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In the Matter of:

ECOLOGY CONFERENCE

FRIDAY MORNING SESSION

Date: May 20, 1955

Washington, D. C.

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United States Atomic Energy Commission

ECOLOGY CONFERENCE

FRIDAY MORNING SESSION

May 20, 1955

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Washington, D. C.

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ATTENDANCE

Atomic Energy Commission:

Biology Branch:

Dr. Paul B. Pearson, Chief, Division of Biology and Medicine

Dr. W. R. Boss, Physiologist,

Dr. Nathan S. Hall, Soil Scientist

Biophysics Branch:

Dr. Walter D. Claus, Chief

Dr. Gordon Dunning

Dr. Douglas L. Worf

Dr. Robert A. Dudley

Division of Biology & Medicine:

John C. Bugher, M. D., Director

Charles L. Dunham, M. D., Deputy Director

Medical Branch:

Dr. Bernard R. Nebel

Victor E. Beard (Idaho Operations Office)

W. D. Billings (Duke University)

Murray F. Buell (Rutgers University)

John T. Curtis (University of Wisconsin)

Kenneth L. Englund (Hanford Operations Office)

Richard F. Foster (Hanford Operations Office)

Norman R. French (Idaho Operations Office)

Edward Held (University of Washington, Applied Fish Lab.)

Karl E. Herde (Savannah River Operations Office)

Frank P. Hungate (Hanford Operations Office)

Bostwick H. Ketchum (Woods Hole Oceanographic Institute)

Louis A. Krumholz (Lerner Marine Laboratory)

Kermit H. Larson (University of California)

Robert G. Lindberg (University of California)

Frank Lowman (University of Washington, Applied Fish. Lab.)

Eugene P. Odum (University of Georgia)

Orlando Park (Northwestern University)

Ruth Patrick (Academy of Natural Sciences of Philadelphia)

D. W. Pearce (Hanford Operations Office)

Frank A. Pitelka (University of California)

Paul B. Sears (Yale University)

Royal E. Shanks (University of Tennessee)

C. Samuel Shoup (Oak Ridge Operations Office)

Edward G. Struxness (Oak Ridge Operations Office)

John N. Wolfe (Ohio State University)

FRIDAY MORNING SESSION

...The Friday Morning Session of the Ecology Conference convened at nine o'clock, in Room 1201, T-3 Building, Washington, D. C., Dr. Charles L. Dunham, Deputy Director of the Division of Biology of Medicine, presiding...

DR. DUNHAM: I think we had better call the meeting to order.

First, let me express Dr. Bugher's regrets that he cannot be here. He was not able to be in Washington at all today, although I know he takes great interest in the prupose of this meeting, and he had originally had every intention of meeting with you.

As Dr. Pearson has probably already indicated, this is the first full-fledged go-round between the Atomic Energy Commission's supported ecology investigators, many of whom may not be ecologists, but, nevertheless, their approach and concern as to what is happening to radioactivity are at least in my ignorance ecological in nature.

Most of these programs have been going a number of years and cover, I think, quite a wide variety of topics from marine biology on into streams and various land and air forms.

Dr. Pearson has brought together with this group a number of you whom we consider to be outstanding ecologists in the United States, and we hope in presenting our program to you that you will be sympathetic and, at the same time, critical in a constructive way. In other words, we want help and guidance at this point to see that our program is going in the right direction and suggestions as to where emphasis might be increased and perhaps also the effort itself can be increased considerably.

I was having breakfast this morning with my family and my wife was telling me a story about a little experience she had out at the Parker Laboratory in Montana recently.

All of the mice in the colony were developing hernias and then there was a complete fall-off of procreation. It was simply a question of determining whether something had gone haywire in the mice environment. It turned out that these mice were being fed food from a nearby food supplier who was supplying cattle, and I believe it is still not legal to put large doses of estrogens in the food, and, of course, they were rupturing their mice. I assume this falls in the field of ecology, but perhaps on a micro scale. What we have to think of is perhaps not large doses of some particularly highly specific substance but a fairly wide-spread low-level contamination. I think I have said enough. Before we go on to the first paper, Dr. Boss has a few announcements to make.

...Announcements...

