FRIDAY MORNING SESSION

October 6, 1967

- BRUES: Dr. Donaldson, you have us at your mercy!

DONALDSON: Mr. Chairman, Ladies and Gentlemen: This morning we should be able to have free run of our scientific acumen plus the widest breadth of our imagination, for if we are to talk about the environment and man's relationship to his environment in the field of ecology, 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, I'm sure we have an equal number of opinions of the effect of the weapons testing upon man and his environment. I toook our convener at his word specifically that we were not to write speeches; we weren't to deliver orations, but after 41 years as a schoolteacher I'm specifically tempted by almost heritage, for my mother and my grandfather were also schoolteachers, to deliver that morning lecture that should come 22 minutes from now on normal schedule.

FREMONT-SMITH: We have 30 years of interrupting practice! [Laughter] We expect to challenge your 47 years.

DONALDSON: Looking around, there are many schoolteachers I notice in this gathering. So I'm sure they will use the professor's prerogative to interrupt at any occasion.

Well, to more or less set the scene, I should like to, with your permission, somewhat limit the parts of the world we are going to talk about.

If you will just turn on the first slide, please.

[Slides] Well, each of us again have our own immediate interpretation of what we think of as environmental contamination. I think if we go back to the source area for many of our problems we would go to the Hanford work or to the Oak Ridge establishment and eventually to the Savannah

River area where materials are fabricated. We have learned to live with radiation in these areas and in the environment we have learned a great deal.

Then we could jump to the Japanese side, as we have in preceding sessions talked about the Nagasaki, Hiroshima, and on the mainland of Japan the experiences there or drop down to the Marshall Islands and concentrate on Rongelap and the fallout problems there, as we did in part yesterday; or to Bikini, to Eniwetok. But with a few jumps I would 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 at Johnson Island where we have some of your usual type of problems because of an accident that occurred that is not discussed usually but one that I think is germane to our operations here and to the Christmas Island area with yet another and even included the northernmost tip of the North American continent up at the Chariot site where Dr. Wolfe and his associates have gathered a good deal of both actual and projected information on this problem of environmental contamination. I did not include here the Amchitka area and the Aleutians where many of you are aware there have been some atomic detonations and they are preparing for one at the present time.

Well, this presents a very big order in itself. It includes about 50 per cent of the earth's surface and in a very unusual environment. I think it would be well if we could put some input on the British tests of 1952 and subsequent years particularly one off the Great Barrier Reef which is germane to our discussions here and a word or two possibly 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.

DONALDSON: Thank you. I think it's important we do consider them in the over-all problem of environment, particularly as far as the Pacific Ocean is concerned.

To be a bit more definitive as to locale and orders of magnitude, may we just by contrast superimpose the scale map of the United States over the area that we will concentrate on, I hope, and talk about the Pacific testing center with Johnson Island, Christmas Island and Bikini ane the Eniwetok areas in this instance.

UPTON: Is that a Mercator projection?

DONALDSON: Yes.

FREMONT-SMITH: Be sure we get that in the volume. I've never seen this before and I think it's very striking.

DONALDSON: I think this is one to one, but I'll be very happy to leave the slide with you if you wish it.

Specifically again if we may just review our natural history for a moment, atolls are most unusual structures. I like the statement that you'll find in "The Voyage of the Beagle" and other of Darwin's writings, that no biologist can be really considered a qualified biologist unless he has lived and worked in a coral atoll. They are very unique biological entities, and I'm sure those of you who have worked at Bikini and Eniwetok or the other atolls probably have cussed them or enjoyed them as your temperaments would dictate your own behavior pattern.

There are atolls that are dead atolls, such as Christmas Island where the growth is not quite equaling the sloughing of the atoll. There's 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 atolls before the Pacific tests were initiated. I recall that the geologists in the group were convinced, and in some of the lectures on the HAVEN that were held under Dr. Warren's supervision, they told us that the coral was about 180 feet thick. This was so because during the formation of the atolls the water had receded to about that level and so the coral can only grow in the upper layers of water. So there would just be a little cap. And there were many discussions as to the possibility of blowing this cap off the top of the mountain that the coral was superimposed upon.

These discussion went round and round, Dr. Warren, you recall, during the voyage of the HAVEN out to the test ground and we listened very intimately and in subsequent expeditions out there it was possible to drill in the atolls to see how thick the coral might be. In the 1947 expedition, particularly, the drilling was geared to go down as much as possibly 1000 feet into the base. But 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 it is. You'll be able to tell how old it is because geology is an exact science."

FREMONT-SMITH: You remember I mentioned the half-life of facts are getting shorter and shorter.

DONALDSON: Yes.

FREMONT-SMITH: I'm glad to have it illustrated. Go to it!

DONALDSON: The next day they may be down 200 feet, 350 feet, 400 feet, 800 feet, 900 feet and they were quite convinced they were in a hole and they had to change their estimation about the thickness of the coral, which meant some change about the age of the earth, which meant some change of their concept about 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 1200 feet and they still hadn't found out how old the earth was nor how thick the coral cap might be. By this time we were running out of food and we were running out of drinking liquor, which everybody worried about because the supply vessels were bringing mud to grease this hole that they were drilling down into the atoll.

The following year they moved over to Eniwetok and began to drill there and the element drilling went down to a total of some 4300 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 such great gaps in our thinking because we don't have the physical and biological parameters upon which to work. Like the statement of the Senior Senator from our State who repeatedly has made the statement that we know a great deal more about the back side of the moon than we do about the oceans that cover 72 per cent of the earth's surface. Well, with this as a 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 at, starting in 1946,and not 1947, as in the statement in your first volume.

FREMONT-SMITH: 1946 is right.

DONALDSON: 1946 is the correct one, not 1947. But we may take a quick look at Eniwetok on the next slide, please.

The atolls were selected, according to the Task Force reports, because they presented an ideal environment in which to work. Of course, they were isolated; they were in relatively favorable weather areas and they did provide a safe anchorage for the fleet and probably equally important there were a number of outposts upon which instrumentation might be based.

As far as we who were interested in the environmental sciences, they were quite ideal because they did provide a native flora and fauna that gave us a good cross-section of what we might expect. Now, you see these tiny little islets each with a peculiar environment quite its own, as the entire atoll type of environment is peculiar.

The land emerging area, about three square miles in each of the atolls, divided up into some 20 little islands in each atoll. The land plants, the fauna, is relatively limited. It's limited to those forms that can survive in a tropical environment that is subjected to wide temperature and salinity variations. The land and animals are limited to one group of mammals divided into three species of rats that were introduced apparently at the time that the native people came there. The birds are limited only to those aquatic birds that can fly long distances. Insects, there's one amphibian and one introduced, a reptile.

On the contrary,the marine fauna and flora is extremely diverse. There are about 700 species of fish in contrast to Puget Sound where I work in my normal activity. There are about 50 to 70 species of fish, probably tenfold as many species of fish. The same is true with the algae groups of great diversity and, of course, then the corals are something unique to this part. One might go through the other biological forms.

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: Well, these are forms that were native there. We have introduced some forms there. There are no species that have been exterminated in Puget Sound. All the native forms are there.

UPTON: I see.

DONALDSON: Well, added to the complexity of the environment and the great distances, we have a great diversity of energy releases and the types of releases. Just to review rather quickly, there have been 59 detonations at this test site. They vary in size from the normal device that we've talked about, some 20,000 tons of T.N.T. on up to, well, a statement was made it might have been 11, 12 megatons. That is the March 1st test of 1954.

These devices have varied from rather primitive ones by present standards to some very sophisticated ones by the measurements on up to 1958. They were detonated under a great variety of conditions and this is germane to the subject we're talking about; from under water to high in the skies, from tower tests to tests in barges sitting on the water. This means that fission products varied not only in quantity and some in composition but the induced radiation varied very fantastically in quantity and composition. So the numbers and amounts of radioactive nucleides introduced into the environment runs almost the entire gamut of possibility.

