only going to be one-third of its original size—if you can find those members.

SPEAR: No, I think you would have to say that there would be large centers taken out, where the deaths are very heavy. But in the areas where the immediate effects of the attack were not felt, you would have reasonably complete small communities. In other words, this isn't 2 out of 3 in every community across the nation.

WOLFE: It's 2 out of 3 in the 200 targets.

SPEAR: Yes, but roughly a hundred of the targets are in the counterforce attack, where few people live.

AYRES: I think you must distinguish between the situation where people have had warning and are prepared and where you don't have warning. I don't think the latter is a very realistic assumption, but it is the assumption that many people base their plans and calculations on.

WOLFE: Do you mean the enemy will lose half of the effect of his attack by telling them?

AYRES: Do you, after all these years, believe in a surprise attack out of the blue?

WOLFE: I believe it.

DUNHAM: Let's define what we mean by a surprise attack. Do you mean an attack in which there's no sign in advance that anything is going to happen?

AYRES: Yes.

WARREN: Today or tomorrow?

AYRES: Right.

HEMLER: Your first knowledge is that the weapons have hit?

WARREN: Yes.

DUNHAM: Suppose there was a condition in Hawaii where there was a build up and the Army there had a good plan and everybody was set and unfortunately the precise instant was not known?

AYRES: You may not know the precise instant but even so you should be alert because you are waiting for trouble and therefore you should be listening to the radio.

TAYLOR: Say a tactical warning would be ignored. You didn't pay any attention it; it was tense in the sense that it could logically lead to a nuclear attack all of a sudden.

HEMLER: And it was the beginning of tension.

FREMONT-SMITH: I don't think you can make an arbitrary statement of this sort. I think you don't know what might have come and when you don't know, therefore it could have happened.

AYRES: I don't believe that there was the slightest chance, at the time, that the Cuban situation could have led immediately to an attack. It certainly could have developed into a more tense situation, which might have developed into something.

FREMONT-SMITH: I don't believe there's agreement in this room with you.

AYRES: You expected an attack any moment?

FREMONT-SMITH: No. I happened to be out of the country at the time; I didn't know anything about it, but I'm sure that many people had no idea. But I don't think it's a question of any definite relationship between people expecting an attack and the reality as to whether there is a possibility of an attack.

AYRES: My point is that the real possibility was much less than the expectation. If you do expect the possibility, then you'll pay attention to tactical warning.

HEMLER: I think it should be pointed out that at the highest levels, at the military and the governmental and the civilian levels, there was just what you were saying; there was a belief that an attack was not imminent.

TAYLOR: What's it going to take to make people believe that it is imminent? I wish someone would describe. . .

AYRES: The use of a nuclear weapon somewhere in the world, for example.

TAYLOR: That's a much more tense situation.

UPTON: For instance, we learned that in the recent Middle East crisis the hot line was in use for an extended time. Something might have touched off an international incident there involving the major powers. I think all of us watched developments very closely, but I'm not aware that there was any attempt to put civil defense on the alert.

AYRES: It just wasn't tense in those terms. You probably just haven't thought through how much more tense it can get before the bombs start dropping.

UPTON: When do you begin to alert your civil defense?

FREMONT-SMITH: When it's 50 percent tense?

UPTON: When you have noticed that the missiles are aloft?

HEMLER: Just to take an example here, there are conditions of readiness that the Armed Forces are in at all times and there are degrees. They're actually defined, and the same thing is true with your civil defense forces, the same thing is true with your governmental officials.

As the policy makers decide that a condition has been reached—and they have very definitive criteria for deciding this—the Armed Forces move to a new condition of readiness and this goes throughout the world.

FREMONT-SMITH: This is announced to the world?

HEMLER: By instant communication. Let me point this out. As they move through these conditions of readiness, the forces get more and more alert. More and more people get involved. The point I think that Bob is making here is that during the Cuban crisis, for example, the readiness of the Armed Forces did not get down to even the level nearest the condition, the final condition.

TAYLOR: Remember Pearl Harbor.

HEMLER: They were several conditions away from full alert, but they had not reached a condition of readiness criteria where the civil defense began to be completely and wholly alerted, and so forth.

AYRES: People keep forgetting that at the time of Pearl Harbor, we had issued an ultimatum to the Japanese.

TAYLOR: That's my whole point.

AYRES: And we were waiting for their response.

TAYLOR: Bob, that's my whole point. We got right down to the wire and still the civil defense authorities were not deployed. That's my point.

HEMLER: The way that things are defined now—and this isn't particularly classified in any sense of the word—that ultimatum would now take us to the condition where all of your civil defense forces would be notified. In other words, it has been defined and the criteria have been laid out.

UPTON: I guess the thing that concerns me is that history has shown us again and again that the unexpected does indeed happen; that although we have well-laid plans to alert ourselves through a succession of stages, these are based on the assumption that time will be available to shift gears, so to speak, into high. Is this a reasonable assumption?

HEMLER: The times that they are considering available now are fairly realistic.

DE BOER: Still, if we recall the day President Kennedy was shot, we must remember the period of insecurity that followed. I dare say, nobody had anticipated that event that morning and few had any idea about what the reactions of the new President would be. No one knew precisely what had happened and the question of a plot has not been settled yet. I don't see any point in discussing the pros or cons of a plot here and now, except that it should put us on notice that it did happen and could happen again. Where I was, on a military installation, it took more than an hour before I heard anything resembling a coordinated effort to stop the tide of fear and instill confidence in the situation. I don't know how it was in Washington.

HEMLER: I was in Korea at the time and we moved to more alert readiness conditions.

DE BOER: We sure didn't where I was. At least not all officers and civilians on the base were notified to that extent.

HEMLER: Your particular base may not be one of the strategic bases.

AYRES: I think the best argument we can make now for total unreadiness in the event of an attack is this one, that people, despite having ample warning, may not believe it.

DE BOER: That is another thing I meant to say.

AYRES: That's a good argument, but that the warning isn't there, I don't believe.

TAYLOR: I think there is a much stronger argument. Let me just try to sketch this. The reason I don't believe that we are likely to get much warning, if any, if bombs start falling on the U.S. cities, is that the circumstances in which we would get warning would be

those circumstances that would be very likely to reveal the identity of of the attacker.

I think that any effort to make an attack on the U.S. on a large scale that has any possibility of occurring is one which has the property that the attacker is not known. There are five countries in the world now with the ability to attack the U.S. in a major way and, therefore, I think it's much more likely that there will be a purposeful effort to confuse us, and the best kind of confusion is complete ignorance rather than allowing a situation to develop which is clearly suicidal for the country that's going to attack us. I think that's extremely unlikely.

AYRES: I'm glad you agree that the other situation is unlikely. I think this is unlikely for the reason that if nobody is going to suspect the real attacker, there can't be much of a reason for the attack, so why do it?

TAYLOR: Because you want to destroy the United States.

AYRES: That's not a good enough reason. If there's no burning issue at the moment, it's just not a good idea to launch a nuclear attack even if you think you may get away with it because they won't know who did it.

WARREN: There's always a burning issue, isn't there, in this world?

FREMONT-SMITH: Two or three, I think.

WARREN: I'm reminded of the fact that the Japanese did some shadow boxing on this. Their ambassador was in Mr. Hull's office when they attacked Pearl Harbor. The Japanese ambassador could very well have committed hari-kari in the corridor of Mr. Hull's office because he lost face by this act of his government which used him as a pawn for deceiving.

Now, I've had a little bit of private horror thinking that the Mongol is quite devious in this way and in their long-term plan it would not be beyond them, under conditions of warfare to make every effort to be affable, friendly, open, everything just wonderful. It's the old trick of inviting your enemy to a big banquet to celebrate your peace and then poison his cup. On the day of the greatest evidence

of peace, when all of the people are relaxed and celebrating the great success of peace, that's the day you'll get a surprise attack.

HEMLER: Maybe that's the one criterion we should stick to in our readiness conditions! [Laughter]

WARREN: Yes, a healthy military suspicion of a nation with nuclear bombs promoting exaggerated peace proposals. It's not the day to shut down our alert system.

AYRES: What you say is not wholly unbelievable, but you are still talking about an attack by a much weaker power and they can never be absolutely sure they won't be found out.

WARREN: Yes and no. It could be a strong power.

AYRES: They certainly have very little hope of destroying us completely. So it's not like Cicero and Carthage.

WARREN: China is in that situation, isn't it? They can't invade us without more preparation and yet there is every indication that they are getting ready for something.

AYRES: There's every indication they are ready to deter people. That's about all.

FREMONT-SMITH: This is an interpretation. It's not the only interpretation, is it?

AYRES: It's not the only interpretation but I would point out that they have left Hong Kong, Quemoy, and Matsu alone.

FREMONT-SMITH: This would fit with Staff's opinion.

AYRES: It's convenient for them to pretend that they are great enemies of the United States but, in fact, their immediate interests are on their borders.

DOBSON: It seems to me that we have been talking about the likelihood of war. This is an extremely important issue. But would it not be even more pertinent for us to consider what the situation would be in the event a nuclear war actually occurred? Perhaps we should assume that a large war had started, or to make it easier, that something smaller had happened.

The reality of a limited thermonuclear war can easily be visualized as follows. Since it is well known that China is now in possession of nuclear weapons, the Soviet Union, taking advantage of the difficult position in which the United States finds itself today in Vietnam, might choose to accomplish a major double purpose for itself by carefully but quickly increasing world tension to fever pitch over an invented but highly advertised imminent attack by the Chinese on U.S. forces in Southeast Asia. Then, in one way or another, arranging the secret delivery of a serious blow to American military operations in Vietnam by means of a "small" or "medium-sized" nuclear device, the U.S. would immediately loudly protest and decry this atrocious, inhuman and unpardonable aggression on the part of the Chinese. Knowing it was most unlikely that the U.S. would attack her, because of the deterrence of her vast strike-back ability, the Soviet Union would have seriously damaged the American war effort in Vietnam, would have caused this country an immense loss of face before all the world, and would have invited us to attack China. She would have presented the United States, by this one simple maneuver, with a grotesque situation and a critically important decision: Should we retaliate? Against whom? China? The Soviet Union? By means of thermonuclear weapons? A "small" one—on Peking, on a selected Russian target, on Chinese atomic installations in Sinkiang? Possibly the decision would be to refrain from nuclear retaliation and to take other steps.

We could make other assumptions, that it was the Chinese themselves that did this, for one reason or another. Many variations on this theme can be imagined. But in any case, the important thing is that somebody, not us, exploded a nuclear device in Southeast Asia. It would be, to say the very least, enormously provocative. And even apart from questions of retaliation and escalation we would still have the aftermath of limited nuclear war, even after this one single thing.

AYRES: That's the kind of thing I believe in.

DOBSON: And so we might get started.

AYRES: I think that could happen and, of course, if it does happen, first of all civil defense becomes a very popular idea in the United States. It will be thought of in a totally different way than it is now.

TAYLOR: Isn't it probably true of any nuclear explosion killing a lot of people anywhere in the world at any time?

AYRES: Yes.

HEMLER: You could also take one city in the United States as a scenario. Take New York with a big population and then answer some of the questions that are in my mind. In other words, if we had one city hit like that, one area, what would be some of the psychosocial effects there?

FREMONT-SMITH: And in the unhit cities, too.

HEMLER: Would it be realistic to assume, for example, that no help would come from the outside immediately—and I wonder about this—from the people unwilling to go in? Certainly there would be military who could be ordered in, but I'm talking about people around New York. These are some of the questions that bother me. This is one small area.

EISENBUD: I think this is a very good question because it makes it possible to focus on a situation that is realistic.

HEMLER: Yes. That's what I'm thinking of.

EISENBUD: I think that's very worthwhile. Of course, the first thing that would happen would be that those people who were able to would want to leave New York and there would be a tremendous wave of population leaving the city, and I suppose people in the suburbs would want to stay there to make sure they can hang on to what belongs to them.

HEMLER: Would they allow them to leave?

ROOT: Would they allow them to come in?

EISENBUD: Let's think what it would be like in a city that's just been struck. What do the streets look like? You've got fires, you've got debris. Do you have people carrying on any kind of police function?

DUNHAM: They would be trying to but not very effectively. I think New York is unusual in this in that it's difficult to leave New York.

EISENBUD: No. What I'm thinking of now, Chuck, is the problem

UPTON: How big is the weapon? How far off the ground? What's the nature of the burst? How big is the fireball?

EISENBUD: Let's consider one or two weapons detonated in such a way that it produces essentially complete destruction of part of the metropolitan area and fallout all over the rest of it.

HEMLER: I think the type of scenario there for New York is two to three 10-MT type weapons.

UPTON: How many people are left on Manhattan? Do you have to think about them at all?

EISENBUD: It could be all of them. We play games.

WARREN: Based on buildings?

EISENBUD: We play games and we've had bombs detonated in Newark and the Bronx and in Manhattan. It was reasonably intact except for the fallout. There was a lot of damage but you didn't have mass casualties on the level that you have with a direct hit. But the point is what do the streets look like.

UPTON: This is basic information, and, considering how you evaluate the situation, you have to supply the conditions before you can consider them.

TAYLOR: I think in some areas the streets I would visualize would be full of people running as fast as they could if they knew that the fallout would not get down to the ground for a couple of hours.

AYRES: But they wouldn't know that, because nobody would know that at that time.

CONARD: I think that most outside people would be scared that there's another bomb coming and they wouldn't want to do anything but get in the shelter and wait until they're sure that there would be no further attack.

EISENBUD: The best situation would be where you were able to instruct your population to get into some sort of protection, and then I ask again what would the streets look like? Not everybody is going to follow the orders given to them over the radio, assuming that they hear them.