DR. DUNHAM: Kermit Larson is associated with our project at the University of California, and he is probably the very first person to get involved in the ecological implications of the detonation of atomic weapons. This work goes back to the very first Trinity shot in New Mexico, and he and the group associated with him there at the University of California have almost from the very beginning carried on studies of the transfer of radioactivity in and out of the very close-in crater area and in the environment.

DR. PEARSON: May I interrupt just a moment, Dr. Dunham, to say that I come first from the classification office with authorization to talk about strontium results in the United States. This has been classified up to the last ten minutes.

DR. DUNHAM: Strontium is one of our principal problems, so I think it is very nice that we have officially allowed this meeting to discuss it freely.

We will now have the first paper by Kermit Larson.

MR. LARSON: We started our work at UCLA in 1947, studying first the Trinity area. This is in New Mexico. It is about 75 miles southeast of Socorro, New Mexico.

We started with the problem of finding out what fall-out was, what it might have done two years after the detonation.

A little bit of history on this: When they did have the detonation, the best estimates were that the cloud, for example, was only to go something on the order of 20,000 feet. The fall-out, if there was any at all, would probably go something on the order of 10 miles downwind. There were a few monitor teams distributed around the territory plus a lot of people who were anxious to see whether the detonation would work or not.

When it did happen, the cloud kept going far beyond what anyone had anticipated. The monitors for the most part took off, some to Socorro and others to Albuquerque. The plane that was supposed to do the observing from 30,000 feet decided that the cloud was going far beyond where they could go, so they took off also for Albuquerque. In other words, there was very little information on the fall-out or where it had gone.

In 1947, we took a group of 13 men to Alamagordo site and we started out with the crater because that was novel, new, and we wanted to know something about the radiation intensities that were in and aground the Greenglass surface. This extended some 1600 feet in diameter. In the early days, it was one solid sheet.

Working with this material, we found several things such as the uptake by radish plants, which was nil. The uptake of activity by animals collected on the Trinitype or adjacent to it was nil. Then we extended ourselves in 1948, after having just had a preview in 1947, effectively three weeks work, and we spent three months outlining the pattern.

This map illustrates that we did find. This (indicating) is about 100 miles from ground zero. The levels of activity were such that we had to use the beta gamma survey meter with window open measuring intensities at one inch above the ground, but we were able to outline the pattern of fall-out. We spent most of the time in close, as it were, the first third, if you will, and this (indicating) we detailed.

(Map) This represents 30 miles. The map is one inch to the mile, and we have coded or grouped the radiation intensities that we found, calling this the isotope line, this being two times the instrument background, arbitrarily set at that because we could not spend too much time tracking it down.

This was all cross-country work. The first thing we did was establish the primary reference line by regular civil engineering techniques, permanently marking each location.

Taking jeeps or going afoot, we measured either side of that reference line every two-tenths of a mile, this (indicating) being the highest which is only 6.4 miliare per hour, or greater gives us an idea what the configuration was in here.

Then we had this pattern that laid down initially a very general, low-level next, and then we run into this (indicating), this being a so-called hot spot.

This was what they found more or less charactericelly ever since. There had been hot spots of various dimensions some distance on but very much lower than what is in close.

This area (indicating) we know now is where the cloud more or less stopped. It went up to its maximum elevation and then it dropped everything, all the rocks, as we call it. These are particles. We had actually collected particles that are a quarter-of-an-inch in diameter in close.

We have worked in this area (indicating) every August from 1947 until 1950. So far, we cannot say that there has been any observable damage to anything we have looked at, which includes the plants, the animals, the Kangaroo mice, jack-rabbits, and the reptiles, etc. The devastation that was present inside the circle here, which is now fenced in--fenced in in the early days by the Army for security reasons and not necessarily because of the radiation. This had a re-invasion pattern. Not being ecologists but observers, we observed such things as prairie fires, and so forth. First of all, we noticed that the Russian thistle came in in predominance, depending on the rainfall. As soon as this happened, the population of the Kangaroo rats were on the fense where the Russian thistle had accumulated. The sand had drifted. This was an ideal collecting area for us, but there were no damages that we could see. There was no activity observable in the animals here. We found traces only up in this area (indicating). This is what tipped us off to the distance effect that we have been, just recently, in the last series in Nevada, emphasizing.