Now to try to evaluate in this weird and wonderful environment, to try to evaluate the impact of the detonations upon the biota presented a 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 rather quickly and then get on with the discussion aspects--the amount and kind of radiactivity released into the environment quite obviously is one of the important things, but I would call your attention to the primitive nature of the instruments and 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 1943 up to 1946 and even in subsequent years as we went along. I recall that we used to buy a scaler, an old Victory scaler from some of Dr. Warren's people and we would chop off a piece of fish tissue or some algae and push it in and if we went off scale we would say, "Well, there must be some radiation there. Throw it away and push in the next one." So it was essentially a presence or absence situation in the some of the instances. There was either some radiation or there wasn't. But I would have to qualify my statement as to the amounts and kinds of radioactivity which came somewhat later in the entire series.

We were particularly interested in the radar uptake particularly by biological systems and this again was dependent upon good instrumentation that wasn't available during the early years. We were interested in the amount and kinds of radiation within various systems; the selection and the concentration, and this becomes germane when we begin to talk about permissible levels because we have selective concentration. Some of the algae groups will take out one entity, for example, which will pick out iodine with tacticability to concentrate into the orders of magnitude of a millionfold for short periods of time. These blotting techniques then are very germane to over-all evaluation because this clerpa or this algae is eaten by some of the fishes and the fishes in turn then will pick up the iodine, and the most specific radiation damage that has been measured in direct measurement have been the destruction of the thyroid in some of the algae-eating fishes. We were interested in the metabolic transfer and the---

DUNHAM: May I interrupt and ask you what kind of stable content this does have normally?

DONALDSON: It has so much that we would not eat it because it has a bitter iodine taste. It's red.

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 perhaps a millionfold, you mean compared to the concentration of radio-iodine in the water?

DONALDSON: Yes.

UPTON: Rapid iodine turnover in this organism?

DONALDSON: I rather doubt it. I think it probably is always at a relatively high level and the limiting factor may be amounts of iodine available to it.

UPTON: Is it a rapidly growing plant?

DONALDSON: Yes. It grows rather rapidly.

UPTON: So that it's building a new cell and building in new iodine.

DONALDSON: Yes.

WOLFE: We have in Canada an algae in the Agean Cara in the river which have very large amounts. Yet it could be taken into the water except for sophisticated techniques and we analyzed the coral for manganese and found that 20 per cent of the ash was manganese.

DONALDSON: I think the specific concentrations are really germane to this sort of discussion because we base our interpretations on the familiar and forget to realize that in nature there are a wide variety of spectra of uptake.

We were also interested in the rate of transfer and elimination. In the discussion yesterday Dr. Warren mentioned the uptake on the side of the ships, but if you recall, these ships were always upwind from the detonation and so the question would be how did the radionucleides that would normally drift downwind work their way upwind and come up underneath the ships and be attached to the ships? So these are problems of interest.

We were interested in the disposal out into the open ocean, and one of the intriguing things was the more or less breathing of the atoll. Of course, the nature of the atoll allowed the constant thrusting 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 area is from the land to the sea, but in these atolls there is a very appreciable transfer from the sea back to the land or the limited terrestrial area, which comes from a variety of ways: by transfer from spray into the vegatation, and we find that this is a very positive transfer. This occurred in Japan to some extent, for those of you who followed the movement up on to the terrestrial area there. As a matter of fact, spray along the coast from the downwind drift was transferred up onto the land there.

In the atolls the more specific ocean-to-shore transfer is carried on by aquatic birds and this is very complicating, a very complicated thing in the evaluation, for the birds to transfer back on shore and upset the nice spectral establishment that one would establish when the first fallout comes. You have this group of nucleides;

you wish to follow them and then suddenly or gradually there is an overlay of others that have been selected out of the sea by selective concentration in an uphill migration. These further complicate but add a spice of interest to the evaluations that go on.

Then, I guess most specifically we are all interested in the amount and kind of radionucleides concentrated by various tissues. In making evaluations, we are always hard pressed to sort out the various parameters that are involved. We have the overlay of a blast effect and fire and radiation intermingled especially in the closed-in areas, and, if I may, I would like to use an illustration or two to point this out. May I have the next one, please.

Let's just take a quick look at what I think is one of my favorite photographs. This was made under rather unusual circumstances possibly, but since we do not rate sufficiently high on the Task group priority list who have the luxury of a photographic plane and we do occasionally travel--we did in the early days--by the older PBYs, and those of you who remember those old flying boats, you remember that they usually didn't have the usual facilities that are now found on modern planes but did have a place in the back that they called an air-flush toilet and by flipping up the lid of that you had a place to take a photograph! [Laughter] This may be a bit unusual.

May I have the next one, please. Let me use this as an illustration of the type of proposed thing one might use to document some of the things that 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 radionucleides in this great mass of moving water, a 3-dimensional plot. In order to do that we have to occupy various stations in some logical sequence. So

I drew these nice plots of the way we should do this sort of documentation. We will start over here at the point near A, A-1, and we will make these zigzags on this sort of a track, cutting back and we'll finish up some weeks later over here at a point E-10. Everything is very nicely in order now. May I have the next one, please. This is the way it eventuates! [Laughter] We started, we went along very well,everything was going pretty much on course except the problem of doing oceanographic work from a destroyer has its own problems. But when we first started out we asked the skipper, in case the WALTON, to let us go 50 miles this way and then we'll stop. "Stop? I don't know how to stop. I've never stopped this 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." Then he decided to stop. Then after he stopped he drifted some and then he quite lost his course and he couldn't quite go back on course again.

If you just turn it off for one minute, do you have room for one story?

FREMONT-SMITH: At least. One and a half! [Laughter]

DONALDSON: This problem of navigation really surprised me out there. It becomes almost--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, all nice and lonely, we had one little ship that had no way of producing water. So very helpfully the Navy would send us a ship every four weeks with a new supply of water. The water would get pretty stale and they would bring us some food and some mail. But on the back of this little supply ship was a little box and in this box lived six Marshallese boys. And I asked the skipper of this ship, "Why do you have these

Marshallese boys on this box in the back of the ship?" Actually it was a little cover on deck where they lived. And he said, "Those are my navigators!" [Laughter]

"Well, you have all the modern equipment." He said, "Oh, we have a compass and the sextant and the usual things but we don't have radar and any of the sophisticated equipment on this little ship." He said, "I couldn't just do 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, and their ears and their eyes and their built-in compass. 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 he first arrived there to this command, he was asked to take this ship from Kwajalein to the Atoll of Wotje. Wotje is east of Kwajalein 200 miles. Some of you who were out there in the war remember it was the place they used to have the milk run. They would go out and bomb it every day. So he set out from Wotje. His Executive Officer also was new, they plotted their course--just two officers aboard this little boat--and they plotted their course and when they arrived just where they thought they should be, there was the great big Pacific Ocean. So they looked around and, well, they talked to the sailors a while and the sailors were very reserved, of course, as sailors would be. This is the new Exec and the new skipper and they don't want to commit themselves. So they said, "Well, we'd better plot it again." So they plotted again and they came out with this point and they were in the big Pacific Ocean. Now, in all fairness to them, atolls are very difficult to spot. They stick up, atolls of about eight feet with palm trees mixed in the haze and the waves, and they're very difficult to spot. So they couldn't see it. They weren't close enough even to see it. So finally one of the sailors said, "I suggest you ask the Marshallese boys." Whereupon he says, "I'm a graduate of Annapolis. I know how to navigate a ship," and his back went up. But finally in desperation he said, "Well, do you fellows know where Wotje is?" And again this is typical of their behavoir, never a direct response. "We'll think about it for a while"; and 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 and they had another consultation and they said "Wotje that way [indicating]." This was a real big help. At least he knew the direction to go! [Laughter] He thought maybe this fellow is so damned smart, maybe I could ask him another question. So he said, "How far is it to Wotje?" Well, 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." So he said, "Sail that way 40 miles." They went into the harbor and dropped the anchor and everything was lovely and he began to think about this. So he gathered them together again and he said, "How did you know where Wotje was?"