AYRES: I think the streets of Manhattan would be extremely dangerous.

MILLET: It seems to me most likely that people would huddle together in the lowest possible spot and would wait there hoping that things would pass over and probably begin to come up at the most dangerous time, because they would not have enough information.

AYRES: It's quite possible that they might be better off—the best shelter spaces, of course, are in the middle of those buildings—in the thirtieth and fortieth floors of those buildings.

EISENBUD: Don't you think that those people are going to seek their families at whatever cost?

DUNHAM: It depends on where they are and what information they have.

DOBSON: Another question is how many people would know what happened if there had been a surprise attack and, let us say, a single explosion? How long would it take those who survived and were anywhere near the scene to know what had happened? Would they have radios or television or some other source of information?

AYRES: They would have radios and they would probably get to them. That would be a first response.

WARREN: The broadcasting on the tops of buildings would be out.

AYRES: Anybody with a portable transistor radio. There are stations all over the place.

EISENBUD: There was a terrible feeling during the blackout for a half-hour or so.

AYRES: I listened to the radio throughout it.

EISENBUD: Yes. But what happened was that first many of the larger stations went off the air. So you knew something was wrong. The lights were out. So you would look around for a radio station on a portable radio, if you had one, and you found that some of the biggest stations were off the air. I think that one of the big ones located in New York had an emergency power supply powered by a

Diesel engine for which they didn't have fuel. So they sent somebody down with a can to get some fuel and found that the gas station pumps were all knocked out because they all operate with electric power. And somebody had to struggle over to New Jersey, where the lights were still burning, to get a couple of gallons of gas and come back so that they could get back on the air. You can see how complicated this is and how subtle these things are. But it was a horrible feeling to be listening to these reports of the power going out first in one city and then in another without an explanation.

SPEAR: With respect to that point about the electric gasoline pumps, I recall a very interesting moral. Back in the early days of Federal Civil Defense, when we were trying to anticipate all of the things that could go wrong, we stumbled upon this problem of electrically-driven gasoline pumps, in which case there might be plenty of gasoline in underground tanks, but with the power off it would be inaccessible. We presented this problem to the major oil companies and asked them to solve it. Only one of them came up with a recommendation to its dealers that, in addition to the usual array of electrically-driven pumps, they should have in each station one old-fashioned hand-cranked pump for emergency purposes. The recommendation wasn't too popular because it involved an investment they really didn't think they were going to need and one that would never really pay for itself, and not much was done.

In 1955 I happened to be up on Cape Cod at the time when the hurricane had just hit and power was out all over the Cape. As I drove along that afternoon I saw that every filling station had solved this problem in a variant of a single way. You take a plate off the front of the pump and inside there's the electric motor and a belt for the pump drive. In some cases there were small boys on jacked-up bicycles with a rope activating the pump wheel. In other cases they had old power lawn mowers hooked up to the pump and they were in business and selling gasoline. The moral to this story for me is that if you want to solve a technical problem, don't go to the executive, go to the guy that operates the gadget! [Laughter]

HEMLER: Going back to the question you were addressing here earlier, and that's how soon people would know. In the case of the blackout, people knew there was a blackout; they knew about that instantaneously. What they didn't know were the specific details of how it had occurred.

AYRES: And when it would be over.

HEMLER: And when it would be over. I think the same thing would be true with a detonation. I think they would know instantaneously that an explosion had occurred. Whether it occurred in other cities, whether it was a mass attack they wouldn't know.

EISENBUD: Of course, what I was referring to is the feeling that I think many of us have, that there was something extraordinary happening that just wasn't going to be explained by the fact that there was a minor break in a relay up in Canada. It looked as though there was a deliberate plan, for a few minutes anyway, to sabotage the Northeast.

HEMLER: But in this case you've gone to the ultimate already, you've reached it.

UPTON: If such an event were to occur without warning, who would take over, and how soon, and how would information be obtained concerning the precise whereabouts of the event and the measurement of radiation levels and that sort of thing?

MILLET: That would depend on who was left, wouldn't it?

HEMLER: That's true.

ROOT: Or on planning.

DOBSON: If one just hypothesizes a single weapon instead of three over New York, is this taking liberties with your question?

UPTON: Yes, surely.

DOBSON: If one hypothesizes a single explosion over New York City, I think there is a rather important question to ask to get started realistically: who takes over and how does he or they obtain the information necessary and to whom do they give this information? Who are the remaining people? What do they do? Who helps them decide what to do? Is it possible to help them decide what to do?

TAYLOR: In the world as it is today or otherwise?

UPTON: Today. People will need information; they will be tuning in the open bands expecting some direction, and where will it come from?

TAYLOR: In the hit area I don't think anyone has been designated to take over. The leaders will rise out of the crowd.

AYRES: No. There are plans. I don't know if they are uniform in every state and I don't know what they are, but the OEP is the office where the plans are. They have all kinds of lines of authority.

TAYLOR: There's one man in Manhattan, maybe he's dead.

UPTON: Did this function during the blackout? Did he then take over? Did it work?

AYRES: It usually goes through the states. The state governor has people, and so on. I can't tell you the details.

UPTON: We had such an experience in the blackout. Did the plan work? Did he take over?

AYRES: That wasn't such a situation.

HEMLER: Just to give you an example of what could occur—and I've been through some of these scenarios in military courses and war games— the United States is broken up into districts, military districts, and we have First Army Headquarters located in New York. All of the First Army is not located in the New York area, and the commander of the First Army and his alternate designees have criteria established for them whereby they take over, the first condition being that they must contact local or state authorities, if possible, and work with them. It's not a question of the military taking over; they work together with civilian authorities. If the city authorities are no longer available or the borough authorities are no longer available and can't be reached, the military commander reaches progressively up the civilian chain of command, if you will, until, if necessary, he reaches the state governor; then he works with the state governor.

AYRES: But in the blackout case there was no breakdown.

HEMLER: There was no breakdown. They were in constant communication.

CONARD: The civil defense set-up in Albany, New York, is quite elaborate. I've seen it and it has multiple lighting systems for all

parts of the state and representatives everywhere so that if something goes wrong anywhere he's immediately notified by direct telephone.

HEMLER: To answer your question, there's a chain of command, both civilian and military, to take over under the appropriate conditions.

UPTON: So in point of fact, during the blackout the proper authorities were in communication and it was decided that the plan need not be implemented on that occasion?

AYRES: The President was in touch with the Director of the OEP and the Director of the OEP was working on various aspects of it. But there was no need for anybody to take over because there was no hiatus.

HEMLER: And the national command centered in turn on the military side, were in complete communication with the civilian.

TAYLOR: What do you mean by takeover? This is a semantic question. If you mean by taking over sitting at sort of a control center and giving orders, there are people set up to do that. What I mean by taking over is telling people what they should do, mouth to ear. I don't think these people are designated.

EISENBUD: The first thing you would need is a system for damage appraisal, and I don't think it exists.

AYRES: There is one.

EISENBUD: I know; I've seen them. For example, somebody asked how long it would take for a person to find out what the radiation situation is; I think it would take forever because in most large cities they depend on a few monitors of doubtful availability, on instruments which, from what I know, may not be in working condition.

AYRES: I think your doubts are well-founded.

EISENBUD: Yes, and they are going to use systems of communication which simply won't exist, as I say, in New York, for example, with its twelve channels. That was a few years ago; they may have corrected it now, but even if they have 40 or even 100 channels, that still isn't enough to get the information in from the hundreds of monitors to interpret the data and get it into a form in which you can make

decisions. Then you need physical damage assessment. Who's going to get it out?

HEMLER: This is the question. The first things that you were asking about, the detonation location, and so forth, can be determined very quickly, assuming again this is a localized situation. There are aircraft and other instrumentation methods that can move in quickly and get this type of information. The question of on-the-spot physical damage I think is probably the one that you are talking about. It is not so well defined as to who does this, where it's passed to and where the assessment occurs. Where the assessment occurs could be well defined. It's how the information gets there to be assessed. It's well defined as to where it occurs in several places. It's not defined as to when it would arrive or who would provide it.

SPEAR: A question was raised a little while ago about the plan for issuing information. I can't pretend to be completely current on this now, but I'm sure we all remember some years back there was something known as Conelrad. This was a system designed to get out at least a minimum of information while denying electronic radiation as a navigation guide to bombers. That was thrown out a while ago and in its place there has been established an Emergency Broadcasting System.

The programming of this has been the subject of a good deal of planning between Civil Defense and broadcast officials. The emergency lines have been installed, bypassing the major centers. A good question, of course, is what information is to be broadcast? Here I'm not current by any means, but when I last had contact with it, the information, before one knows what the situation is, was the kind of thing that is found in the little yellow booklet. At a time when some of the general precautions are certainly in order for the population, the policy would be to give out as much useful information as possible on protective measures. I don't know whether you hear this around where you live, but as I listen to the radio in my car every now and again there is a beep, followed by the announcement: "The next minute will be devoted to a test of the Emergency Broadcasting System." This, of course, is testing the hookups to be sure that they are working.

AYRES: Incidentally, as of last summer FCC was very seriously considering a test program in which there would be a switch installed on a number of radios which could be triggered by the broadcast itself

so that in the event of an emergency your radio would go on. I don't know how far they've gone with it. This was a year ago.

HEMLER: They've been discussing this quite widely with manufacturers, in order to get it to a satisfactory automatic thing.

ROOT: Will your transistor go on?

HEMLER: I would imagine that this requirement would be one of the desired characteristics.

ROOT: What radios, other than transistors, would go on if the electric power was out?

DOBSON: The question is could such an automatic switch be practical on a transistor radio?

TAYLOR: The receiver has to be on all the time.

DOBSON: It has to have energy. If the batteries are turned off, there's no energy.

TAYLOR: You can do that; you can just have it run on all the time. Why not just leave the radio on?

MILLER: I guess the impression from what has been said is that if there is a local incident, there is nothing to worry about—that everything is under control.

HEMLER: No. I believe there is a lot to worry about. That's the point I was addressing myself to a while ago, the question of what happens to the people themselves when this occurs. These are the questions I think we need to address ourselves to, particularly in this section right here.

EISENBUD: I'm still trying to visualize just what the situation would be like within the first hour, let's say.

AYRES: I would agree with that. A lot of things that people think haven't been done have in fact been thought about but there are still uncertainties and very bad problems and those are what we should discuss.

HEMLER: The reactions of people.

UPTON: Wouldn't people want some information first of all? You would want to know whether you're safe where you are or whether you need to go someplace else.

TAYLOR: The only people who can know that are people with some understanding of these things right with the people and I think that's why I keep harking back to this business of there not being a large number of individuals who are designated before an attack as being the persons to whom people will turn. When 18 people go into a room, if no such person is designated, probably someone will emerge as just sort of taking hold, the way people always do. That's why I say the leaders will just pop up; the real, on-the-spot leaders will just appear.

FREMONT-SMITH: The information wouldn't be available locally.

TAYLOR: They do the best they can. They'll make decisions of all kinds that they never imagined they would ever make before.

HEMLER: And presently the information will get to them.

TAYLOR: Part of the time they will tell people to do exactly the opposite of what they should do, and I think that's the difficulty. This shouldn't be the case. People should be designated and have some idea of what the right thing to do is and what's the wrong thing to do, but that situation doesn't exist.

AYRES: You still have a credibility problem. Even if such a person were designated, is he believed?

TAYLOR: If he's got an armband and a tin hat or something.

EISENBUD: Is the confusion in this discussion due to the fact that the experience of Hiroshima, the Marshall Islands, Hamburg, London, is simply not applicable to what we're talking about and we have to agree with Kant that all knowledge has to stem from experience and we just haven't had the experience? Is that a fair assumption? Is this why we're beating around the bush?

MILLET: I would back that one, yes.

FREMONT-SMITH: I think it's semi-fair.

WARREN: Some of it has been covered but not all of it. Can we go back to some of your prognostications? I have a clear idea of how deep we would fall and how much deeper the gradual decay would be before we would have an uprising, but I don't suppose one could put numbers on this even if the assumption of 1,400 megatons on the United States is true. Can you? You've been thinking about it.

AYRES: You mean numbers like production, potential production?

WARREN: Yes.

AYRES: Roughly, I'd say of the order of 50 percent of raw productive capacity would survive, with actual production in the first year somewhere in the neighborhood of 15 to 20 percent of pre-war.

EISENBUD: Let's talk about something we do know about. We know what Japan was about and we know what Western Europe was like, a good deal of detail. Suppose Japan didn't exist, what would have happened in Europe? Would that typhus outbreak in Italy have been a serious situation if we didn't rush doctors in with serum? What would have happened in Japan?

AYRES: I should imagine that both Europe and Japan would have been many, many years recovering without the help that we pumped in. I think it might have been ten years.

DUNHAM: I think we're talking about two different things. One is talking about epidemic disease and the other thinking more of economic recovery.

EISENBUD: No. I'm talking about everything, Chuck. I feel, from the little bit that I saw, that the key to survival in World War II or the key to recovery was the fact that we came out of it with an intact productivity that was of enormous potential and we had the will to use it to help both enemy and friends.

Now, I think even with conventional weapons one could extrapolate what we saw in World War II by a factor of 10, let's say, and increase this level of destruction tenfold and then extend that to the United States on the assumption that the weapons deliveries systems would have been different had the war been fought twenty years later. One could visualize very serious problems in survival even with conventional armaments. Now, to extrapolate that further to nuclear weapons, I wonder if we don't have a body of experience that would make

it possible to discuss the thing intelligently. This is the thing that Stonier has done. Drawing on the knowledge of microbiology, he has gone very extensively into the problem of disease and also the problems that will be created by the fact that the population would have to subsist at a very low level and under conditions which would involve very great individual hardship. It's not the same kind of population that emerged from wars centuries ago when there would be total devastation, but these were people who knew what to do. They knew how to go back to the hills.