Dr. Lindberg and myself, Norman French, Karl Herde and many others here--their organiztion at least is represented--finished up the program in Nevada which lasted for the duration.

We are particularly interested in the development of the cloud and its adjacent drift over the landscape and how this might influence the accumulation of various isotopes in the plant life as well as the animals.

If we visualize a detonation, in the first couple of winks of an eye, they have this fireball. (Chalk Demonstration) In Nevada, we have two kinds of detonations--those that are on towers and those that are dropped from an airplane. We are particularly interested in those which are released from towers. If we visualize this as having had a tower in here, with the little bomb sitting up here, the fireball first, depending on the size, it will intersect the ground. Then, at a very fast rate, we have the typical development of the atomic cloud. There are two parts to this which we will use in our discussion. From here down, it is known as the dust column; from here up, it is known as the fission cloud.

Depending on the wind structure prevailing at the time, this will be dispersed over the landscape and in various concentrations. Usually this has most of the activity that has been generated.

A common figure that has been used for the dust column here, if you have the fireball intersect soil surface, you will have something on the order of about 10 per cent that is generated. In turn, this is what comes down in the distances that we have been working--out to two hundred miles. This will take off and drift with the winds that are present at 30,000 to 40,000 feet above sea-level. A good detonation will go up that far.

The work that we did two years ago out there indicated that we should probably take a look or try to design experiments that would allow us to study this fraction of the column just where these two start intermingling. This being very heavy material, it will drop down in something like, say, the first 20 miles. This will be all heavy material out to here in various sizes, and maximums, again, will been the order of some two millimeter sizes. From here on out, you get various gradations. We have measured variations at 125 microns, this being at about 140 miles. If we know the time of fall-out and collect soil surface samples, we can extrapolate where this originated on its downward path, so we have this sort of thing (indicating).

We feel that that portion is probably the one on which we should do some more work.

This time, we tried a different method of air sampling. In the past, we used a membrane filter or a felt pad called an "MSA," mine safety appliance. This time we used an additional filter which used liquids as the collecting media. We set four of them per station, and we had water in one, Kaolinite in another, thiolate in a third, a buffer solution, 7.6 in the fourth. The results that we have obtained show that we have some additional problems coming in that we do not understand.

For example, if we consider just for illustration here, at 20 miles, then 57 miles, and the results obtained at 140 miles, and the tenth normal A.C. story, as we call it, we have 97 per cent of the solubility of the material that was collected directly from the air at 20 miles. At 57 miles, you have 67 per cent; and at 140 miles, we have something on the order of 80 per cent. The water thiosulfate and the buffer were lower.

If we separate the predominant size that is dropped on the soil surface and the zero to five micron size from the soil, we have an entirely different story. This is 15.8 versus 18.9 per cent soluble. At 57 miles, we have 5.5 and 19.4. At 140 miles, we have 4.4 per cent and 18.7. The one suspicion that we have here with the 18.7 and 4.4, the point of contact with soil before we did anything with it. We will have to check this, and we have experiments in progress on it.

Incidentally, to show how recently this data is, our teams are just coming in off the field, the last group of them, this Friday, so we have had no chance to evaluate this material. We will check and see whether the time factor enters into this by a possible reaction with the soil in holding the difference between these two; in other words, fixing it so it is not available by our solubility studies.

In the laboratory, in order to answer these problems that we observe in the field, we have a group, and we have decided to divide it arbitrarily because of the nature of the problems into a soil plants section, a metabolic history section, and then we have our biological field survey section.

In the soil plant work, we have adopted the philosophy that if we study this problem from the same point of view that the soil morphologist does--in other words, if we consider the fall-out particle the same as a rock--then the soil-forming factors should enter into the picture and do the same thing to the fall-out particles as it has done to the formation or breakdown of the rocks to the formation of the soil.

Here we can consider the parent material which in this case for our purposes would be the fall-out particles. Of course, we have the climatic factor, principally rainfall, and temperature. Next, we have the effect of organic matter. Next we have the good old factor of time, and, of course, topography again applies only when we are considering the terrain.