"Oh!" This was a very serious problem. So 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--and I've forgotten which one; it may have been Kwajalein; I don't think so. And the navigator was navigating and they had a native on the bridge and the native said to the skipper, "I think you've gone past the entrance," and the skipper turned to the navigator and the navigator said, "No." And so then they tried to get in and found they were up against the sand. And they went on and they came back again and then the native told them just where the entrance was and they went in there and he was right. So the man I was with, the captain, said that he spoke to the native and said, "How did you know?" And he said, "I could tell by the sound of the waves." And you probably know this very well, the winds provide waves that hit the atoll which then have a backwash that flows a way out from the atoll and these make a perfectly steady lap, lap, lap on the side of the ship. And when you come to the break where the entrance is, there is a shift in the sound because the waves differ. And 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: Then in addition to the problem of living and organizing, may we just take a look at another illustration or two and then we can get on to the particular problem.

In the detonation, of course, we have produced some blast, some fire and some radiation. Now, the next one, please.

EISENBUD: Which detonation was this?

DONALDSON: We weren't supposed to say that, but it's Okinawa.

This is going to go this way or that way. It will carry you off in some manner that the meteorologists always are very exact in determining the direct way it is going to go. Sometimes they are right. And then it will leave a trail across the lagoon or into the sea that one may be able to find or not find.

Now if we may see the next one, please. This on the islets will produce various and sundry effects. It will knock some palm trees over, break them off and you can say, "Yes, the blast pressure was such," and here you can make a very direct measurement of the amount of blast it takes to knock a palm tree over. Well, it's a very appreciable amount of energy. Palm trees are made to resist winds of almost hurricane force.

The next one, please. Then it will take a certain amount of energy, thermal energy to burn the leaves and one can make some rather exact complications here of the amount of thermal energy that was produced at X number of miles. And here you see the leaves are burned and you can make this measurement very directly.

The next one, please. Then there are the other effects on the animal populations. The aquatic birds are in fair number in the atolls although the speciation is limited, as I mentioned, and one can make some measurements here. We see these birds are flying around very nicely and they seem to be all right. So nothing happened to them.

The next one, please. This little fellow forgot to take off when the rest of them flew. So we'll take a look at him a little closer.

Now, if we can go back to the next one. So I'm going to try to sneak up on him and see if we can catch him since he doesn't fly very well and it's obvious that he has some particular problem. So we'll take a closer look at this one.

Next, please. Well, he was pointed the right way, I guess, as he should have been, for he was looking away from the blast and he had gotten his tail feathers singed and some of the primary wing fins or feathers have been burned. He's in about the same shape, the same problem as a ship without a rudder. So we'll put him down and go back and go to the club for a while and come back the next day.

So we come back the next day and here are numbers of dead birds on the beach. Well now, the logical assumption, I guess that one makes is that, well, these birds must have died from radiation damage. They were all right yesterday, at least they were alive. So we assume that they died from radiation damage. We have a look. We examined them very closely. We tried to measure this, measure that, do the best autopsy we could and find little or no radiation or they are too far away for any neutron flux. So why did they die? This is the question you have to answer.

In our report we would just write a simple thing. We would just say they died of radiation. Then we have to draw upon a little bit more background. We have to draw upon the natural history of these beasts. 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 don't drink it. They get their moisture from their food, their food being the fish in the sea.

Well, the salinity of the fish in the sea is the same as yours and mine and it's about 75/100 of 1 per cent, and by getting the moisture from the sea they're able to maintain their moisture balance if they feed. But if they don't feed, they can't maintain their moisture balance. So they die of desiccation. So these birds died of desiccation? No, they died from thermal burns because they burned their "rudder" off and they weren't able to fly. So you can by elimination sometimes arrive at a reasonable solution of things that are happening.

Now, if I may have just another minute or two.

CONARD: Did those birds die in one day?

DONALDSON: Yes. These pictures were made on subsequent days.

CONARD: It seems like it's a pretty quick death.

DONALDSON: But it's terribly hot.

FREMONT-SMITH: They dried out fast.

DUNHAM: Did you decide these fish died from desiccation or from thermal burns? I wasn't clear what your conclusion was.

DONALDSON: Desiccation, because the burns weren't there.

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.

DONALDSON: Yes.

ROOT: This was obvious in the autopsy, too.

DONALDSON: Yes.

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 the pathologists and the epidemiologists have. What is the cause of death?

DONALDSON: That's right.

FREMONT-SMITH: Multiple causality enters into it.

DONALDSON: Surely. Multiple causes that

complicates 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: I don't think we have. I'm trying to recall.

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 birds that survived two or three days almost invariably were in good shape. They set up housekeeping somewhere else, except for those that can't fly, the young birds.

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 them off, they don't dive.

TAYLOR: I see.

DONALDSON: The major other problem I guess one might call attention to at this point is that we are dealing in really two environments: The birds living in both, but the other animals essentially living either terrestrially or in the aquatic environment. And the quite obvious situation that existed immediately is that there is the stratification of the fallout into a finite layer essentially on the terrestrial area where there is a three-dimensional distribution in the sea. This immediately changes all approaches of one or the other. In the terrestrial area the fallout is available to the biota most specifically if it's in a soluble form. In the soluble form it's picked up by the plants and enters into the food chain of the animals that feed upon the plants.

[Blackboard] I put down just a partial illustration of the sort of fractionation that takes place in the land area. This is part of a very complicated long table, but just as an illustration let's look at it. All the spectra of radionucleides, of course, are available on the land as they fall out there. This is from Eniwetok. The particular island, Cabell Island spectra. The soil has, in 1961, this configuration of presence or absence of radionucleides. The plants substantially pick up only four out of this complex and of the four that the plants have, the rads essentially concentrate too: strontium and cesium or cerium. The fish, on the other hand, have essentially available, one would assume, the same complex of radionucleides since they also fell upon the water but the fish in the main pick up manganese, cobalt-60 and zince-65.

Now, we might add to this, if we take the dominants-- in this particular one we don't have iron-55 but in the open sea iron-55, along with cobalt, at the present time are the two most dominant radionucleides.

Well, in a sweeping statement of generality, which is always ridiculous, but in the main the terrestrials are the soluble nucleides in soluble form, those in the sea of particulate form are concentrated most. Since the induced radionucleides of the cobalt series and iron series are in particulate form, although the finely divided form, they enter through the food web more dominately than do the soluble forms that are more distributed through the water.

Then we might comment on the competition that exists in the sea which is quite completely different than the competition that exists on land, for on land there are nutritional mineral deficiencies that for the most part do not occur in the sea. Now, this seems to refute the comment I made earlier about the iodine, but again generalizing, the cesium uptake on land is directly related to the deficiency of the fat. The strontium-90 on land is an uptake we say because you have the calcium deficiency, and I just mentioned a few moments ago that there billions of tons of calcium in an atoll that's some 4300 feet thick column that grows on the island. But this is in insoluble form and only when it's made soluble does this become in evidence.

So, in the sea the potassium ratio is about 360 parts per million and on land the calcium is about 440 parts per million. So it's not quite obvious that a straight atom or two of cesium or a straight atom or two of strontium in the sea; we can't get excited at all about it. So when we have this great nuclear war, I'm going to run out and catch myself a fish and eat it entire and feel quite secure that my food supply isn't in jeopardy.

Well, if we may have the next slide, please.

AYRES: May I just interrupt. That's cesium there, isn't it, Cs?

DONALDSON: Yes.

In terrestrial areas it's quite obvious that in some instances there's little chance for re-vegetation or regrowth. The soil is burned away; the seeds have been destroyed; the entire fauna and flora one would assume in this place would not be re-established.