AYRES: Today we are much better off rather than worse off in many ways. We know about boiling water, for example, to sterilize it. There are a lot of things that we know which they didn't know after the Black Death. This kind of knowledge is very widespread; it isn't just limited to doctors or the health people.

WARREN: I think that's right but I think the question that bugs me is what other nation is going to put their treasury in our recovery as we put our treasury in theirs?

AYRES: Keep in mind one thing in discussing this. After World War II, the U.S. GNP was 200 billion. It's just on the point of 800 billion now. If we lost half of it, a GNP of 400 billion would still survive.

WARREN: That's right, but suppose we lose more than half of it? A psychological feature of this that's really important is: do we have friends who are like relatives and help us or do we have friends who don't? This is a competitive situation.

DUNHAM: Or friends like other relatives!

WARREN: Yes. They could very easily help downgrade the gross national product by putting us off, too, couldn't they, and by doing everything they could to prevent our recovery in the world's markets?

AYRES: Assuming somebody else survived in a more intact way than we.

WARREN: Yes. Your point is that South America hasn't got enough potential to influence much in the way of our recovery, because of their low economic level.

AYRES: No. Only Russia or the European countries as a group.

WARREN: If they were badly knocked out, this would be serious.

EISENBUD: I would think the Latin American countries would begin to slip badly if Western Europe and the United States lost their potential.

CONARD: We would have to stop to help them!

MILLER: Don't you think it would be very unwise to count on any help?

AYRES: Yes. On the other hand I don't think I would worry too much about being kicked in the pants, but it does depend on how the war was fought. Not only who won but how it started. If we have too much of a guilt complex about it and if other people think that it's our fault, that could be a very bad situation.

WARREN: Isn't it funny, though, when the chips are down in the long run it's our image and personality that are still so important.

TAYLOR: If that occurred, it means to me we won the war, because if we didn't win the war it's going to keep happening. There's not one drop; there is a drop in living and another drop in living and another and another and it goes on and on and on and when it stops it's because we won the war. The only way we won the war is by destroying all their weapons or because they have used them up, one or the other. So I think . . .

EISENBUD. You can have a condition of mutual stagnation.

EISENBUD: Staff, you must have given a good deal of thought to the situation you saw in Japan because you got there almost ahead of anybody else. As a physician, what do you think the public health problems might have been in the first year without outside help?

WARREN: Well, they were potentially very bad. If we hadn't used DDT right away I think epidemics would have gotten loose. This is quite obvious in Hiroshima when I first went there. The flies were so thick that you could see them on people, as they walked by, as polka dots. It was hard to tell whether the white cloth was white or whether it was polka dotted. Within a week they were gone and this had a big boost on the morale of the inhabitants because they knew

now that the United States was helping them and also that the hazards of typhoid and cholera and dysentery were not greatly reduced.

LANGHAM: Let me ask, did Japan herself have the capability of doing this thing if we had not done it?

WARREN: No. We had upped the production of DDT. This was the first use on a large scale outside of Guadalcanal by our own forces. Isn't that right, John?

Their community medical-public health programs dealt mainly with nutrition and keeping up production. Active tuberculosis involved almost twenty percent of those under military age, requiring lots of sanitaria. The mortality was high. One of the Japanese doctors complained to me, "If we put in this public health policy that you have demonstrated as being so effective, we'll have more people to feed and employ. We have neither the food nor jobs. It would make our situation worse." They were somewhat satisfied in a hard way to lose some who were ill because illness put a strain on their economy.

I was much more impressed by their wartime brutal military point of view than several here who have been there after the war. Some of you were incredulous when I said that the Japanese military had a policy that every woman had to have a child almost every year, or she was put to work in the factory or field after other males in the community had tried to get her pregnant. I was told this by several responsible Japanese, so that I was convinced of its truth. After Muckden, the Japanese military planned to train and equip and put in to the field a million new men each year for ten years, when they would be ready for war. They could afford to lose a million men a year. Thus, baby production was a very important consideration. The war's end saw the emancipation of the women.

The general disrespect for life was a cultural thing. After all, it didn't matter too much if they died. They had a chance of improving their lot when they went to whatever was in store for them. This was part of their culture and was the basis for the bonzai charge and the kamikazi activity. We wouldn't in our culture have introduced kamikazi procedures. I doubt if we could have made them stick, but this was tied in with the religious ceremony at the time the young man graduated and was ready for the great honor of his final assignment.

During the war, the medical schools and community practice followed the German policies and practices. The full professor was chief of the department, and his word was law to those beneath him. Advancement was very slow. The Dean did not consult with his faculty, and made his own policy mostly after the German system. The texts were mostly in German. A few faculty had some foreign training in Germany and the United States. Tsuzuki spent two years in the University of Pennsylvania while he apparently was a ranking Admiral, yet as Lauren pointed out, in the presence of the Herr Geheimrat he was a "little boy" in the medical school hierarchy. We met no health officers in Nagasaki, Hiroshima or Tokyo, only members of the local TB sanitaria or medical school faculty. Neither the prefectorial governor of Nagasaki nor the acting mayor of Hiroshima refer to a health officer, only to local practitioners who were advising them in a nonofficial status.

SESSION VII
PSYCHOSOCIAL REACTIONS (Continued)
(Initiator: Merril Eisenbud)

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SESSION VII
PSYCHOSOCIAL REACTIONS (Continued)

PROBLEMS OF POST-ATTACK RECOVERY

EISENBUD: There seemed to be a fair amount of interest on the question of what would have been the fate of the heavily bombed nations following World War II had outside help not arrived, particularly during the first critical winter. John, you've indicated some interest in this. Do you want it or should we just throw it out?

HEMLER: Well, as we were talking this morning it became apparent that in order to extrapolate the way we need to for a general war situation, we have to move quantum jumps from the situations that we've experienced in the past. This would require a rather large number of assumptions and, of course, a number of studies have been done in this area. I think there are many questions that need to be addressed in this type of general war environment, but I thought perhaps as another intermediate step in the route to that quantum jump extrapolation it would be worthwhile discussing Japan, using one single assumption.

We have, in the case of Japan in World War II, a country being hit for a number of years with a large number of conventional and incendiary weapons so that a good amount of its industry and its population had been hit and destroyed. Then, coupled to that, two nuclear weapons were detonated to provide the radiation environment, however limited it might be. I thought perhaps there might be several things we could do with just one basic assumption and then perhaps several variations of the theme. The basic assumption is similar to the one that was mentioned at the end of the last session, that no outside help comes after this catastrophe. It's a major catastrophe—not only a nuclear catastrophe—but a conventional war as well. Suppose that no outside help had come. Could we extrapolate from there and then, perhaps to give us a little variation here, maybe move the weapons in position and/or location—just the two weapons that we're talking about here. Whether this would be useful, I'm really not sure.

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AYRES: I think we have a good lesson from World War I where after Germany was defeated instead of coming in with help, we blockaded it. That didn't prevent German recovery. Actually Germany wasn't physically damaged, which is a good point, but their economy was almost wrecked by the inflation.

FREMONT-SMITH: Was the inflation due to our blocking them?

AYRES: Yes, substantially so.

HEMLER: But in World War II we had even more than that situation as a scenario really. We have it where the economy has been wrecked, where its cities and industries have been largely destroyed and we do have the radiation environment that we've been talking about.

FREMONT-SMITH: In Japan.

HEMLER: We have what you might call a limited nuclear war, a situation that we've been mentioning here, one type of weapon on New York and that sort of thing. I think somebody has mentioned the fact, I think it's true, that we talked best from things that we have had experience with, realizing, of course, that there are some culturaal differences that have been brought out in the last several days. I was just wondering what people thought about it being appropriate to make that type of extrapolation with that single assumption rather than trying to go this large scale route in a big hurry.

FREMONT-SMITH: Do we have data as to how soon and how much we brought into Japan? Was it almost immediately? It was in some respects of course. You brought plasma in almost instantaneously.

WARREN: Yes.

FREMONT-SMITH: What else happened?

HEMLER: By September 1st we had forces in there. Well, Stafford, you probably are more familiar with this.

WARREN: You mean as far as forces were concerned? As soon as the surrender was over, of course, we had this huge fleet in Tokyo Harbor and they went to shore and began to spend money.

HEMLER: Within 30 days.

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WARREN: You mean as far as forces were concerned? As soon as the surrender was over, of course, we had this huge fleet in Tokyo Harbor and they went to shore and began to spend money.

HEMLER: Within 30 days.

WARREN: Within less than that.

HEMLER: Yes. That's what I said.

WARREN: I spent fifteen cents to buy a couple of frogs, which astounded the Japs. After that, the rumor went around that there were all kinds of things for sale. They expected us to take them and this little incident was almost a diplomatic event. We must have poured an awful lot into Japan in a relatively short time in just the fact that the civilians were paid for things that they did for the military occupying forces.

HEMLER: Right. What I'm saying is, again taking the same scale here, the prologue, the logue, and epilogue, how far down would Japan have gone?

FREMONT-SMITH: How much further down? Did her economy continue to deteriorate after the war was over or did it start to come up right away because of what we brought in and what we sent?

WARREN: I can't answer that because I didn't pay any attention to it particularly.

AYRES: Jack Hershleifer ran a study (Reference 58) on this at RAND, and drawing on my memory of this work and not any personal knowledge, I think the situation deteriorated for a time.

FREMONT-SMITH: You mean some months or what?

AYRES: Possibly a couple of years. It's perfectly true, of course, that immediate starvation was avoided, but we're talking now about the economy.

WARREN: Yes.

AYRES: He believes that the Japanese did some very unwise things with regard to currency controls or something of that nature. I'm not very clear on what happened or what he thought was wrong about it. But the actual recovery didn't start to really gain momentum for about four or five years.

EISENBUD: Jack Schull can help us on that.

WARREN: There was a plan to reorganize the Japanese Government with the introduction of new governmental policies, the setting up of voting and its procedures, and then advice to the new Japanese Government on what they might do during the time their financial policies had to be rearranged. Wasn't there some kind of a struggle between Mitsubishi and other cartels for dominance?

AYRES: See the prewar Japanese "Zaibatsu." Mitsubishi, Mitsui and so forth, were broken up.

WARREN: Yes, but somewhat later. This shook their whole structure to the bottom because then they had to start over and there was no machinery for setting up a large business, although the Japanese traditionally had done a lot of manufacturing in their own homes and did this very intensively during the war.

AYRES: 1950 was about the beginning of the real upsurge.

WARREN: Yes. It must have been delayed that long.

EISENBUD: That, of course, coincides with the Korean War when we had put in about $700 million a month there for a few years.

SCHULL: There are some unusual features about the Japanese situation which must be borne in mind. Much of Japan's steel-making industry was still intact at the end of World War II; Yawata, for example, which accounted for about fifty percent of the pig iron and steel was quite limited in the immediate post-war period; Yawata was operating at less than a quarter of its 1940 output. Many factors contributed to this diminished productivity, and not the least of these was the proposed dismantling of the mills as part of the reparations to be paid to Russia, China, and the like. The inventory of Yawata, a tremendous installation, was not completed until 1949 or thereabouts, but shortly before MacArthur announced there would be no further dismantling of industry for reparations. Thus, there was a hiatus of four years or so when, I assume, the Yawata complex could have been more fully utilized to support the Japanese economy than it was.

HEMLER: Suppose an opposing force had not been in there?

SCHULL: As far as I know there was no reason why they couldn't utilize this plant again. Japan, of course, in this context is vulnerable

because she doesn't have significant deposits of iron ore. The ore would have to be obtained elsewhere.

HEMLER: So they couldn't use it.

DUNHAM: There was a lot of scrap around.

HEMLER: Where did they get the coal?

SHULL: There is much soft coal to be found in northern Kyushu in Nagasaki, Saga, and Fukuoka Prefectures. I've always assumed the existence of this coal was one of the justifications for locating so much of Japan's steel industry in and near Yawata.

ROOT: They also had spared the Imperial shipyards which were being used for the repair of American ships. Then Kaiser came in and leased the shipyards to build his revolutionary giant tankers. He taught the ship builders his methods, contracted out a lot of construction and suddenly all the unemployed ship workers had jobs and a big expansion was underway providing desperately needed tankers to the world. This was a kind of fateful spinoff. Kaiser had gone around the world trying to lease shipyards and they had all started up production except Japan which had been held back by the Occupation.

DOBSON: Merril, could I go back in time a bit in this hypothetical consideration and ask Dr. Warren about the possibly very significant health problems, pestilences and the like, which, without outside help to the Japanese, might have modified this kind of economic recovery? I wonder if he would be willing to either tell us or to speculate what he thinks might have been the situation without DDT and without the other medical and hygienic measures that we helped them with?

WARREN: Do you wish to limit this to Hiroshima and Nagasaki?

DOBSON: I thought maybe Japan as a whole.

WARREN: As a unit, it has been mentioned before, they lost, presumably, effectively more than half their people and in effect their whole metropolitan area was essentially abandoned because there was nothing left there to develop any kind of activity. Even the gas tanks were ruptured, so there was no gas. The power stations had been badly bent so that it would take some considerable repair to get them back into being. I don't know about the hydroelectric power sources, but there was almost no power there. The hospitals, of course, were

gone. There were some outlying sanataria where the surviving doctors collected. In Nagasaki, which had part of the town left on the south and where the prefectural governor presided, he still had the majority of the staff yet in hand because he himself was not injured in any way. He organized the feeding of refugees and the injured. In fact, this became a complication because the place of a patient who died was apt to be taken by somebody who just walked in and laid down on the mat hopefully to get a handful or a couple of balls of rice and some tea, which was issued by the Prefectural Government. So this was a way of getting some food. They had to be given the bum's rush to make place for those that were injured.