We have had work going on in the organic matter problem, and assuming that we have gone through a stage here so that we have entered into the clay picture of soil--in other words, the fall-out particle has been broken down and it has entered in finally into the clay and can then be considered as part of the clay complexion and a soil solution relationship.

We then have used the isotope strontium seriously--SR, CE, CS, RU, and Y--and growing plants on these things which include the crops, let us say, such as barley, radish and string-beans to see what the uptake might be from each of these isotopes.

Now, the independent soils used are those which have a definite clay type. In each case of crops, we found that strontium was the only one we had to worry about initially. So, we might classify this plants plus the strontium, and then you have the unavailables over here (indicating).

Since we found then that strontium is the only one that we need to consider initially, we used this strontium isotope plus the organic matter picture to find out what the influences are. We find that the higher the organic matter content, the more unavailable the strontium becomes.

Another study we did was to take eight different agricultural soil types, including one from Rutgers University, the sassafras, and compared it to the California as well as the Nevada and New Mexico soil. In all cases, using these crops, again, we found that the sassafras permitted the greatest accumulation in the crops. There are various ways that we might explain this, but I think it suffices for the moment to say that there is a very definite and a very significant difference between soils; and the more work we do on that approach, the better, I think, we will understand the total picture.

The effect of strontium itself--in other words, the effect of stable strontium--if we plot activity accumulated in the barley crop versus increments of stable strontium and keeping the activity in the soil constant in each interval here with respect to stable, we find that the cumulation does this--it increases as you increase the carrier. If we substitute calcium for this, we get this sort of a picture (indicating) So, now we have the problem, and we are learning more and more about that from all combinations of work that are going on in the various laboratories; that there is an equilibrium that is going to be established here some place depending on the stable stontium versus the calcium.

We have some indirect evidence that the aeration, the soil texture problem, is important. We did this by controlled experiments in the greenhouse where we cut off the aeration to the root system in ultra-solutions. We found even a 36-hour period of minimal amount of aeration decreased the uptake in the plant material. Light has an effect upon the uptake, and so does temperature. If you lower the root temperature from 27 C. to 17 C., there is an increase in calcium as well as the stontium uptake. If you lower the light intensity from 1,000-foot candles to 450-foot candles, you notice that the phosphate particularly is decreased, although not so much on the other two.

DR. DUNHAM: Dr. Larson's paper is now open for discussion. I am sure he has a lot more to tell if you just ask him.

FROM THE FLOOR: What are the stable amounts of strontium and calcium in this picture? Are these in equivalent amounts to show the reversed curves or not?

DR. LARSON: No, it was not an experiment done on various levels. It was one separate experiment for the strontium level and then we took the carrier level but using a normal soil.

FROM THE FLOOR: You have your two curves here. Say, over at this point, your furtherest point, is that the same number of equivalents in calcium as it is in strontium?

DR. LARSON: No, it is not.

FROM THE FLOOR: What do you mean by unavailability when the strontium is there? What you mean is that it is not being taken up by your plants; is that correct?

DR. LARSON: That is right.

FROM THE FLOOR: In other words, you do not know what effects it will have as it remains in the soil with respect to soil biology or microbilogy. It gives the misleading impression that it disappears.

DR. LARSON: In soil chemistry, we have three breaks or three divisions--that material which is water soluble, that which is exchangeable, and that which is unavailable or unexchangeable.

With water treatment, you take a soil and run through a known amount of water. With exchangeable, you run through an amount of ammonium acetate; the difference of the two by this treatment is supposed to represent what is or is not available to the plant root.

FROM THE FLOOR: My point was the stuff is down there and it is emitting radiation even though it is not available to the plant.

DR. LARSON: It is there and it is going to bombard the roots, but it is fixed in such a way that the plants cannot transfer.

FROM THE FLOOR: In other words, things above the ground are not going to be affected by it, but underneath it will---

DR. LARSON: We are just getting experiments started to try to quantitate breakdowns due to various weathering processes. This is a joint operation between Dr. Overstreet at Berkeley and our group.

FROM THE FLOOR: I would like to ask about the size of those particles you had at the extreme right of your distribution curve--125 microns which is very large for the condensation which is involved in the formation of rain. In the other experiment, you used the zero to five. Was the 123 the predominant size at that distance?