The next one, please. Now, in areas where the soil has not been burned and has not been removed, you see in this illustration the soil core where the organic material on the upper inch is and on the right the radioautograph with the distribution of the remaining radionucleides in the upper inch.

Next one, please. This means that plants that have shallow feeding roots that feed close to the surface have a better chance to pick up these soluble forms, to incorporate them into their tissues, and those plants that root deeper, like the coconut, for example, do not have radionucleides available to them in the soluble form. They are feeding deeper in the substratum. So you have a different accumulation depending the zone of feeding of the plants, as do the zone of feeding of the animals.

The next one, please. The plotting of the distribution in the sea is one that is a constant shifting pattern that changes with the seasons, that changes with time, of course, the direction flow of the currents, the distribution carried on, and it changes from hour to hour and at least it changes from day to night.

The radionucleides in the sea are incorporated in the lower strata first since they absorbed on the small biota and then absorbed up the food chain. Many of these organisms are in the deeper layers in the hours of darkness and migrate to the surface during the hours--the deeper layers during the hours of day and migrate to the surface during the hours of dark. So there is a vertical diurnal migration as well as this constant shift, depending upon the direction of the prevailing currents.

AYRES: Is that deep water?

DONALDSON: What?

AYRES: Is that deep water?

DONALDSON: It's surface water.

AYRES: Diurnal doesn't normally extend into shallow water, does it?

DONALDSON: Well, speaking of deep water. I'm speaking of deep water as water--shallow water as being the water in the mixing layer which in this area is about 600 feet.

DUNHAM: You don't mean shore water?

DONALDSON: Not shore water, no. I mean open ocean water.

WYCKOFF: I'm sorry. What do the numbers represent? But then the lines? Okay, they are contour lines.

DONALDSON: They are contour lines, distribution lines. These are the planktons that do migrate up and down. This is plankton that was collected through the entire mixing layer. So this shows that there is this distribution out on the sites with a concentration closer to the island as it's coming from a point source driving out into the sea.

UPTON: How long after detonation were these measurements made?

DONALDSON: I will have to go back and look at the original. But this is just some weeks at most. That's one of the tests, but out of the family of curves I just picked an illustration.

UPTON: I see.

BUSTAD: But lower than that in spite of these high levels, incidently, these three in the fish, the only damage that was observed in the fish from the radionucleides was in the thyroid, wasn't it?

DONALDSON: Yes.

BUSTAD: Even though it was, as you pointed out, manifesting a high concentration of water, it certainly was in the herbivorous fish.

DONALDSON: Yes.

BUSTAD: Now, do you have any later results than that of Boardman? 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 has been set.

BUSTAD: Or couldn't they compete? That's it?

DONALDSON: We have looked diligently over the years but we haven't actually seen nor found fish that we could say was specifically killed by thyroid damage or other radionucleid damage. Now, there's always the complexing situation here as far as the fish are concerned. And the complicating one is that immediately, no matter what the radiation levels are, no matter what the peripheral problems are, the cleanup squad move in almost immediately and clean things up. This means that a fish that is just a wee bit incapacitated is removed within minutes, at least within an hour or so. Sharks move in and they scavange the place with a great regularity. If it isn't the sharks, some of the other predaceous forms. So 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 phenomena doesn't extend right up to the top?

WARREN: There isn't very much ingested with any one fish, is what he's saying, of the radioiodine because the thyroid is so small in terms of the body mass of the shark.

AYRES: So peak concentration is featured in the lower forms then?

BUSTAD: That's right, and they may destroy the thyroid or severely damage it and they shall be no radioiodine left by then. Time competes with it, too.

DONALDSON: There's a big difference. The physiology of the shark is quite different than that of the bony fishes.

AYRES: Of course.

BUSTAD: But we have to admit, I think, that many of those fish that Boardman picked up down there relatively early manifested severe thyroid damage were probably not a compromise from the standpoint of your cleanup squad. I mean he got there before the cleanup squad.

DONALDSON: Yes.

AYRES: Are there any turtles in the area?

DONALDSON: Turtles are very secretive beasts. They just don't like people about. There are turtles there, true, and when the 4000 or the 5000 members of the test group descend on the place, the turtles go somewhere else.

AYRES: I see.

DONALDSON: The turtles are back at Bikini now, and I hope we can see if the Chairman will allow us to take a look at what the place looks like now.

WARREN: One thing I think we've left a little dangling in the discussion. You said the plankton ~~with the~~ diurnal ~~variation there in~~ their location, does occur in the atolls where the depths may be 200 or 250 feet or there-

~~abouts~~ about as well as in the open ocean. The shallow waters you ~~were talking~~ mentioned were meant to be the shallows, weren't they, at depths of 15, 20, 30 feet?

DONALDSON: Yes. It comes up on the shore at night. It's carried in the surface layers and as the waves bring it up on to 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 ~~of~~ by the deeper currents, ~~cooler water~~ and with considerable ~~enough boiling~~ upwelling of cooler water on the ~~upturn~~ upwind side. ~~So~~ This is the deep circulation that you mentioned.

DONALDSON: In part.

WARREN: In part it ~~leaps~~ leaks out into the ocean on the other side, too.

DUNHAM: How deep is an atoll?

DONALDSON: Most of them are 180, 200 feet. In a living atoll this seems to be about the growth rate. They grow into the wind, grow into the east, since the prevailing winds are from the east. They decay on the downwind side and the inner reef or ground more slowly. So they tend to expand out to the deeper portions of the atoll decay or the corals decay and make the bowl shape so characteristic because of lack of food, lack of light.

CONARD: But you get a lot of coral heads, don't you?

DONALDSON: Yes, we have localized ones. But the coral heads are so spaced that they get food produce coming in.

UPTON: Lauren, our coffee is here. Would you like to break now or some time soon?

DONALDSON: It seems a logical place to break.

UPTON: Whenever you're ready.

ROOT: I wanted to ask you was the species that you mentioned of the algae, was that like the Japanese seaweed which concentrates iodine entirely? Do they have that there?

DONALDSON: I'm sure it exists in Japan but I don't think they eat it. I've never seen it in the markets in Japan.

ROOT: That heavy purple seaweed that has a strong iodine taste, does that exist down in the coral, in these atolls?

DONALDSON: Yes.

ROOT: It does? Because that is a naturally high concentrated one.

DONALDSON: Yes. Why I'm hesitant, there are so many algae.

ROOT: I was wondering whether the one you mentioned was the one?

DONALDSON: If I recall correctly, there are some 63 species at Bikini alone. Many of them are various shades of purple and red.

ROOT: These would be the naturally high iodine concentrated.

DONALDSON: Yes, within this whole group there are those that captivate much more specifically than others and I think that it's this lack of uniformity that we have to guard against, not saying all algae do this and all fish do this and all corals do this; that all plants do such and such. And this is why I hesitate to do like this I put on the blackboard. Land plants with a term like this, because it's self-defeating to do this sort of thing because you lose all the understanding that can be gained by looking at the variety of parameters that are available to you.

AYRES: You have indicated that maganese 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, and that ~~is~~ you said, when you finished up at Bikini, that it was very fortunate that you had made prior studies because the ~~support~~ rate ~~of~~ the genetic ~~cases~~ going on in this population was much higher than had been suspected and it might have been ~~allied to~~ the radiation later if it had not been found ~~prior to that~~. 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. It's like saying, "What's the yardstick of comparison with the Japanese situation?" The change in the biota may not or may be great. I think we have to go back to the flora where we have fairly definite anchored things that we could look at. I would like to refer this question to Dr. Wolfe here. After all, he was the botanist-ecologist here.

WARREN: Well, I thought snails were particularly demonstrating this change.

DONALDSON: I don't know.

UPTON: I suggest we break now and come back to this question after coffee.

WARREN: All right.