With the lack of people in the area there was no health problem as such outside of the casualties. But the potential was there certainly. The water was off for quite a few days because the leaks were just everywhere and they had to shut off between the intact and the damaged areas in order to get water pressure back in the center of Nagasaki.

There was another serious problem in Nagasaki. The honeybucket system continued to work from the south part of the town. But the barges began to overflow because the men who managed these barges had been killed or died or fled. Anyway, there were none available. So the manure, the human manure, just piled up there and this made a very bad stench and a very bad fly problem.

FREMONT-SMITH: On the barges?

WARREN: On the barges. They would just bring up the honeybucket, and the barges were full so they would just dump it all into the estuary, and it was just on the surface everywhere. So this problem created an emergency which the Prefectural Governor somehow got the manpower to deal with. He had the barges towed out and the disperal of the contents was then carried out in the usual channels because outside of the city area the farmers were ready and organized to receive the honeybucket materials. But I would say pretty nearly a month went by before that really got taken care of properly. The life of the rest of the town went on pretty well, I think. The restaurants worked and they had no trouble getting food. Of course, you would naturally expect we would get preferential treatment.

DOBSON: But supposing the Americans had not been there and there was no outside help, what would have been the situation?

WARREN: I think they would have had to do something like this anyway. After all, these were sophisticated people; they had a pretty sophisticated government; they had a big industry there. Of course, the military establishment was the major support of Nagasaki. Its people mainly perished.

UPTON: What fraction of the population perished or was incapacitated?

WARREN: My figures, of course, are greater than others, but I get nearly 100,000 in Nagasaki, which is well around half. It was the whole Urikami Valley clear to the terraces all around down as far as the Mitsubishi Works near the gas tanks.

UPTON: It left a great many people, though, who could begin quickly to institute emergency measures.

WARREN: Yes.

FREMONT-SMITH: These 100,000 perished or were incapacitated and perished?

WARREN: I think perished.

FREMONT-SMITH: Then there must have been quite a large number that were also knocked out but were.....

WARREN: Yes, but a lot of these perished right away.

FREMONT-SMITH: Right. How many would you say were capable of functioning? That was really your question?

UPTON: Yes.

FREMONT-SMITH: 50,000 do you think?

WARREN: Well, another 100,000 in the town, say, within a day. These were around the corner or across the Bay, you see. But they didn't want to go into this area. This was a different kind of explosion and the word got around awfully quickly that this was different.

EISENBUD: You see, the situation would have been very different if instead of being air bursts these were surface bursts; if instead of

being 20 kilotons they were more like 100 kilotons or a couple of megatons; and, let's say, in addition to Hiroshima being bombed, Kure, a short distance away, was also bombed.

WARREN: This would have eliminated the help.

EISENBUD: And this really is the extrapolation that's hard to make. For those of you who haven't seen it, I would urge that you look at the maps that are included in the volumes of your Joint Committee on Atomic Energy showing the fallout patterns throughout the United States and particularly of the eastern United States coated with lethal levels—superlethal levels of radioactivity. In other words, levels in excess of 400 or 500 r total.

WARREN: This really wasn't much different from Yokohama or some of the other places except it happened in one day.

EISENBUD: That's right. So this really is the bridge that we find hard to cross because of lack of experience.

DUNHAM: Let me ask a question, Staff. It may sharpen up this other thing again. From what you saw and heard, even if these cities had been isolated and no help had been able to get in in any big way, such as DDT, would there have been a real problem of epidemic disease other than insect-borne or tick-borne? In other words, would typhus be the critical one you would worry about?

WARREN: Well, typhus coming from returning soldiers.

DUNHAM: But would they have been able to handle the ordinary water-borne diseases in the absence, say, of a flu epidemic which nobody can either increase or decrease? Typhoid or cholera—would it have been thinkable or unthinkable?

WARREN: This matter came up in the first week because of the bloody diarrhea which the patients had who had the gastrointestinal destruction, and for a couple of days they tried to mobilize whatever resources they could get to deal with the impending cholera or typhoid epidemic. This consisted mainly of getting ready to throw out the tuberculosis patients who could be handled outside, and expecting to bring in a limited number of casualties to these two or three tuberculosis hospitals which were on the outskirts, maybe five or six miles out. But other than that they had no resources to deal with it.

You see, a lot of the doctors and nurses died, leaving, let's say, three-quarters of the staff of the Nagasaki medical school who were in a very strong concrete building or who were out of town that day. But even there they had brought in patients on doors, and I suppose a large number walked in. But then the short circuits in the Nagasaki medical school had caused fires which burned up the majority of the wards. I think the second and third floors were pretty badly gutted everywhere; the beds were burned, and the bedding was burned because of the maplewood flooring which conducted the fire through the building wherever there was a little breeze downwind. So I don't think they could have done a great deal.

DUNHAM: I'm not talking about treating epidemics, once they have occurred. Would just their natural habits and degree of sophistication have seen to it that you didn't have widespread water-borne disease?

WARREN: The chances of it being water-borne would depend upon how good the chances would be for the reservoir in Nagasaki and the ones in Hiroshima to be contaminated. I think this is pretty near zero, although there were people living on the hillside above the reservoir, but it's not very likely that they would have harbored some returnees who brought the disease in and that it would then spread from their honeybucket toilet arrangements into this reservoir.

DUNHAM: It takes an awful lot to contaminate a very large reservoir effectively.

WARREN: That's right.

DUNHAM: With the delay in pipes and things.

WARREN: All of the other arrangements which are essentially barriers to the threat of the disease. It's a different situation where you've a stream from which a lot of people drink, and this was not the case here. I doubt if anybody drank out of the stream that ran down the center of the valley. They drink hot tea, not cold water.

CONARD: How long did it take for voluntary medical personnel from outside communities to get there to assist them?

WARREN: Well, I don't think any came in outside of the teams that went down there from Tokyo and Osaka to study the situation. There were several medical officers who came in with a little Japanese major from Fure. But they didn't do anything but look. In Hiroshima, of course, the mayor died and most of the city officials were casualties one way or another. So they couldn't act. But in this book that somebody mentioned it gives a diary of the time. It's quite accurate in what happened there in trying to set up a city government to make some sense out of their problems. They got clothing and food and things from volunteers, but it took six months or longer, I guess, before this came in in any organized way. That was only after people began to come back in.

EISENBUD: I wonder what fraction of the food consumed by the Japanese in the first year was brought in. Was there a significant fraction of it brought in?

WARREN: Only until the local crops were harvested.

DOBSON: Was there a rodent problem of significance in either of the atom-bombed cities in Japan?

WARREN: There was almost no animal life. We saw one dog in Nagasaki and he was a great big police dog. He wandered around like a wolf for a couple of days. But there were no bodies around, no rats, no cats yet in Naha. On the other hand, when we went through Okinawa, there were lots of rats and the stench was pretty bad even though Naha had been bulldozed flat then. But there were still available parts of bodies, I guess, if the rats dug down. I suppose there was a considerable amount of food that had been destroyed by the shelling and was available even after the bulldozing. Okinawa had large farms There was a big farming area around there and some of the forests and wild lands, so the animal population was available. But around Nagasaki and Hiroshima, while there was a lot of bamboo, it was pretty heavily cultivated, and I imagine they had hunted down and consumed things like rats. There was little food for pets and there were no game animals.

CONARD: Would you say the medical facilities in Japan had been pretty well exhausted because of the long war?

WARREN: Yes, they were short in all medicines.

get there in 1953; from 1948 through 1952 the people looked about the same. They were pretty shabby; I couldn't help but feel sorry for them. Most of them had pre-war clothing and sweaters with lots of holes in them and beat-up old sneakers and things like that. When I got there in 1954 there was a big difference. The girls were beginning to look after their appearance, and there were new clothes; you began to see that they were on the upturn.

TAYLOR: The question was how much of that would have happened without outside assistance?

EISENBUD: The impact of the Korean war was enormous in the parts of Japan that I saw up around Tokyo and down around Hiroshima where the U. S. and Great Britain had bases but I don't know what the figures are in relation to their own economy.

FREMONT-SMITH: So it wasn't our effort to help Japan but it was our fighting the Korean war that helped them, incidentally.

TAYLOR: That was of the order of 10 billion dollars a year, I would say.

EISENBUD: Probably.

TAYLOR: That was what, a few percent, 10 percent? What would one say the Japanese gross national product was?

SCHULL: I have no idea.

TAYLOR: Was it $15 billion a year?

EISENBUD: It was a few hundred dollars a year per capita in the early 50's.

WARREN: We didn't have any drives to supply cooking utensils like we did for the Germans and the Austrians.

TAYLOR: It was a big chunk, wasn't it?

WARREN: So, in that sense, our population didn't contribute directly to the recovery as they did with the Europeans.

CONARD: Which would be in contrast to the situation in this country, if we had a sudden atomic war; we would still have in the fringe areas many medical personnel and supplies available. The situation is not exactly comparable in that respect.

WARREN: You will recall their policy of putting a million new men a year in the field equipped and supplied. This included all of the necessary accessory military and medical equipment and fuel. The home front began to sag and then the military support began to suffer.

In the last year they could not do this and caused a very severe crisis, so that there was a minimum and in many places no replacement of supplies including medical instruments and medical supplies. Part of this was due not to the amount of damage to their cities but because they had no more imports of steel and aluminum and they had stripped all the bannisters and all the statues and everything that had metal. The only thing that didn't go was the big statue of Buddha and things like that, but even the shrines were deficient in metal.

BRUES: When I was there in the second winter they had not replaced the glass in the hospital windows in Hiroshima because of the cost in fuel of making glass.

WARREN: Fuel was very scarce.

BRUES: Yes.

EISENBUD: Hiroshima has a fairly mild climate, which is helpful.

WARREN: Yes.

EISENBUD: In contrast, let's say, to the northeast United States where in the wintertime you would have problems that the Japanese didn't have, even in Tokyo.

BRUES: Yes.

WARREN: Tokyo gets pretty cold.

EISENBUD: Tokyo gets pretty cold but they don't have the heavy storms that we get; subzero weather.

WARREN: No.

TAYLOR: Does anybody have any idea of the amounts, in dollars and cents, of materials and supplies and money that were furnished the Japanese by the U.S.? In other words, what kind of support was really given to Japan from the end of the war until 1950 or so? Was that half a billion dollars a year of which half was money and half was supplies, or what? Does anyone have any idea?

EISENBUD: Well, in 1950 the figure that sticks in my mind is some $700 million a month. Is this too much that was being spent in Japan, $700 million a month being spent in Japan in support of the Korean war? This had a tremendous effect on the economy, but I could be wrong with that figure. It's something I could check.

WARREN: Undoubtedly right. We deposited a lot of K-rations in Japan which were given to the Japanese as an easy way of supplying food. We had a big excess at that time, too, but it didn't come until after many months.

EISENBUD: Yes.

TAYLOR: Did this really make a big dent in the Japanese food problem?

WARREN: Well, I think so. There must have been millions and millions of cans.

DUNHAM: When did the rice rationing go out, Dr. Schull?

SCHULL: Rice rationing was still in effect as late as 1953 when the clinical portion of the genetics program was terminated. There was access, however, to much more non-rationed rice, and as a consequence many individuals no longer felt the same compulsion to make certain they obtained their rations. The government still maintained the ration machinery for some years after 1953, but more and more persons who could pay the higher prices had turned to the free market.

DUNHAM: But you would say that it was probably critical for at least five years?

SCHULL: Yes. It was certainly still critical in the early 50's—say 1950 and 1951.

EISENBUD: I noticed a big change from 1950 to 1954. I didn't

DUNHAM: Let me ask you another question that may sound foolish, but I don't intend it that way.

FREMONT-SMITH: There are no foolish questions in these conferences.

DUNHAM: In these exercises that we were having, are we to consider ourselves to have won or lost the war in conventional terms?

FREMONT-SMITH: Both.

DUNHAM: You know what I mean. What's the psychology? Did the Japanese lose and they knew they had lost? Is this situation we're trying to extrapolate to, is it one in which we lost in the old-fashioned sense and know we lost, or is it one in which, in one sense, we won but we're in a hell of a shape? I think this would make a great deal of difference in the motivation for recovery.

FREMONT-SMITH: And the morale question.

WARREN: I would like to know in that context, too, if we lose, are we invaded and do we have a local official from the foreign government sitting in Washington telling us what to do?

TAYLOR: I don't know whether the winners would do it this way but one way they could act as though they had invaded the country is to have a lot of left-over bombs which would be used to threaten us if we got out of line. Then what would get set up, presumably, would be some provisional governments of which the members would be Russian or Chinese or what have you. What would be held over us there wouldn't be bayonets but long-range missiles.

FREMONT-SMITH: How about assuming the simpler one; let's assume that we won and know it. This is at least a little bit easier to work with. Try that out first and after we've settled that one we can move on.

SCHULL: In the case of Japan, there was one special problem created by the loss of the war. The Japanese suddenly found themselves obliged to house and feed some six million individuals, civilians and soldiers, compulsorily repatriated from Manchuria, all of southeast Asia, etc. This was a sizeable burden to add to an economy which was already hard-pressed. If Japan had won the war, this particular problem would presumably not have arisen.

FREMONT-SMITH: Yes; in fact they would be sending people out.

SCHULL: Yes.

CONARD: What was your impression of the psychological reaction of the war on the Japanese people as you observed it?

SCHULL: How they respond to it now? Then?