[After coffee break]

BRUES: Lauren, you were talking about the concentration of some of these elements in particular, plants and, of course, you can tell this with these traces that are essentially cleaned out of the ocean by living things? We see this in fresh water situations. If you throw a little P32 into a pond, it all disappears into living matter. In fact, that's probably a major limiting factor, I suppose, in how much will grow. Does this happen in the ocean or is there plenty of âll the elements to go around?

DONALDSON: I'm sure that there are plenty of elements in the ocean, but are they available? And if you suddenly make many essential, biologically essential, available, of course, they are blotted up. Maybe we can use the same illustration with that of my photograph that we presented a while ago in this case were the giant column end was standing up against this one detonation of the north region at Eniwetok. The fallout of this came right across the northwestern edge of the atoll. We dubbed this shot "the manure spreader" shot and this was rather popularly used in the test group, for in making the reconnaissance sweeps over the atoll there was a band inside the atoll of brilliant green, just a brilliant green. But immediately you had--you can fly from the relatively blue waters of the lagoon over this green band that persisted for several days and immediately the radiation instruments would jump several orders of magnitude. Well, it was quite obvious the thing that happened. That is, the detonation had burned a good deal of the calcium carbonate, just to take one element. It converted the calcium carbonate into an oxide. The oxide had dropped in the waters as hydroxide. Being soluble, it was picked up in the explosion of plant growth. But there are other elements involved in this, too. In other words, a nutritive media dropped in the sea had stimulated a very great growth, but in this were the direct materials, if you want a good sample, biased tremendously, this was the place to go get them. One could get a concentration of radiation tied up in this form.

Well, 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. You remember, Dr. Warren, when we used to sit there and hang on to our hats to keep them from going away from the pressure. But these steaming vats had tremendous algae growths around the edge, and they still do. They in turn absorbed--and there was very serious consideration given at that time of "we'll simply collect these plant growths and put them some place because they blot up the radiation very nicely."

EISENBUD: Lauren, iron-55 is an interesting nucleide. 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's the No. 1.

EISENBUD: I would think so, yes.

DONALDSON: Yes.

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 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: Yes. Did you see cobalt?

EISENBUD: Cobalt-60? We haven't seen it. In fact, it isn't there. If it was there we would have seen it.

TAYLOR: Is there any persistent biological sign still at Bikini or Eniwetok of the testing 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 not answer, of course, but just postpone it until we have a look?

TAYLOR: Yes.

DONALDSON: Because I think it will be more obvious when we look at the film, with the co-chairman's and our host's permission, which I should 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: That 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 specifically of aquatic life because the surface you said in places where the surface has really been completely sterilized, there has been a change, I gather, in the surface of the islands.

DONALDSON: Sure. 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 plains 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 heredity of that part of the atoll, and it hasn't been settled.

WOLFE: We had some guy who worked up a monograph on those coconut trees.

CONARD: Fosberg?

WOLFE: No, not Fosberg. I don't know his name. And this double crowning--he even got a coconut tree in one place with 51 of these crowns and there hadn't been detonation around that. 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 that you can say that it was caused by radiation.

CONARD: Yes.

WARREN: Weren't there some broad stems, flat stems, in Eniwetok?

DONALDSON: Yes.

WARREN: That you were wondering about the neutron effects?

DONALDSON: Well, we have recorded over the years a number of varients on the normal, particularly among the plants. Whether this is induced somatic variation or whether it's inherent we don't see them now. We've gone back to the same place. They have either died, were unable to survive. Of course, we do see variations, but we at one time---

Dr. St. John counted as many as 23 varients on one island from the normal. 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, and I've seen it mostly in the composites and it has nothing to do with radiation.

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 stem, 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.

WOLFE: Yes.

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 the radiation experience?

DONALDSON: I don't know how to answer it.

CONARD: I was talking to a botanist and he thought it would be worth while to study some of the pollen from the coconut trees on some of the island atolls and he thought, I believe, by chromosomal aberrations and this sort of thing that he could detect persisting radiation damage, and 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: You know, the Indians have done something along this line in the palms associated with Carilla and they do report a higher frequency of chromosomal abnormalties in the palm trees that grow in the strip than those that grow farther away. But it seems to me that when everyone begins to talk about the genetic problem, you can approach this as an either-or situation. There are, so far as we now know,no unique yardsticks of radiation damage and therefore you ultimately are cast in the role of trying to show a dose dependence and if you can't get variability in the doses that you can recognize, then you have no means to get at the problem.

There's an observation here that I think is relèvant to what Dr. Taylor, the question that he asked. In 1950 or 1951--I think it was probably 1950--Yimashita Cosko, who is a Japanese cytogenetist at Kyoto University did a fairly extensive study in Hiroshima on the distribution of abnormal forms of cosmos which is a little garden plant and they could show a definite correlation between the frequency of aberrant forms of this plant and distance from ground zero. So that it diminished as one went outward although the very things---

TAYLOR: Just looking at people's gardens?

SCHULL: Essentially that. In Japan it grows along the roadside in many areas or did then. The very count that he was making, though, was a situation that you find these aberrant forms all over Japan but it was the frequency and its relationship to growth that is the real key, and I think that this would be typical in Bikini or Eniwetok because you probably don't have enough known about the gradient in dose so that you could make any kind of strong statement to show that the frequency is varying as the dose is varying.

CONARD: You would have quite a gradient on Rongelap, 2300 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 mrp per hour downwards to normal levels, and this is about 12 r per year. So that in 100 years you have 1200 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 of 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.

FREMONT-SMITH: Dr. Taylor, you just wanted to say something.

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, and I'm not sure from the conversation whether they are really well known or not. Do people agree that the dose levels at Rongelap have been normal within a factor of, say, one and a half, the total dose?

CONARD: I would think so, judging from the dose calculations and the hematological responses of the people, that we're not too far off.

AYRES: With a position of 50 per cent you ought to be all right.

TAYLOR: That's the trick.

ROOT: I would think that there would be a considerable difference in the Rongelap material, too, than the Brazil because that would be cumulative and you would have no control from ground zero before they were exposed, whereas here you would have the sudden exposure to whatever it was, 2300 rad and would have your before-and-after picture. So I would think this would be terribly important material to have.

EISENBUD: One problem that's cropped up in Brazil which hasn't been solved that might be pertinent here is the fact that it's hard to tell where these chromosomals 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 gene type 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. And 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 transversed 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 ten-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 part 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 and these chemical changes presumably are important. This is another factor that has to be considered.

WARREN: Yes, it is.

EISENBUD: Yes.

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 gets about, a couple of kilometers across, about 300 meters high above three---

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 and 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, and this is a few kilometers across and this is where the work is going on.

WOLFE: I haven't seen it.

WARREN: I've only read it. You don't run sheep on this because there's no grass or enough foliage?

EISENBERG: No, that's not so. In fact, the cows graze on it and 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 over-all environment and Bob has some data on the whole-body burden of the Rongelap people that we might bring in now, with your permission.

CONARD: Well, yesterday I mentioned that after about six months, a year or two, the body burdens of the Rongelap people dropped down to barely detectable levels and by the time they were moving back to Rongelap he couldn't tell the difference between the comparison of unexposed people and the exposed people, the level of body burden. 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, which had some strontium-90 and cesium-137 and, strangely enough, from eating fish, the zinc-65 in the fish, as Lauren pointed out, got in the people since fish is one of their mainstays in their diet and we then were able to get a whole-body counter out to Rongelap. The first one was a big monster that weighed about 21 tons and that was a real endeavor to get that thing out on Rongelap Island, but we did. We finally gave that to the Navy and had to get another one. So we wound up by using a shatter shield type, more portable type of whole-body counter consisting of lead brick.

The first slide will give you an idea of what that looks like.

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 three-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. Both populations exist there. The residual fish that live in the lagoon are there but there's also tuna fish that are used.

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 maybe lagoon fish, wasn't it?