CONARD: At the time you first saw them, did you see any residual psychological effect in the people that you could relate to the war?

SCHULL: No, but it would have been a very difficult evaluation for me to have made then. I spent the war years in the Pacific and must confess my reactions, when I first landed in Tokyo after the conclusion of the war, were ambivalent. I frankly didn't know whether I harbored any residual hostility to the Japanese. If I did, I may have over-compensated with time, at least so my wife accuses me. I've much admiration for Japan and its culture, and may, as a consequence, have lost the objectivity to critically evaluate certain aspects of that culture and the people. It is my impression, however, that most Japanese felt they had been badly misled in getting into the war in the first place and few seemed to feel any personal guilt. Some were stunned by the events, I suppose, but most seemed prepared to accept, almost enthusiastically, the dictates of the occupying forces.

UPTON: I would like to ask Bob Miller. He spent a good bit of time over there. How would you respond to this question, Bob?

MILLER: Unfortunately, Dr. Lifton should answer, but he is not here. It was my feeling in the pediatric clinic, without being able to document a word of it, that the most prevalent late effect from the atomic bomb was the fear of late effects—a fear of all somatic and genetic consequences.

FREMONT-SMITH: From the bomb—not from the war but from the bomb particularly?

MILLER: From the bombs, right. But the physical effects were very infrequent. They were large enough to show that they were the result of radiation, but they were still small in numbers. We did see children and mothers who were fearful of late effects, and sometimes

incapacitated by such fears. I don't know what the numbers were, but I thought they were large.

DUNHAM: Was it 10 percent or 50 percent?

MILLER: I would have guessed that it was 50 percent or perhaps even higher.

DUNHAM: You mean these people were essentially not contributing anything to society?

MILLER: No, but they were living in fear; fear of marriage when they got older, fear of having defective offspring.

MILLET: What percentage did you say?

MILLER: I said I would have guessed over 50 percent had this apprehension of late effects; premature aging, premature mortality, leukemia, children with congenital defects.

CONARD: In the Marshallese there were many fears, too, about the fallout. For instance, they attributed fish poisoning, that has been going on for years in these people, to the fallout. If the arrowroot flour is improperly prepared it can cause a soreness of the mouth and digestive tract and they immediately started claiming that this was due to the radioactive fallout that had gotten on the plants. You do have these psychological factors that need an awful lot of reassurance by competent authorities.

UPTON: Didn't you say, Staff, that they were thoroughly demoralized; that there was speculation about a poison associated with the detonations, which caused panic and hysteria and anxiety among the survivors? Then they saw their fellows suddenly sickening and dying after they thought they had escaped. Families lost dear members of the household after a time when supposedly they had escaped unscathed. This led to a succession of recurrent waves of alarm.

WARREN: That's right.

UPTON: And depression.

MILLET: Over how long a period did that go on?

WARREN: Well, for about two to three months, and by that time most of those who were going to die had died. But they were still alarmed about whether this would not flare up again and there were lots of questions on this.

UPTON: I can see how this would very seriously complicate the work of an emergency team. If the leader, so to speak, on whom all reliance was placed, developed radiation sickness himself and had to be replaced, and then his successor dropped dead by the wayside, this would be a very demoralizing and disruptive influence.

WARREN: That's what happened with the Mayor in Hiroshima. He died. Also the commanding officer of the contingent bivouacked in Hiroshima and most of his aides suffered a delayed death.

DUNHAM: I would expect this to go on much longer in a fallout situation.

UPTON: Yes.

DUNHAM: It would not all be LD-50, a 30-day sort of proposition with a few stragglers up until a month or two later, but you would expect certain groups of people to hit their peak mortality at sixty days.

MILLET: How did the leadership develop after these officials disappeared?

WARREN: Well, a young major came over from Kure and tried to take command, and, of course, he did not have any great number of troops available. He had been bombed over there, up at Yubo, and he didn't know quite what his prerogatives were, but he couldn't bring in any food although I think he attempted several small provisions of food. In this chaotic period the strongest personalities will begin to emerge—the man with leadership. He appeared as the acting mayor who came up from the ranks, began to get clothing and organized the people and began to get food, and was finally the mayor. But they had a couple of false starts, too. This was rather slower I think in developing than it would be in our country. You see, they really were under this military regime which, up to that point, sent down dictums from above and the gendarmes relinquished their control rather reluctantly, but finally did. Of course, they had no mobs to deal with or anything of that nature.

EISENBUD: After World War I, I seem to recall that there were bands of children who in a few years grew to be adolescents and roamed around eastern Europe, but I don't recall that anything was said about this after World War II. Was this because in these countries they took care of the orphans in better style or what? Or was the economy left at a higher level?

DUNHAM: I don't know, Merril, but I have a feeling that this did go on in Central Europe after the war but for only a year or two. You read Russian stories of children, their own children.....

EISENBUD: Yes.

DUNHAM: I don't know where it would be documented, but it was a transient thing.

WARREN: You recall the wolf children.

EISENBUD: Yes. That's in World War I, I think, where I first heard that word.

DUNHAM: I think the same thing happened.

ROOT: You had it in Germany in all the bombed out buildings and things, but I do think the mass of American support that came in counteracted a lot of that.

HEMLER: During the first year they had a number of children and all of your various so-called DPs wandering, trying to get away from Germany or wherever they were back to where they had come from.

EISENBUD: Yes.

HEMLER: It took over a year to organize these people—to get an organization going where camps were established. Although I say over a year for the complete organization, in many cases it occurred as the armies moved into Germany. It was a full year before they got a complete organization where they had full communications so that messages could be transmitted back and forth on people regarding their relatives, and before food supplies were adequate and not rationed, and so forth.

WARREN: I understand from some of the earlier meetings that we had long ago that the worst police problem was the person looking for his wife or family or child. Murders were committed; they would steal food and disrupt communities and they would be gone the next morning or even that day, looking, looking, looking, and they wouldn't subscribe to any discipline. They would do no work because they hadn't found their family yet. We went to great trouble in Civil Defense to try to devise a way of reporting survivors and their location and get communications back and forth to locate absent members of the family.

One of the important elements that would determine whether a civil defense organization would stand still for the attack was the safeguarding of the family for a man who had a responsible position. Could he be guaranteed that his wife and children and other dependents would be safe while he carried out his assignment? I don't think this has been solved yet.

HEMLER: The military has set up a rather elaborate network for doing just this.

WARREN: Yes, a very elaborate one.

HEMLER: Because one of the original problems considered was the fact that here you have a commander of a garrison and no matter how he may be regimented, if he doesn't know that his family is well taken care of, how well will he react to taking and to giving orders?

WARREN: Yes. Would he go looking for them and abandon his post?

HEMLER: Right. Not only him. He may be regimented enough but how about the men under him; the troops that he needs to use to direct and control.

WARREN: That's right.

SPEAR: This was brought out also in the "Goebbels Diaries" (Reference 59). I recall that he was quoting Albert Speer (Lo kin!), who reported that where German production centers had been heavily bombed and damage had been done to the factory and workers as well as their homes, they were able in a fairly short time to make repairs that would have kept them at, say, sixty-five percent of production. But they couldn't even make a start on significant production until the

workers had somehow made arrangements for their families that seemed adequate to them. They wouldn't come back to work even in a regimented society like Nazi Germany. I think this is a very important consideration in planning for constructive recovery efforts, to make provision for the family to be reunited.

EISENBUD: How long does it take for an unattended factory to get so rusty that it can't be put back into operation?

WARREN: I've heard a figure of twenty-four hours in the rain and maybe three days in dry weather. Factories, in general, particularly those that have heavy machinery where the bearings can get rusted, get frozen. If there's no roof, then they get wet and this makes a very difficult maintenance problem. These figures were quoted before I went to Japan to see what the percentage was. The engineers in my party were to try and estimate how much of the production of the Mitsubishi Works was put out of action.

AYRES: You said seventy-two hours would be the upper limit for a shutdown?

WARREN: Then you've got a problem of complete takedown of the moving parts and relubricating the bearings and removing the rust which forms quickly if wet by rain.

AYRES: Before you start up?

WARREN: Before you start up again. Depending on your manpower available and a few parts, this determines the time.

TAYLOR: I wonder if factories in the United States that depend heavily on electronic programming are more or less susceptible?

WARREN: And water damage? Didn't we have some experience with typhoons where the roof was taken off of factories where there was a lot of electronic gear? That would help in this.

TAYLOR: There may be a high short-term susceptibility in the sense that it's easy to knock out the control system but it may be easier to put it back into operation than the more old-fashioned type of system. I just have no idea.

EISENBUD: I don't know. I was amazed to find after the blackout

that there was so much turbine damage in the Northeast and the turbines now are very large. They are building them at one-million kilowatts. This put a real serious crimp into the area of this power supply; they had so much turbine and generator bearing trouble that a fairly large percentage of the generators had to be repaired.

TAYLOR: That was just because a very sensitive thing was knocked out.

EISENBUD: Yes.

DUNHAM: There was a hurry to get it back but after one of these deals, what's an hour or two extra?

EISENBUD: Yes.

MILLER: It has been shown that when the Japanese thought they were threatened by fallout they became panicky, whereas in Palomares, where fallout really occurred, the Spaniards did not become panicky. What would be the likely situation, say, if a similar thing happened in Berkeley and fallout fell on the campus? Would the people there be more Spanish or more Japanese in their reaction?

WARREN: Civilians would be abandoned except inside the university building! [Laughter]

DUNHAM: You can't jump from an alpha situation where Wright, in all good conscience, could reassure them that here wasn't any important problem, to a situation where you have highly radioactive, penetrating fission product radiation which does require that you put something between you and it. I'm not sure that you can jump from one to the other. But your question is still a valid one.

HELMER: I would say jump from one to the other.

DUNHAM: Yes. How would the people react?

MILLER: What would happen? Would Governor Reagan go on the radio and reassure the Berkeley students? [Laughter] Is there a plan of action to reassure the people should fallout occur in California?

WARREN: Yes, a very extensive plan was worked out statewide many years ago, but I think the best-controlled plan was the radiologi-

cal defense plan, because this was at the height of the emphasis on this subject. Plenty of money was given in the early days to organize the program; lots of equipment was available. Chuck may remember when this test at Nevada went into Los Angeles; we had a complete contour by the next morning.

DUNHAM: Yes.

WARREN: The word came at four o'clock from Nevada that it was likely to happen, and by the next morning we knew the contours, if any, all over the state. Then there's a good level at which a certain occupancy or non-occupancy can occur and there are extensive instructions for cleanup, which probably won't work.

DUNHAM: It will keep the people busy.

WARREN: Yes, it will keep people busy.

SPEAR: Is that degree of preparation still existing in California, Staff?

WARREN: The University maintains a pretty tight organization partly on the need to monitor the laboratories using isotopes and nuclear machines. Countrywide you will find the isotope people hooked in with the students' health and safety policy on the campus. Isn't this true at Ann Arbor? And as far as the universities are concerned, their local problem is probably in good shape organizationwise. They may not have a director who runs the Civil Defense but there's usually a man who has responsibility, most often, I think, in student health.

EISENBUD: Staff, we are contemplating situations in which presumably attenuation factors of the order of 100 would be needed over relatively large areas to protect people, so that the dose on the outside could be something like 40,000 r total which would be several hundred or maybe a couple of thousand r per hour initially.

WARREN: That's a pretty severe restriction since we were making all of our original estimates on the basis of a bomb or two. Now, I don't know. I'm not one of the team any more, so I don't know how these adjustments have been made.

EISENBUD: Yes. These are the things that bother me and still do.

Many of the Civil Defense handbooks at the local level were finished back in 1950.

WARREN: Yes.

EISENBUD: The handbooks were based on the nominal bomb concept where the military people told us that nobody would want to waste a bomb on the ground because they could get more higher up. This is what we were told in those days and the instruments are geared to handle them with essentially no radioactivity or levels that can be dealt with.

AYRES Are you sure of that? I don't believe that's so.

EISENBUD: Yes, it is so. Well, I don't know of any large city, for example, that has anything other than hand-held counters and detectors that have to be taken out into the area and read; and then you've got to go somewhere, to a telephone usually, because they don't have their own communication system.

AYRES: Are you talking about instruments or doctrine?

UPTON: Radiation monitors.

EISENBUD: What I'm saying is that the monitoring system.....

AYRES: I thought most of the peacetime monitoring was primarily the responsibility of the Division of Radiological Health of the Public Health Service.

EISENBUD: No, it is not. Why would that be?

WARREN: I insisted when I was a member of the Scientific Committee that the Air Pollution Control put up fixed radiological monitoring stations with communication to the central station. So, in Los Angeles, San Diego, San Francisco, and I think in Sacramento, there is a constant background reading. Part of that is to get some experience because occasionally from the former testing we had some fallout. Whenever there was an opportunity for a Russian or a Chinese measurement, these were made too, routinely, even if they showed nothing.

UPTON: Are they supplied by emergency power?

WARREN: Yes.

EISENBUD: How many do you have, do you know?

WARREN: I think there are seven in Los Angeles with pretty wide coverage; I don't know the number, but it's comparable in San Francisco; and Oakland and Berkeley I think have some. Now, of course, the big radiological laboratories all have background counts daily, so that they would be able to give a quick count. There are many in industries (like North American where, during the episode with the Nevada fallout the background of their instruments rose when people tracked it in from the parking lot).

EISENBUD: Yes. Of course, these instruments would be totally ineffective in a high level situation, Staff.

WARREN: Yes, but they would get an alert. They would get some idea.

EISENBUD: Yes. One of the critical things, in contrast to what we have all experienced where a bomb goes off a hundred miles away and you get a fairly uniform level of fallout, is that in an attack situation the gradients would be very steep. We have seen on the board here that the gradient between the northern Rongelap Islands and the lower islands, which were only 30 miles away, was what, a factor of ten?

CONARD: Yes from 150 to 2300 rads.

EISENBUD: More than ten. So, you see, for decision-making purposes a man needs better information than you can get out of relatively few instruments. On the study we made in the metropolitan area, we included something like a hundred automatic stations (I don't remember the exact number we decided we needed), and the cost of this was just prohibitive in terms of what people were willing to provide in those days, and for that matter still are.

DUNHAM: Of course, if you had planes you could do aerial surveys which would be quite helpful.

EISENBUD: We looked into that, too, Chuck, and concluded, largely based on the experience that we had developed in the later tests out in Castle, that the planes would become so contaminated that they couldn't

get the information in the first hours when you need it for decision-making purposes. You can get it after the stuff had blown away.

DUNHAM: After that, yes.

EISENBUD: Right. In other words, it could be tragic if you keep all of your police force underground because you think that you can't sacrifice them by sending them out, if it turns out that the levels were actually low enough so that they could go out and perform their duties. On the other hand, if you had misinformation you could send them out and kill them with a few thousand r. So the information has to be good, it has to be delivered promptly, and it has to be delivered with a fair amount of detail with respect to geographical coverage.

FREMONT-SMITH: And it has to be believed.

EISENBUD: I just don't know of a single system that would do that anywhere in the United States. I may be wrong, because I haven't had contact with it in the last few years.

WARREN: I dislike very much the thought which is in the minds of certain mayors that I've dealt with—"Well, if we get an alert, we'll put up all the equipment that you want and you won't have this gap." This came out in the discussion with the supervisors, too, but they agreed to these seven stations with power and everything. The reason for the emergency power that we were so quick in obtaining is that the same need comes up for monitoring of the ozone and other instrumentation for carbon monoxide, to avoid having a power failure, which is not uncommon in our area for small periods of time. These stations all have emergency power for active monitoring of equipment for radiation safeguards. If each had been put out there by itself we would have had a more expensive installation to supply it and we might have failed to get them.

DUNHAM: Merril's proposition is that you've got seven stations for the whole State of California.

WARREN: At first this was the case.

DUNHAM: Maybe it's 50 now, but it still doesn't give you a real pattern.

WARREN: If you get one reading of 1000 r, that's enough. These cover the wind patterns of our Los Angeles basin very well.

DUNHAM: Sure.

WARREN: You know then to be very cautious in policing and other things that would require exposure on the streets.

EISENBUD: Yes, but what I'm saying, Staff, is that an efficient recovery requires that people have to make decisions, have prompt information about radiation levels, blast damage, and I don't know of any system for getting this.....

WARREN: Yes.

EISENBUD: .....for getting this to the officials in a reasonable period of time. I do know that in Nevada, with the best of equipment and for all practical purposes an unlimited number of personnel, it would take at least a day to construct the isodose levels with sufficient detail so that you can really make a decision within a factor of two or three as to what the level was. If you had an area where it says the total dose is going to be 200 r and it turns out to be 600, that is a dreadful mistake. If you keep the people out because you think it's 600 and it's only 200, then you may have wasted lives.

Now, in the Pacific on the scale of superbombs it used to take two or three days to work out the isodose patterns. You had to fly in a lot of territory.

WARREN: There are some benefits from the visual aspect and drift of the cloud and in knowing your local meteorology. If you can locate the site, then you can guess at the upwind safety aspects from fallout. In our case we are very fortunate because we have a mountain essentially in the center of the town from which Civil Defense has a lookout and monitoring station. You would know right then—headquarters would know and would have on their network information that there was a visible detonation in such-and-such a quadrant, and other information as it develops.

DUNHAM: Let me put out another question just to keep the ball rolling, and that is, is it safe to assume, on the basis of the Bravo experience, that unseen fallout is probably non-lethal?

CONARD: I would guess yes.

DUNHAM: I don't know. Do we have any experience other than with coral fluff which apparently is quite readily visible even to the Rongerik people?

EISENBUD: Well, Chuck, if it's going to do a lot of harm, it has to fall out.

DUNHAM: Eventually fall.

EISENBUD: It has to fall out within a few hours. In order to fall out within a few hours, the particles would have to be greater than about 20 or 30 microns and they would have to exist in large enough numbers so that I think you could see them.

DUNHAM: Is this important?

EISENBUD: Yes.

DUNHAM: Better than a reading?

EISENBUD: No, I don't think it is because a fallout won't be white and this is something that bothers me.

WARREN: You're thinking of 100 miles away where you can't see the column, are you not?

BRUES: You can see the burst.

WARREN: Because a column, in general, would have some moisture in it.

EISENBUD: You can't always see the column anyway, Staff. I mean it could be a cloudy day and you would have lots of dust kicked up. You see, we have exploded these things under rather unusual circumstances necessitated by the fact that they are tests. Over the desert you don't have the conflagration that you would have in a large built-up area. You know better than I what Hiroshima looked like just a short while after the fire. I don't think you could see the column very far away particularly if it was in the superton range, but you're going to have fallout because you have fallout, not radioactive fallout. Every fire produces fallout of carbon some distance away. I've seen it in New York City from forest fires up in Canada. This could affect the

population and you would have to have a very quick means of determining whether or not this stuff was radioactive.

The other thing I've often wondered about is how the character of the fallout would be influenced by the fact that now, instead of scavenging the condensation and having a fallout of relatively small particles, this carbon stuff would build up in the fallout system. Is this going to have characteristics completely different from anything we are used to dealing with?

WARREN: It's like the stuff that settles on your curtains and your wife has a hard time washing out.

EISENBUD: Yes.

WARREN: I don't think we have yet solved the problem of cleaning streets and surfaces and other areas which you think you should clean. Whatever you use, it just displaces the site of your contamination, and if you get it into storm sewers, this might not always be a boon. It might end up in the water supply and give you another problem.

EISENBUD: Yes.

WARREN: I don't know that we have solved this, have we?

EISENBUD: No. Very little is known about it. The other things that I think we ought to get into, which we hardly touched on, is the question of fire. I would like to ask whether anybody has experience, or knows of experiences, of how cities burn when there are no fire-fighting efforts, which we would have to assume would be the case with flash fires, fire storms and so much radioactivity that you couldn't deploy your fire fighters?

WARREN: I saw Bel Air burn and a house go in three minutes in what was the equivalent of a Bunsen burner temperature. The fire department was completely helpless because of the size of the flames, the area involved, the updraft which developed, and the fact that the very dry air just made an explosive situation.

FREMONT-SMITH: So, without any fallout to bother with. . . .

WARREN: . . . . . and no contamination to inhibit it. Now, if you had that area all nicely contaminated, spread all over, it would be different.

CONARD: We probably wouldn't have a fire storm in an American city, do you think?

WARREN: Yes. I think so.

UPTON: They had a terrific fire in McCormack Place just a year ago and that was supposed to be a fireproof building.

SPEAR: It had a lot of combustible material in it.

UPTON: Most buildings are full of combustible material.

SPEAR: That was furniture and packaging material, and so on.

WARREN: In a densely packed residential area with houses 20 feet apart, you would get good fires started. I think in our area our fire chief is just panicked all the time. If a good-sized fire started with good updraft developing, and we unfortunately had a wind, it would be a very serious problem to control even under normal conditions, i.e. water, equipment, men, etc.

DUNHAM: Was there any fire-fighting at Nagasaki? We know at Hiroshima it was essentially zero because they had no opportunity to fight it and it was a fire storm that was comparable to Dresden. But what about Nagasaki?

WARREN: They had fire lanes. This was very effective in stopping the progress of the fire southward towards the mouth of the Irikami Valley. They had a lot of antiquated equipment around. We saw a lot of burned firehouses. But they had a fire storm, too, Chuck. It wasn't as dramatically described as the one in Hiroshima was, but in about half an hour to three quarters of an hour the short circuits and the hibachis fired buildings that were collapsed. I think because of the confinement of the hills, the blast probably flattened a high percentage of these buildings in a huge area. It was a densely crowded area with lots of combustible materials. With the collapse of these one- and two-story buildings, it just went up like a torch.

SPEAR: There have been some fairly sophisticated statements of what it takes in terms of the percentage of ground area occupied by combustible buildings to support a fire storm. I forget the numbers now, but there was a lot of involvement of the National Fire Protection Association and people in this field in the early 50's based upon the

evidence from Hamburg and Hiroshima. The general conclusion, as I recall, was that there are some American cities that would support a fire storm. Of course, many more would support a conflagration.

EISENBUD: Well, wouldn't a conflagration that's not fought eventually—you've got to remember that I'm speaking now of the eastern part of the United States where I think the major problem is.....

DUNHAM: Prejudice! [Laughter]

EISENBUD: No. I think the problem is soluble in many cities in the United States, but I think when you get to Boston, New York or Richmond, Virginia, it may not be. These targets are going to be on roughly 100-mile centers, and if they are megaton bombs, the range of blast damage is out to 20 miles in the case of a 20-megaton bomb and the spontaneous ignition out to about the same distance. If you paralyze your fighting capability, or if it didn't exist in the first place, you haven't anything on that scale. In any case, people have to go underground, and then how can you help but develop a general conflagration or what you called a general fire storm? What would stop it?

SPEAR: A lot would depend, I think, on the building materials you used. To affect a city that has had a real tough fire code, with fire resistant materials, and so on, would be different from, say, a frame slum area in a large city.

EISENBUD: Every city is the same. Let me just say that most cities have large numbers of wooden dwellings and a large number of relatively old wall-bearing buildings that would just collapse from overpressure, so they would spill their contents. Would you agree to that, Bob?

AYRES: Yes. I would like to add some comments at this point. First of all, fire research is not stopped; it's going on. OCD still spends over $1 million a year on fire research and there are now some sophisticated programs. I think they've done virtually a house-by-house survey of several major cities. This work has been done by Illinois Institute of Technology Research Institute (Reference 60) and they know in very precise detail, I think, what the burning characteristics of the cities would be.

The question as to the pre-conditions for fire storm is not so clear because we don't really know what the dynamics of a fire storm are.

EISENBUD: Yes.

AYRES: There's a lot of argument over whether fire storms occur in forest fires, for example. Some fire service people say yes and others say no. It's by no means a well-understood phenomenon, but as regards your main point, which is that cities would support conflagrations, it doesn't matter a great deal whether it's a fire storm or a conflagration.

EISENBUD: Yes. I don't see why this is the point.

AYRES: One other point. You said something about ignition out to twenty miles. This, of course, is very much a function of the weather. I think at 10 megatons you get about twenty-two miles as the maximum range of ignition on an ultra clear day, if my memory is correct. But that's a rare occasion. It does not apply to a ground burst. Ground bursts are not really so good at igniting fires. In average weather, you have a lot of cloud cover, and of course about a fifth of the time in the eastern part of the country and in the upper Midwest there is actually rain or drizzle. In such conditions the radius of thermal ignition is quite small and, of course, it tends to be nonlinear with the weather conditions. So part of the time, perhaps a fifth of the time, you would get fires ignited over a large range like twenty miles. A good part of the time you would get fires ignited out to, say, ten miles, and another part of the time not much at all would happen.

Further, let me add a remark which is somewhat conjectural based on experience with forest fires—and we don't have enough experience with city fires to really be very sure. The Forest Service has four categories of conditions. The worst category is a condition where the fire is uncontrollable. This has to do with the dryness of the weather, the very low humidity, high winds and, in general, it requires something like ten days of drought before the fire. A certain number of fires occur in all times. But a great majority of the fire damage that's done by such fires is done by fires which are ignited during uncontrollable-type dry weather.

DUNHAM: One plus, two plus, three plus, four plus.

AYRES: At the other extreme, of course, there is the kind of

situation where the fire won't even ignite. This occurs about a third of the time on the average throughout the country. That's after it's been raining, or during a rain, or when everything is wet. The second category comprises situations where an uncontrolled fire would spread but it can be stopped by appropriate measures. The third category covers cases where a fire will ignite but then burn itself out even without human intervention.

The difference between nuclear war and peacetime is basically that the controllable fires will not be controlled. The fires that occur during the other conditions, in general, won't spread. If time averages and geographical averages can be equated, you could probably say that about two-thirds of the time, or in two-thirds of the country at any given time, fires would not spread.

EISENBUD That's not very good odds, Bob, when you consider what the issue is. The bad fire weather conditions occur about 5 percent of the time. So most of the remaining time the fires would spread, but not very rapidly. If anybody was around to control the fire, it would be controllable. That 5 percent is where you have real disaster. If the bombs are dropped on extremely dry days, especially if it happens to be dry throughout a large part of the country at once, the situation is critical.

TAYLOR There are a couple of complications, though, that I think may have an effect on this. One complication is that the insides of houses are dry in all weather.

EISENBUD Yes.

TAYLOR That's the difference between the human situation and the natural situation; at least the last time I got into any discussions of this with fire experts, they were still very uncertain about the conditions under which there would be fire spread from window to curtain to sofa to newspapers, and so on. There's always a lot of dry combustible material around inside, even in a very heavy rain. But most of the combustible material is dry. The problem is ignition through windows. That's why closing the windows and the curtains is such a valuable thing to do.

AYRES But let us emphasize again that most ignitions in the blast area would be caused by broken gas lines, short circuits and this kind of thing.

TAYLOR: But the blast area doesn't extend anywhere near as far as the fire area for the high bursts.

AYRES: This is true on clear days, but most days are not clear.

WARREN: Trees, brush and ground cover are important, too, on dry hot days. Eucalyptus, pines, drought resistant brush and grasses are full of oils and waxes which burn explosively. They're trying to replant the areas with different kinds of plants that do not have a high wax content. But this made a great contribution to the Bel Air fire.