DONALDSON: The ocean fish are essentially carnivores and the lagoon fish are herbivores and you immediately fractionate on this basis alone, that is, the food chain is different.

FREMONT-SMITH: Yes.

DONALDSON: And as you go up the thing looks again as if you dilute it.

FREMONT-SMITH: So it was the herbivore that was responsible obviously.

DONALDSON: The herbivore are obviously the best concentraters.

CONARD: The next slide I think shows a spectograph of what you get from the whole-body count, showing the comparsion of 1957 and 1959. In March, 1957, shortly after they had come back showing an increase, the first peak being cesium-137 and the second peak is 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.

The next slide, please. This is a histogram that shows the changes over the years. The first 1954 data there shows the higher levels, of course, connected with the initial contamination and then up until 1957 their body burdens reduce practically to zero and then you see on their return to Rongelap the increase in cesium and zinc and strontium-90, of course,also began to appear, and this had to be detected not by whole-body counting but by urinalyses, radiochemical analyses of the urine.

The levels reached a peak about 1961 or so and beyond that time they have seemed to be at equilibrium with the environmental levels of the isotope. Cesium, for instance, peaked at about a little less than one microcurie of body burden, which is not high, but it represents about 300 times the level of those of us in the medical team that were counted. Since that time it seems to have remained fairly constant. In other words, they are taking in just about as much as they are putting out.

In regard to the 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 the previous year's value, and 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 rather drastically and greatly.

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: 2.6 years isn't it? Something of that order of magnitude.

CONARD: But that wouldn't account for a sudden change?

DONALDSON: If I remember the data correctly--and I would have to look it up. I have it here, but essentially there has been no drastic change in any incidence in the usually expected declines that have gone on. Maybe if they have changed their habits not only in eating fish but in eating birds; 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: For chromosomal abberations. At ten years we had quite a few cultures, about 40 cultures.

FREMONT-SMITH: Did they show anything out of the usual?

CONARD: They showed persisting aberration, low levels of aberration.

FREMONT-SMITH: More than other people would have?

CONARD: More than the control. They were compared with the controlled unexposed population.

EISENBUD: I think it should be emphasized that those doses that you are showing on the board, 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: Then another isotope that was found was cobalt-60 to some extent, which is about 1/10 the single level. We haven't seen any iron-55 in the people but we haven't done---

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.

DONALDSON: Yes.

EISENBUD: It should be very interesting in that group to see what the iron-55 level is. Iron-55 is an interesting isotope. It's been neglected up till now because the emission is a 6 Kev. electron which has a rank of ony one micron in tissue and it's been generally ignored. But iron goes to very small volumes of tissue. Specifically it tends to concentrate in these 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 yet to understand, chromosomal breakages. They show about as many breakages of chromosome as to 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 that taken up as zinc or is it surrogate to something else?

CONARD: I really don't know. I know it gets into the body and is fairly well distributed, as I recall it.

LANGHAM: It's concentrates in the epithelial tissues. The hair is very high, the skin is high.

CONARD: The prostate I believe is very fairly high.

LANGHAM: The prostate and pancreas. There is zinc exudated. The skin and the hair, if you calculate the total amount in the body, the majority of it would be percentagewise in the skin.

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 probably we'll have levels that can be detectable.

WARREN: It's interesting that the tropical rains don't leach it downwards. ~~It's interesting that the tropical rains which they have would~~ the tropical rains should produce quite a bit of water to leach this down into the soil. Is it complex and fixed?

DONALDSON: It doesn't leach to any degree. It stays pretty well fixed.

WOLFE: It's accumulated in the algae in that upper layer, isn't it? That is as that radioautograph shows.

DONALDSON: Yes.

WOLFE: And the algae are only in about the upper end. Below that it's apparently too dark.

CASARETT: Maybe we can ask the same question about man as we just did about other organisms. What mineral studies could be done to show that they had this radiation exposure, and so far it seems that the cesium-137 would reveal the exposure. Cytogenetic studies do. The thyroid studies do in two ways by nodules or ablation and the beta burns, the scar are depigmentation, or the nevi. Does this give some clues as to what may be looked for in animals or plants? For example, where are these radioactive isotopes concentrated in the tissue of birds or plants? The cytogenetics has already been mentioned.

Does this give some clues from man who can be studied in greater detail as to where you might look in other organisms?

CONARD: This is going backwards, isn't? We're usually trying to extrapolate from animals to man and now we're going backwards.

CASARETT: You can do it both ways.

CONARD: I suppose there would be some correlation here. It would depend on the animal. We hadn't thought about it.

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 the laboratory animal 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. The chronic exposure over long periods of time and pick a level, a half-hour per day for 100 days of total exposure at 50 roentgens and follow them through several generations, and instead of finding a damaging effect you find a stimulating effect. Double the dose, and the same sort of thing 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. But we'll have several hundred fish coming back from the sea that have had this experience.

FREMONT-SMITH: These are salmon?

DONALDSON: Yes.

FREMONT-SMITH: I thought there might be somebody that would know it.

DONALDSON: They're the only ones that actually come home to us from the sea.

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 per cent of them.

DONALDSON: Yes. The survival is better---

DUNHAM: It's the ones that come back that are bigger and better.

FREMONT-SMITH: Do you lose 90 or 99 per cent? Is that right?

DONALDSON: Actually the normal expected mortality in the sea of salmon is in excess of 90 per cent and these go through about that same experience plus or minus half a per cent. We have controls going along, but the survival are the irradiated up to--we have the information back at 1.5 r per day for 100 days during the embryonic period as their survival, coming back from the sea, is greater than a like control group. We use siblings in either case.

FREMONT-SMITH: So that is really as if you had benefited the fish by radiation.

DONALDSON: Yes.

UPTON: How about the hatchery?

DONALDSON: Better. Fairly significant.

EISENBUD: Do the salmon say the university is always here? [Laughter]

DONALDSON: They don't make mistakes! I wish I had students as smart as those fish.

WARREN: I think this is a point that Lauren has found that 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.

WARREN: It's against the dogma.

FREMONT-SMITH: Not just ignored. It's submerged, it's surpressed.

WARREN: I've examined this with great interest for years since he first 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 it must be done over many years and it must be done in the complete environment. In other words, the fish must be exposed during the time that they would be, say, comparable with the Hanford Works and you must follow them out where they must compete in the open environment and you must in some way get your hands on them again so that all systems have to be operative. In other words, you must not simply say because, well, they didn't die in the first 90 days or 20 days or the first year or something, that there's no importance to it. So in doing this I have very naively told Dr. Warren, let's see, 24 years ago, that, yes, we can do this. Well, I didn't realize that it would take me 24 years to get an answer, but that's about where we are now.

FREMONT-SMITH: You're going to telephone tomorrow afternoon and tell us what the answer is?

DONALDSON: One step of the answer.

FREMONT-SMITH: But go ahead, Staff. You were goin to comment.

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 ove the years, 20-odd years, with such a small yield in return o a few percentage of fish, that this has been maintained over the years and you're now in what, 26 ~~ix~~ generations. ~~which~~ This ought to be of interest to the geneticists here~~,~~ from some c the original exposures in 1943 or 4.

DONALDSON: Those are with trout in 1943.

WARREN: Those were with trout.

DONALDSON: Yes.

WARREN: But here has been the longest, to my knowledge, the longest single set of observations on one o more species of fish that have been exposed to relatively small amounts of radiation, ~~and~~ I think this ought to be co tinued as long as it's 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 m in not claiming too much too early. But I think this is as important as following the Nagaski situation, where the dosages are not so well controlled.

FREMONT-SMITH: May I make a comment also on this, because 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's some evidence to believe that there was a higher radiation in the past than there is today and that therefore 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 and just wanted to make that hazardous statement. I know it's 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, and 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.

FREMONT-SMITH: Yes.