TAYLOR: I'm just questioning whether it's really true that under general circumstances there won't be large conflagrations in wet weather; at least I've heard a lot of discussion about this from the people who seem to know.

WARREN: You've got attenuation of the infrared by the cloud cover.

AYRES: If it's wet weather you're not going to get enough thermal flux to start the fire in the first place beyond perhaps two or three miles.

TAYLOR: Okay. That's a big distance.

EISENBUD: Yes. That's six miles in diameter, which makes for quite an area.

AYRES: That's a big area but it's within the blast perimeter. Everything in that area is flattened.

UPTON: What's the length and breadth of Manhattan?

MILLER: About thirteen miles long.

AYRES: It's a mile-and-a-half wide.

UPTON: So a detonation over Columbus Circle could envelop Manhattan in the fireball.

DUNHAM: It would be academic.

MILLER: That's right.

EISENBUD: Then you have this hot fire even though it's in the blast area, which is then capable of spreading out with the winds. This gives the radius and if you have these on 30- or 40-mile centers, as you would in certain parts of the country, it's devastating. I don't see why it's necessary to make the least pessimistic assumptions in our planning. I think we should make the most pessimistic assumption, and this would be that it's about in the middle of a period such as just happened in the northeast where there was five years of drought.

AYRES: There's one objection. It may seem so hopeless that you do nothing at all. In fact, I think that's what's happening.

DUNHAM: And it's unrealistic to expect it to be maximal all over the country at a given time.

EISENBUD: Yes.

HEMLER: A maximum for all events.

TAYLOR: I think there's a question that's worth asking—aren't areas that are ignited going to be subjected to a general conflagration so that essentially everything combustible will burn? I think the answer to that is yes.

DUNHAM: Provided there's a wind.

TAYLOR: Yes, once it's ignited. The question I ask is, under whatever conditions exist, is it generally so that everything will burn out to the perimeter of the region in which ignition occurred?

AYRES: I don't think so.

TAYLOR: Why not?

AYRES: Well, consider the incendiary ignitions during World War II. The Tokyo fire storm wasn't a true fire storm but pretty nearly so. It covered about only half the area that was seeded. That's a pretty good illustration.

TAYLOR: Was it essentially heterogeneously but still completely seeded over the whole area?

AYRES: It was built up.

WARREN: Osaka went nearly completely.

AYRES: It depends on the weather.

EISENBUD: You probably had a million people fighting the Tokyo fire.

DE BOER: That's right. We're talking about an area in which you don't have any fire fighting.

UPTON: One at a time, please. Dr. de Boer!

DE BOER: I believe that with a nuclear attack, even a small one, under the best conditions and where there was only one square mile of total destruction around ground zero, there would be nobody willing to go into that city to fight fires. I would anticipate that within a very short period of time fires would spread over an area twenty times that caused by the bomb. The indirect effect could easily be more devastating than the direct effect.

AYRES: I go back to the Fire Service's own categories which show that about two-thirds of the time it won't spread even if nothing is done.

TAYLOR: But that's a forest which is all wet.

AYRES: It's not all wet. Some forests are extremely dry, not only during dry weather but in the winter.

DUNHAM: But the inside of your tree is wet and the inside of your house isn't.

CONARD: Weren't there some survivors in Japan from the fire area, who were in concrete buildings?

WARREN: Yes. The telephone building in Hiroshima was in the center and they had survivors. You see, it didn't start right away and they had time to move out or stay in or do whatever they wished.

DUNHAM: In Dresden and in Hamburg they suffocated.

EISENBUD: Yes.

TAYLOR: Was it true in Hiroshima that essentially the entire area that was ignited spottily burned until everything was gone?

WARREN: Pretty much.

TAYLOR: How about Nagasaki?

WARREN: On the way down, or on the way back from there by train, I went to Osaka and then to Yokahama and back into Tokyo. The difference in the remains was that in cities that were not hit by the atom bomb there were little islands in parks—a little bit of shrubbery here and there and parts of buildings. Certainly a lot more chimneys were standing up and it was quite different from the complete flat ashes that were in Hiroshima and Nagasaki. Sure the spectacular big factory chimneys and the concrete buildings.....

TAYLOR: Were they completely gutted, though, by fire so that everything inside would burn?

WARREN: Either that or the wainscotting stripped off. A good many were gutted because of short circuits.

EISENBUD: Staff, I don't think I've ever seen reports on the weather conditions in Nagasaki and Hiroshima in the days preceding the bombings. Do you recall whether it was generally wet or very dry?

WARREN: There was a lot of cloud cover and, you see, Hiroshima was only a secondary target. But the pilot happened to see it in a hole in the clouds. So he turned around and went for that because it was a target on his list. I think Nagasaki had quite a few clouds but not as badly covered.

DUNHAM: Merril asked what had the weather been the previous two weeks.

EISENBUD: Yes.

WARREN: Kobe was the prime target, Nagasaki the secondary target, and he actually went to Kobe and he turned around, yes.

LANGHAM: And came back over his secondary target and dropped it through a hole in the clouds. There was a heavy cloud cover at Nagasaki. There was less at Hiroshima.

EISENBUD: My question wasn't directed at the general situation with respect to moisture. What kind of rain?

WARREN: The humidity was up. This was just before the typhoon started and the rains came a great deal about the time we were there. We were worried about our instruments getting wet and shorting, the batteries running down and that sort of thing. We would frequently have to wait until the shower was through and then go out again.

DUNHAM: May I ask a question in my ignorance? This gets into the definition of a so-called fire storm. Isn't it almost by definition self-limiting because it sets up air currents towards itself?

AYRES: Yes.

DUNHAM: Once it's been defined, that is?

EISENBUD: Yes, but it probably would oscillate. In other words, after it burned out then the storm effect would stop.

DUNHAM: Yes.

EISENBUD: And then you would have a hot area which could be carried by the existing winds and, if there was enough combustibility, start another fire storm somewhere.

AYRES: They do tend to burn more completely than conflagrations. Once you have a fire storm, probably by the time it burns out there's nothing left in the area.

EISENBUD: No, but then you've got a large area of several square miles with hot ashes in it and it can start fires elsewhere in normal situations.

HEMLER: But not right by the fire storm.

EISENBUD: No, not right by the fire storm.

WARREN: Isn't there also apt to be an oxygen deficit right around the fire so that a lot of people can die from the oxygen deficit?

EISENBUD: Yes. This was a dramatic part of the German situation. For those of you who haven't seen it, there's a very interesting document which is a report from the Hamburg police chief. It has been translated into English and is generally available through Civil Defense circles. It describes that particular fire storm in which the most particular cause of death was suffocation; people who just sat in a pool of their own melted fat.

AYRES: That's a different cause of death but a lot of people apparently died of carbon monoxide poisoning.

TAYLOR: Yes.

EISENBUD: This was suffocation.

UPTON: Of course there was the Coconut Grove disaster, a very much smaller one, in which people were killed in the stampede, the panic and the suffocation.

TAYLOR: I think it's important, though; at least some people claim it was carbon monoxide poisoning and not an oxygen deficiency. The reason it's important is the idea that there's no oxygen around that can feed ventilation systems, and so on; it's not clear.

WARREN: Isn't it true that there isn't any oxygen around?

TAYLOR: I think some people argue that it's not true that there's not oxygen around; that there is oxygen, in fact, and that what caused the deaths in Hamburg was a combination of poisoning.

AYRES: If there's enough oxygen to feed the fire, there's enough to supply the people.

TAYLOR: Yes.

AYRES: But carbon monoxide is another matter.

EISENBUD: Carbon monoxide is another form of asphyxiation. That's academic.

TAYLOR: It may be possible to get good air.

EISENBUD: You would have to take out the carbon monoxide and put in a filter, chemicals, and so on.

TAYLOR: Yes. It's a little easier than if there's just no oxygen around.

EISENBUD: Yes. Well, Mr. Chairman, do you think we ought to spend the remaining minutes to see if there's a consensus on anything?

WARREN: You mean you would like a little prayer at this point? [Laughter]

EISENBUD. There aren't many things that there's general agreement on, but I think—no, there are several. I think it seems to be generally agreed that, for example, our country would not be wiped out completely; that there would be foci capable of assisting survival in the other parts of the country, so that is it fair to say that if this is so, we certainly are not going to drop down to 1400, but it would probably be, let's say, 1900. Where would you place the level of subsistence? The economy is going to drop down so the people are living on a scale of economy.....

AYRES: In 1900 what was the U. S. GNP (Gross National Product)? It was around $20 billion.

DUNHAM: By our dollars or their dollars?

AYRES: I think by comparable dollars.

DE BOER: That's usually adjusted.

DUNHAM: It is?

DE BOER: Yes.

EISENBUD: These are important figures. I hope you're right. $20 billion in 1900. Over how many people?

DE BOER: 100 million.

BUSTAD: Have I been suffering under a misapprehension? As a rule of thumb I assumed the predicted attack at the present time might

reduce the GNP and population to something like in the 1940's. Is that a reasonable approximation?

AYRES: The way I look at it, in the 1940's.

BUSTAD: Yes, in the 1940's.

AYRES: That's the way it appears to me. It's unlikely we would be back as far as 1929, I would say.

EISENBUD: I object most strenuously to this conclusion.

DE BOER: I do, too. I would say 1840, which is much better! [Laughter]

EISENBUD: I would go even further back than that. I don't think I would go to 1400 but I might go to about 1650 or 1700.

TAYLOR: You would stay in 1967. There are certain things that you know how to do now that you didn't know in 1940, but with some drastically reduced level of productivity. How you translate this back in time is meaningless.

CONARD: On a per capita basis?

TAYLOR: I think that's meaningless, too.

EISENBUD: Yes. Per capita in an industrial society is very different than per capita in an agrarian society.

TAYLOR: Exactly. We might become an agrarian society in which we might all be happier; I don't know.

EISENBUD: I think a hell of a lot depends on this point that Staff made. If it's true, for example, that plants get rusty as quickly as he indicated.....

AYRES: I thought there was some misconception. I didn't think he was answering the same question you asked. His point was that after a few days you would have to take the machinery apart and make sure the bearings are oiled, and so on.

EISENBUD: Yes.

AYKES: That doesn't mean the plant is useless after a few days.

UPTON: It seems to me, Merril, we ought to approach this in steps. Perhaps the first step would be to consider the surviving population. Then we would have to look at the age distribution of the population and the state of the industrial potential, and so on.

DOBSON: I thoroughly agree with that. It seems to me it is the only really meaningful way to look at it. In this connection, I would like to ask whether anybody feels that the idea is completely far-fetched that the population would be closer to zero?

DUNHAM: No.

TAYLOR: With that level of attack?

DOBSON: No.

BUSTAD: I thought we started out by putting on the record how many we predicted would be dead.

CONARD: Yes.

BUSTAD: And when I say that the population would approximate that in 1940, I obtained it by subtracting the numbers that you presented.

UPTON: You said 60 to 70 million dead and about 75 million injured. So that does add up to 140 million.

DOBSON: You mean we are still dealing with this particular size of attack?

EISENBUD: Yes. One of the things that has bothered me, which I mentioned yesterday, is the fact that these casualty figures, as I understand them, are predicted on a 450-rad LD-50, and it could be that under these circumstances the LD-50 is lower.

DUNHAM: Or higher.

EISENBUD: Or higher?

DUNHAM: Yes.

CONARD: With the fallout you get a prolonged rate.

EISENBUD: I was thinking of the fact that there would be all kinds of opportunities for injury and infection.

UPTON: If you take the conditions you postulated, 60 to 70 million dead and 75 million injured, call it 140 million.....

WARREN: Why not say 50 percent of the population is gone or is useless and 25 percent of the industrial capacity is left or some figure like that?

UPTON: Where do these 40 million survivors live? They're all out in the boondocks. What can they manufacture?

WARREN: A lot of small towns were unhit, too.

AYRES: Not just small towns.

UPTON: I think it's crucial to consider where these survivors are and how old they are.

EISENBUD: Yes.

AYRES: We don't have any of the numbers and I don't know that anybody feels like taking my word for the numbers since I don't remember them very precisely, except I have a strong feeling that you are all overestimating. If you have such an attack the number of dead is possibly 40 or 50 million and the number of injured is something like 20 million.

DUNHAM: Your definition of "injured," though, is important. Are these hospital cases or are they people with one broken finger?

WOLFE: The gamut.

DUNHAM: I think it's a gamut figure. If they re seriously injured, there's a bigger drain.

FREMONT-SMITH: Why do you assume that there are fewer injured than dead?

AYRES: Because the injured are the ones that are on the periphery.

FREMONT-SMITH: They are the big periphery on the center. There's a big periphery around 20 square miles.

AYRES: It's a long periphery but the center is more densely populated.

UPTON: How about the capacity of the dead as opposed to the capability of the living! [Laughter]

MILLET: How about the storage of basic materials? Are they all underground and can we get to them or have they all gone up in smoke?

FREMONT-SMITH: How about funeral directors! [Laughter]

AYRES: You're addressing the question of differential survival of people and of different types of skills.

UPTON: Yes. Have you really eliminated the most productive people in society?

AYRES: No.

UPTON: Are you left with, say, half the productivity you started with or a very much smaller fraction?

AYRES: As regards people or equipment, or what? You certainly have eliminated most of the psychiatrists, I'll agree

FREMONT-SMITH: That's bad! [Laughter]

AYRES: You've eliminated high fractions of certain administrative skills.

FREMONT-SMITH: Have you eliminated all the patients of the psychiatrists?

AYRES: Probably most of them.