DONALDSON: I cringed just a little bit, Dr. Warren, when you talked about small in numbers, because I've made the grandiose statement that this is probably the biggest numerical experiment tha's been carried on radiation studies with vertebrate animals, not with Drosophila or something like that 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. So even---

WARREN: The salmon gives a percentage of return, as you indicated.

DONALDSON: Yes. Even if we get a 1 per cent return, we have somewhere between--never less than 2, but 2 to 5 or as many as 6000 salmon coming back in the University pond that is just slightly larger than this room. When you have that great number of these adult beasts, the average weight last fall was 8.6 pounds, coming to a tiny place like this in a two-week interval, you have a tremendous mass of at least physical material, but you also have a fantastic number of measurements to make. So you're stick problems get astronomical. This population would produce at least 5 million offspring each year. So with 5 million offspring to evaluate and follow through step by step all through their incubation period, determine the number of anomalies, determine the rates of growth, individual variations between lots of some thousand or 1200 lots, you need more than a computer, you need a bunch of trained monkeys, as we saw in the film.

FREMONT-SMITH: How large a staff do they provide for you to help you with this?

DONALDSON: This was a question that was asked me last week by a group of Russian geneticists.

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 guys out there catching them out of the pond and going through all these ablutions that they go through.

AYRES: What do you do after?

WOLFE: During the off-season I don't know how many people there are.

FREMONT-SMITH: What I'm trying to bring out, 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 that one learns being a schoolteacher, Doctor Fremont-Smith. The fish usually come between August and September. The school doesn't start until the 25th day of September. So the return runs the 25th day of September. This year it was the 26th, but it's close enough. Then I have the 25 or 50 students who can help me.

WARREN: He orders the fish to return on that date! [Laughter]

FREMONT-SMITH: I think you ought to go the University to start on that.

DONALDSON: The fish normally go to the sea during July, maybe as late as August, but that's inconvenient because school lets out in June. So let's have them go the sea the first day of May and we'll speed them up and get them out the first day of May. 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. 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 to you give them over what period of time?

DONALDSON: The dose has been increasing year by year. We started out at .5 r per day; went to 1 r per day, then to 2, now 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 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 them in a chamber and expose them to your cobalt-60 source, expose them for this 100 days. At the end of 100 days they are ready to start to feed and then you start to take them out. But at the end of that 100 days they're going through their entire embryological field. They're fully formed.

BRUES: There's some evidence appearing now that the earth's magnetic field flops over every so often which lets in little meteorites and cosmic radiation. I believe the last time that this was supposed to have happened coincides more or less with the time when man first appeared on earth. That is rather speculative, of course.

FREMONT-SMITH: Do the salmon get bigger at the same time, too! [Laughter] Go ahead. I didn't mean to interrupt you.

BURES: No. This is the whole story.

WARREN: He has indicated that there are periods of bursts of irradiation which do affect this at different times due to the shift. Lauren ought to also tell you that he has men study all of the abnormalities that can be produced in these fish with irradiation and there's a certain mortality from this, depending upon the dose rate. You get all of the abnormalities that have been ascribed to this 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 and exposed at the levels as far as we have gone.

FREMONT-SMITH: No increase?

DONALDSON: No significant increase one way or the other.

TAYLOR: What is the LD-50 dose for a salmon?

DONALDSON: An acute dose is between 450 and 500 r.

TAYLOR: You'regiving them about 500 r, aren't you?

DONALDSON: Chronic exposure.

WARREN: Daily.

DONALDSON: We'll give 500 r this year.

AYRES: That's a time when cell reproduction is rather rapid, though.

DONALDSON: That's right.

WARREN: At their rate of maximum 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 range. 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. And the prompt lethal dose of the monkeys is about 550 r.

FREMONT-SMITH: Please, gentlemen, don't have a private conversation because it makes it impossible.

UPTON: I think a smiliar experiment has been in the mouse. I think I seem to recall that Russell could detect defects in mice exposed to dose levels of 25 r in the embryonic period whereas if they administered something like 7 r a day continuously given over a 24-hour period without embryogenesis, they observed no effects, due presumably to the lower dose rate.

DONALDSON: I think if there's an real unusual thing about this experiment we seem to have drifted into, is that the total response has to be functional, that is, they must memorize their migatory pattern out to the sea, and memorize their migatory pattern coming back, and this requires an extremely astute sequence of mental gymnastics. They must compete in a very competitive environment in the sea. They must survive and reproduce and continue on.

Well now, what are the effects of 20th or the 30th generation? Well, I'm never going to live long enough to know because it takes us about four years to do an experiment, one cycle and the subsequent cycles, but we are in the F-3 of some of the groups now and we'll continue to grind along as long as our energies hold out.

FREMONT-SMITH: Do you want to tell us briefly that fascinating story about the olfaction and how they do find their way?

DONALDSON: I keep watching the clock.

FREMONT-SMITH: It's so exciting I think we ought to just get a flavor of it.

DONALDSON: This is the work of Dr. Gorbman. Dr. Gorbman is the same chap that worked on the iodine uptake. He has been doing memory pattern responses by taking the salmon at the return and immobilizing them, lifting the skull case off, putting probes in the olfactory lobes and then dropping water on the olfactory nerves step by step down the environment.

FREMONT-SMITH: Down the river.

DONALDSON: Yes, down the river or up the river or some other place or tap water and getting the input directly in measuring their memory response for this particular environment or 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: It's sensitive to such infinitesimally small amounts. Then you can take it down and distil the water on and on, and they are even so sensitive that you can move up above for 100 yards on up the watershed where they haven't experienced it and 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!

AYRES: It confuses the hell out of them! [Laughter]

DONALDSON: May we come back to the subject at hand for the moment and 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 relationships with Japan since 1954 and how these environmental problems were handled on a bit 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 undercover operation was to contact one of our good friends in Japan, one of the leaders in the SHUNKOTSU Maru expedition that caused so much problem in the 1954 era. One of the chaps agreed to collect and evaluate sample of tuna fish that were caught by the Japanese fleet. He collected some 2000 samples, sent us half of the samples; he kept half of the samples and then we made our evaluations, they made theirs and we compared them. But he couldn't get his published in Japan, but that didn't necessarily matter. There were available these data in Japan. But since they were not the sort of exciting things that would make a good news story, they are part of the scientific record but are not a part of the popular record.

In 1964, during the high altitude tests at Christmas Island, this program was again repeated and Dr. Carl Botter again collected the samples and sent them to us. But under some very real pressures on the part of the hysteria-minded group in Japan there was floated an expedition to evaluate the radiation hazard by a group of reliable scientists. The ship was equipped and sent out and we were advised and we met them in Honolulu in June of 1964 and had long conversations with them 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, to say it very frankly, they did not expect to return home. They were perfectly willing to give their life to the cause, many of them. This was particularly true---

FREMONT-SMITH: They expected to be killed by the blast?

DONALDSON: They expected to be, at least at the very minimum, extremely affected by radiation fallout.

EISENBUD: What year was this?

DONALDSON: 1962. It seems fantastic again or incredible,to use a much used word, but they had the most elaborate air-conditioning system I've ever seen. Every porthole was plugged. They had long filters installed. The ship was equipped so that it could be operated entirely without anyone being on deck; almost a periscopic peekhole and they wanted from us assurance that they could go into the area and possibly survive, but how would they best orient it. "Well, we just came back from down there. We've been traveling around."

"Where were you?"

"Right at this point, that point, that point."

"But your health is good."

"Sure our health is good. Why shouldn't it be?"

Well, the ship left Honolulu; they made their stations, they went home and we arranged again through the Division of Biology and Medicine and the Commission sent Dr. Gordon Dunning over to chair the meetings where we brought all these data together, their data, our data, and we pooled our resources. We did a correlation study even eventually and found that we had significantly the same--it was significant--I've forgotten the exact degree, but at least it was significant that the results that they had and our results were in agreement.

FREMONT-SMITH: Were they awfully surprised to come back alive?