WARREN: You've eliminated those out in the country with the swimming pools.

UPTON: How about the industrial productivity? It would seem that you have knocked out nerve centers, you have knocked out the

7 OF 7

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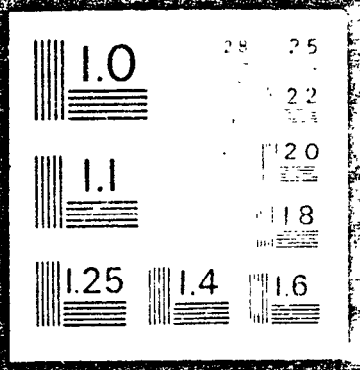

highly skilled technicians in our society and the technology really suffers.

AYRES: These questions have all been gone into at great length, you know. I can tell you what some of the results are, as best as I can recall them. I haven't really looked at the problem recently, but suppose we assume the enemy strategy is simply countervalue—or anti-population, which is essentially the same thing. You imagine that the enemy attacks the largest city first with his first bomb and then he attacks the largest remaining city with his next bomb and so on. Not all the bombs work, you understand, so there is a certain randomness. But each time he looks to see which remaining large cities are left and he goes for the largest one. If you kill the largest 200 cities in order, you will destroy somewhere in the neighborhood of 60 percent of the population (assuming no survivors in the attacked cities). Anybody who knows this should tell us.

TAYLOR: I remember one thing, and that is that twenty cities had 40 percent of the U.S. population and those twenty metropolitan areas would presumably be absolutely destroyed.

AYRES: Yes, if you've wiped out the whole metropolitan area. But don't forget the New York metropolitan area, which has 15 million people, also covers quite a large area and it takes more than one bomb to do it.

EISENBUD: The last time I looked at it, 20 percent of the New York City area population lived within 20 percent of the area of New York City.

AYRES: I'm still trying to address this point which was, let's say for purposes of argument, that 60 percent of the population is gone. Now, it may be that 80 percent of the miscellaneous manufacturing capacity but only 50 percent of the electric power production is gone wh 90 percent of the harbors would be gone, or maybe more like 95. It varies from one category to another.

Now, if on the other hand you want to attack petroleum refineries, you'll get most of the petroleum refineries with the first fifty bombs but you won't get very much population, and so on. So the point is that there is a much greater surviving capacity outside of the big cities than anybody really thinks until he looks at the numbers. And there are a lot of small cities between 50,000 and 150,000 in population, most of which have productive facilities of one sort or another.

UPTON: I think, Merrill, on the basis of this discussion it is imperative if we're really going to cover these questions meaningfully, that we look at some of the projections that have been made and come in the really thoughtful research data.

EISENBUD: Yes.

UPTON: So we don't just count bodies but we have some better knowledge of where the bodies are and what they are good for.

EISENBUD: I think on the basis of what's been said it would be very nice if we could go another day or two here just listening to the reports that these various people could organize.

DUNHAM: If we could get some documents.

EISENBUD: Yes.

DE BOER: If we could have this in documents, or better, have someone who has been involved in such studies. There are a number of people in the field of industrial development and economics who have done work in this area and who would have easily available information. I think this would be very helpful.

UPTON: Wouldn't it be very helpful to have both, to have documents in advance of discussion and then knowledgeable authors, those who have prepared the documents, to present their views?

DE BOER: Yes.

UPTON: Discuss them.

DE BOER: When we talk about the GNP, how many of us can give a definite figure of the GNP?

BRUES: I don't think that these rules, these quantitative rules, are entirely relevant. In any case, I've been much impressed by little examples that have been given of a tiny thing which causes some big thing to be irreplaceable, and the enormous complexity and interdependence of things at the present time as compared with fifty years ago or less.

It seems to me that has a very important bearing on the discussion. We may find, while cutting down the GNP by a factor of two or three, that we are setting ourselves back 100 years in terms of reconstructing the complex situation which we have now. I wonder what one thinks that is valid.

AYRES: I don't.

TAYLOR: I don't see why it should take longer than it did the first time.

AYRES: I don't mean that it will take us 100 years to do it the next time, but that we'll be back in the situation of 100 years ago.

DUNHAM: For forty-eight hours we would be back to the Stone Age in most of the country and then within two weeks maybe up to the Roman times. Perhaps in three months we may be up to the Middle Ages in terms of certain things. You've got to think of it as a dynamic situation.....

EISENBUD: Yes.

DUNHAM: .....which is changing all the time.

EISENBUD: But the thing we didn't discuss, you see, was this part of the curve. I frankly don't know how to talk about it. I mean, for example, you've got 40 million dead people that have to be picked up. Who's going to pick them up and move them into piles and dispose of them, and then let's say.....

WARREN: A lot will be burned up.

EISENBUD: That's right.

AYRES: In terms of the particular attacks we described, I've been objecting to other people's assessments. I believe that surviving capacity would be of the order of 50 percent but that surviving production in the first year would probably be down 15 percent.

DUNHAM: Down to or down?

AYRES: Down to about 15 percent of pre-war. But it could come back pretty fast, though, and that depends entirely upon how much

preparation there was, how much morale there is, how much people want to recover.

DUNHAM: It gets back to the psychosocial factors.

WARREN: What about the agricultural production?

AYRES: That wouldn't be too badly hit. Probably 50 to 60 percent of pre-war capacity could survive.

WARREN: Our economy is built on a very short string nowadays. Take, for instance, the seed that's planted. I looked into this considerably on a committee, a long time ago. In the 1920s every family had a kitchen garden and a complete set of gardening tools, shovels, rakes, hoes, even ploughs and things. Today you seldom find this anywhere, and it's no longer the horse and the mule that could eat off of the grass. Now you've got big agricultural farming. A tractor has replaced horse and mule and the tractor needs diesel fuel or gasoline and frequent repairs. Where is the seed to plant for next year? It's usually not on the farm; it comes from a big silo somewhere and it requires railroad transportation.

AYRES: All of these issues have been studied in great detail again.

WARREN: I think they all contribute to that recovery.

AYRES: SRI has done a detailed study some years ago of precisely this question. They assumed much bigger attacks than we're talking about and they still concluded that the surviving production would be 50 to 70 percent.

DUNHAM: You're speaking about how much preparation, how much stockpiling of seed, how much of this, that, and the other thing is available. This will make a tremendous difference initially.

TAYLOR: I would like to ask a very broad integrating question to try to focus on what we've been discussing in both sessions. The general result of these studies that have been made—they've been made for the government by Central Research Institute, a large number of organizations—seems to be that a postulated attack, presumably as the result of general nuclear war, without any further civil defense preparations beyond what we have now, will produce an effect on our society, and presumably on the opponent's society, which is not at all catastrophic.

AYRES: It is catastrophic.

TAYLOR: I'm sorry, but I mean not at all catastrophic. By catastrophic I mean that civilization as a viable force has been destroyed. And apparently the conclusion of these studies is that this is not true. Furthermore, according to these studies—and I'm not sure I believe the studies—the statement, which is made periodically by many of our government officials, that a general nuclear war means the end of civilization, has no basis whatsoever. Now, is that true? Is what I have said correct?

AYRES: That's true, except that it leaves the one major question. What about our society, the fabric of society itself, will that survive? Our structures, our equipment, our skills, our people, our land, our resources, all of these will survive enough so that if the will and the social structure survives, we can come back. I don't think we're talking about going back to 1850; I think we're talking about going back to 1940 or at worst 1920 in terms of the economy.

DOBSON: Dr. Taylor, in connection with these estimates, especially those on much larger attacks, when you talk about the end of civilization, the end of civilization in the world is one thing; the end of the United States, which is a limited thing, a more limited consideration, is another thing. Is this consideration impossible or ruled out by the authors of these studies? That is, if you're going to kill.....

TAYLOR: I don't believe that even they know because it's sort of an imponderable question. What will society do under these very different, very new situations? If it's really so that everybody will come out, wherever he is, and say, "Yes, I would rather be dead", and just lie down and die.....

HEMLER: And the resources?

DOBSON: My question was simply this: Is it a ridiculous idea that essentially all the American population can be killed by a very large nuclear exchange?

TAYLOR: No, I don't think it's a ridiculous idea but I think it's not a possibility with the forces that now exist in the world.

WARREN: Given another ten years, that would be different.

TAYLOR: It could very easily turn into a possibility.

EISENBUD: I read most of the reports that were published up until maybe three years ago and, frankly, I became sensitive to a certain point of view in these reports that tended to be optimistic. For example, you could select a 3,000-MT attack when it would be just as easy to select one at 5,000 MT. If you did select the 5,000-MT attack, your conclusions would be altogether different. And there was a tendency to accept the fact that there was moisture in the forests so that you wouldn't have the widespread.....

AYRES: At SRI they have studied attacks involving as much as 20,000 megatons, which is certainly pessimistic!

EISENBUD: I'm thinking of hearings. I'm thinking of the Harbor report. I've seen your stuff, which is good.

AYRES: I didn't make any particular assumption.

EISENBUD: No. The RAND reports.

WARREN: Could we get some of those hearings issued to us?

EISENBUD: You can. I started to say that this 3,000 MT might only be 20 KT if you accept the dreadful situation that Ted's worrying about where maybe some Cuban refugees get hold of a 20-KT bomb and explode it on us so that we will get into a fight with Russia, and they can get Castro out during the confusion. After all, they did try to bomb the U.N. And it could go up, I suppose, maybe to 50,000 or 100,000; you name it. It could be combined with BWCW.

AYRES: At that level you don't have to worry about the survival of anything.

TAYLOR: I think that's the level at about 20,000 MT. When you start adding up targets and placing carefully on them one thousand 20-MT bombs and you can kill everybody outright by even including heavy fallout, you can generate a situation in which I think it's at least a tenable hypothesis that in the next few months everyone would die.

AYRES: Yes.

TAYLOR: No, I think it's very important to add that there is a United States in the state that it's in now in 1967. If you ask what the situation could be with a 20,000-MT attack with various kinds of highly purposeful civil defense activity, then the answers are completely different.

DOBSON: Yes.

FREMONT-SMITH: This is exactly the contrast that needs to be focused upon. I would hope that, perhaps, next time, if we select some fairly sharply defined assumptions as to what we are dealing with and stay within the frame of reference of those assumptions, we will have plenty to deal with, and we will also come closer to defining the difference between what would happen today compared with what would happen after 3 to 5 years of intensive preparation, that might be very much to the point. I think that it's perhaps possible to get some of these reports circulated to the group, isn't it?

HEMLER: Yes, sir. There are certain ones that could be.

TAYLOR: It would be nice if there were a summary, just a thumbnail sketch, of all the studies of this kind that have been made and, if possible, overall conclusions.

AYRES: You don't want to go into that. There are hundreds and hundreds of such things.

TAYLOR: Are there?

HEMLER: Yes.

FREMONT-SMITH: It would take ten years to get a summary, but we could do that easily.

TAYLOR: There are a number of significantly different ones or something?

WARREN: The hearings are the ones.

HEMLER: I think this should be done.

EISENBUD: I think that perhaps what you would really like is taking the 1,400-MT case versus the 2,000-MT case versus something in between, and see what the conclusions were.

BRUES: Are there hundreds of reports because they deal with different aspects or with different assumptions as to the attack? Or because they disagree on their conclusions?

EISENBUD: I think the discussion could really only be meaningful if you projected it 20 or 25 years from now. I have a feeling that the situation that we're in is likely to exist for quite a while, but technology is advancing rather rapidly, and just as it would seem possible to discuss today's problems in terms of the weapons of 20 years ago, I think we should look to discuss the problem of war in terms of the weapons that will be here 20 years from now.

TAYLOR: That makes it easier because the total yield of the stockpile in the United States is going down. If you put it 20 years ahead, it may actually be simple.

DUNHAM: I think we should look to the comics and the science fiction people for our basic assumptions then.

EISENBUD: I've always felt that the emphasis on disarmament was in the wrong place. I think that we spent too much time talking about the cessation of weapons testing and nuclear weapons development arrest when we should have been looking at methods of weapons delivery and limiting aircraft size and missile delivery, because even with the present methods of delivery and the projected methods of delivery we do a pretty good job with Hiroshima type bombs.

TAYLOR: That's why it's going down, not because of arms control.

EISENBUD: Of course.

WOLFE: Merril, your plan for 20 years from now would make the decision twice as easy because then all of it would be speculation.

FREMONT-SMITH: That's right. And some of us would be dead! [Laughter]

WOLFE: Yes. I won't be at that meeting! [Laughter]

WARREN: I won't either.

BRUES: I assume that there's nothing left that we can do except to thank everybody concerned for their help, cooperation, wisdom, support and attention to detail.

FREMONT-SMITH: And morale.

BRUES: Thank you.

DUNHAM: And hospitality, at least from the standpoint of the guests.

FREMONT-SMITH: I'm very grateful to our two co-chairmen, to Colonel Hemler, to Telle for getting things started and to everybody. I want to express, on behalf of the New York Academy of Sciences, our enormous appreciation to each and every one of you for coming and participating and helping.

TAYLOR: I would like to second that vote of confidence because this organization is the only one that has ever sent me a ticket to come back to the States! [Laughter] As everyone knows I've been lucky to get reimbursement for a ticket within six months.

FREMONT-SMITH: We'll send you another one, come March! [Laughter] Thank you all very much.

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Specific topics included in this particular conference were the effects of the [illegible] hydrogen bomb tests in the Pacific Ocean which resulted in the [illegible] of Marshall Island natives and of the Japanese fishermen on the [illegible] (Lucky Dragon), the ecological effects of nuclear tests in the [illegible], and the effects of the aircraft accident in Spain, in which nuclear weapons [illegible] did not explode (Spanish Incident).

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