DONALDSON: They were tremendously pleased, I guess, to live.

DUNHAM: You said they were very sophisticated, knowledgeable scientists.

DONALDSON: They were very sophisticated, knowledgeable scientists. I qualify this to say that it was the seamen on the ship plus these chaps. But the precautions that they had and the facilities that they had were so completely out of keeping with anything that we had available to us or that we had ever seen actually.

EISENBUD: How close in did they go?

DONALDSON: The exclusion area was 200 miles. So they were close.

EISENBUD: The Shunkotsu Maru came that close in 1954 and they didn't seem to be too concerned about it although it was interesting, 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 and 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. But at the conclusion of the meetings there was to be a press announcement and the placed swarmed with newspaper people; it just literally swarmed. They had television cameras, newspaper photographers. The place just buzzed. The prepared statement was handed to the newspaper people saying that we were in complete agreement and that the levels of radiation were such-and-such and such-and-such. And you should have seen the expression on these men. "But there are not great amounts," they would say. "No. These are the findings of the joint report." And we searched the papers the next day and about an inch and a half appeared and I don't think any of the footage was used on television.

BRUES: Lauren, should we set up the projector. You wanted to show a picture before lunch.

DONALDSON: Yes.

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 per cent of the MPC in the people. The children had slightly higher values for the strontium-90, to 20 per cent in some cases. But it was estimated that the total body dose from all of these internally deposited isotopes only amounted to several hundred milliroengtens 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 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 calibrated what the total dose would be if they had remained on the Island continuously, but certainly it's not anywhere near in the range of the acute immediate hazard.

ROOT: You mean that's a good shelter hypothesis then if you can get them all under shelter while the actual fallout was taking place? They could emerge the next day without perhaps danger?

CONARD: I wouldn't say the next day.

AYRES: That's a standard self-defense notion that you shelter for a couple of weeks and during that time the drops by a factor of 100 and then you're probably all right.

ROOT: Yes.

CONARD: Most of the radioiodine by that time has decayed.

EISENBUD: I would like this off the record.

[Off the record]

FREMONT-SMITH: Back on the record.

EISENBUD: Things had quieted down in the summer of 1954 and then I guess we forgot to mention yesterday that the Russians started a test 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 any way, which lead 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 and they wanted to have a radiobiology conference and they invited the Atomic Energy Conference to send a group over, and about a dozen of us went over in November of 1964 and sat with our counterparts in Japan and had two weeks of very worth while discussion with them.

Interestingly and apropos of the remarks I made yesterday about the schism in Japanses medicine there, 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 afterwards, of course. This is very interesting.

But out of that conference we saw some Japanese data in which their SHUNKOTSU MARU expedition, I think--was it in May of 1954--I think it 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: May 24th they left.

EISENBUD: They sailed into the equatorial current just west of Bikini and took profile measurements which indicated that about 200,000 curies a day was 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 which wasn't discussed very much and I read it on the way back and I got interested in it and as a result of that and the fact that it was a simple extrapolation to show that this device 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, and 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 at I believe in March, about a year after the 1954 event.

DONALDSON: 1955.

EISENBUD: And gave 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 from--well, 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 and 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. Also we obviously said it was high enough to be interesting but low enough so that we didn't really have to worry about it.

BRUES: Do you want to say anything before the picture is turned on, Lauren?

DONALDSON: No.

BRUES: Let it be turned on then. Lunch will be had in ten minutes instead of 25 minutes and we can continue with our discussion if anyone has anything to discuss until twelve o'clock , and we'll show the picture later in the day. Everyone run out of talk?

DOBSON: I would like to ask Lauren Donaldson a question. Perhaps it's not too well phrased and perhaps the question is too large. But extrapolating from the experiences that you've had over the years with your ecological studies, what kind of situation would you visualize, let's say, in the western part or region, the Washington region of the United States if a sizable number of nuclear devices were exploded? I'm thinking of the aquatic animals, the river systems, the terrestrial, and so forth. It's a fuzzy question. I don't mean an overwhelming number, but choose your number.

DONALDSON: One could approach this with 180 degree differences either way. If one wanted to choose for the moment, say for the sake of argument, we would have to go back to our original comment that in water you are dealing with a three-dimensional aspect. You deal immediately with fractionate of nucleides. Then you have selective concentration of nucleides and they are selectively picked out by different sections of the biota. In vertebrates as a group being different almost than vertebrates, you have the food chain series. Which stage of the food chain is one interested in fish, the herbivores being more specific than the carnivores? So to make a blank statement there would not be an effect, there would be an effect, would be almost ridiculous. The qualifications would have to be so numerous that I think one could almost without question say that a device in the area over a city or away from the immediate contact with the water, there would not be much concern. A few minutes, a few hours, at most, and it would be of little concern. It would be an academic problem, some of the ones we've been talking about today. On the other hand, if it were in a harbor and under the water or in the water, this would introduce a whole new series of parameters because of entrapment of materials and the immediate availability of both fission products and nonfission products and induced radiation to living things.

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'm assuming that.

ROOT: A high burst, you see.

DONALDSON: I'm assuming a high burst in contact.

DUNHAM: I would like Dr. Wolfe to comment on this question because I think I know what Warren is driving at and that is that the earth is so different on the atoll than that of 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 in the coniferous forests of the Northwest that 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 you have both radiation and fire and in the coniferous forests most of them can be rather disastrous in areas of high radiation. I know that there have been those who speak lightly as fire as a factor in nuclear war, but I noticed in this last fire, the fires in the Northwest, that you had available manpower and you 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.

DONALDSON: This is the sort of fractionation of a question I imagine one would expect from basically an aquatic biologist as contrasted to a terrestrial ecologist. Immediately my interpretation was "Well, the only things that are important in this world are those that are associated directly with the water, water mass, this being the ocean. Then, back to some of it, say, we had yesterday: What would you do if the area was contaminated? The same thing that we were doing at Rongelap in the early days. We would run on, sure, grab a sample, and then get out and stand in the water up to our necks until someone came to pick us up. Sometimes that was a long time, quite a wait, but this is just to emphasize the difference between the two environments, that is, where you have a point source as a three-dimensional. There isn't any reason to assume that per area originally there wasn't just such fallout on Rongelap lagoon as there was on the land area. But if you spread it, plus the shielding, you have just different problems.

You're dealing in another media.

ROOT: Do I understand that you are referring to particulate fallout matter in the water which goes into the food chain and Dr. Wolfe is referring to radiation? So that high burst, your high burst would not be so effective on the water but you're referring directly to radiation and not the fallout, aren't you?

WOLFE: I'm talking about the radiation that gets there, whether it's from fallout or any other source.

ROOT: Yes. I mean you would get it in a high burst whereas you would not get it on the water. A high burst wouldn't be so damaging because 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 Brockhaven, why do 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 or Argonne or anybody else could have predicted accurately or could have discovered the thyroid difficulties that Bob Conard has reported on. And you've got to go where the action is.

ROOT: Sure.

WOLFE: And I don't know how I can put it in language to you, but I don't know whether we could put it on paper for the Joint Committee, Chuck. We miss your fine hand there.

DUNHAM: The Atomic Bomb Casualty Commission is always being sniped at in top quarters that I think we go where the action was--I'll change your word immediately--it's awfully good.

WOLFE: We've got a different environment; it involves different biota and different meteorology and different climates and different relationships altogether. That just is 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 reactor.

TAYLOR: Are these ecological studies?

UPTON: Yes. From the 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 chemistry is involved, biochemistry, and the other is 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, because 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 the 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 effected by the fires?

WOLFE: I would doubt it.

BRUES: I'm going to adjourn the meeting for lunch now since the management has offered to have it early for us.

We will convene then at one-thirty instead of one forty-five. I will ask you and Dr. Langham to get together and decide which is the most appropriate time to show the film, assuming we can get it turned around.

We stand adjourned.

[Adjourned at twelve o'clock noon.]