DR. LARSON: It was the predominant size with respect to the distance that was there. We did this by mechanical civvy, and the 0.44 micron which was the smallest fraction we used--we used aerilutriators to break that further. The zero to five represents what we consider the clay fraction, and while this is not strictly soil science, it does come into the picture as far as we are concerned because it does relate to the particle size that we found on the dry filters, and, for the most part, if we use an average figure of 1.4 microns, which was collected on the filter, this will be a happy average throughout the distance. It varies perhaps .2 plus or minus, but we were able this time to measure the particle size on the filter directly. This is what we found on the filter, so presumably we have not been able to do that work and confirm it on the liquid collection; but presumably, that is what is being connected in the liquids.

FROM THE FLOOR: This is on the air filter?

DR. LARSON: That is right; we get an average of 1.4.

FROM THE FLOOR: I would like to ask about the uptake. You said that temperature and light increased with some of these available things from soil.

DR. LARSON: This was done in culture solutions.

FROM THE FLOOR: Did you measure transporation?

DR. LARSON: No.

FROM THE FLOOR: You have no information with respect to the effect of uptake on phosphates, etc.?

DR. LARSON: We tried to govern this by keeping the chamber controlled; that is, the temperature was held constant for the lethal surface, and we altered the temperature for the roots only.

FROM THE FLOOR: In other words, you did not have these aerial parts in different temperature or in different lights?

DR. LARSON: No, they were held constant.

FROM THE FLOOR: I would think the transporation would be involved.

DR. LARSON: We have a graduate student working on it now. That is the next phase of it.

FROM THE FLOOR: When you grew those various plants, did you estimate the amount of growth? Did you get any dry weights on them?

DR. LARSON: Yes. I do not have the figures tabulated here.

FROM THE FLOOR: Did you run a control of unradiated plants at the time?

DR. LARSON: Yes, sir. We could see no difference with respect to wet weights or dry weights.

FROM THE FLOOR: Why did you pick those particular weights?

DR. LARSON: We had a limited amount of greenhouse space. We wanted a cross-section of agricultural crops, barley being generally thought of as a grass, a root crop, and that is all. That is the kind of basis for selection that we made. It was not because any one crop was better than the other. We wanted some quick answers; therefore, the problem of a rapid-growing crop also entered into it, but predominantly it was the idea of getting a cross-section of the various types of crops that are of agricultural interest.

FROM THE FLOOR: A lot of work has been done on tomatoes and tobacco. Do you have any comment to make on them?

DR. LARSON: We are now using a tomato to refine some of this data.

DR. DUNHAM: I am sure there will be an opportunity to ask Dr. Larson more questions later, but I think we had better move along with our program.

Our next speaker, Dr. Lindberg, is also from the University of California, and his emphasis will be on the uptake in animals.

DR. LINDBERG: Dr. Larson has successfully contaminated the environment and given us some idea of the nature of the contamination. Perhaps the logical place to pick up is to try to answer the question, "What is the biological significance of this contamination? What does it mean as far as the plants and animals are concerned which are living in these environments?"

First, we could summarize the Alamagordo work by saying that the surveys pointed out that fall-out did exist. Secondly, that fall-out was extensive. It reached as far as, at least, 100 miles, which is as far as the work at the Alamagordo. The radioactive material was metabolized by plants and animals, and, thirdly, there was some indication that the availability of this material changed with time. That is, in the 1947 survey, various levels of contamination were measured in or on plant-life and animals, and in 1950, that survey indicated that there was this amount of contamination that had increased. These were very low levels. In fact, our accounting methods were questionable.

Nevertheless, it was residual and we could pick it up.

With this backlog of information, we set out to document the area adjacent to the Nevada testing site. The test site was not used prior to 1950. In 1951, we had the first series, and in 1951 certain background samples were taken of the soils, plants and animals in the area around the proving ground. I can give you a quick diagram of the area. (Chalk demonstration) This is the proving ground itself and the various test sites and ground zero are scattered around here--Yucca Flat and Frenchman's Flat to the west is a large lake. To the north is a valley that has no good name, and to either side of this, east and south, are running mountain ranges. This is typical of the whole area; the flora and fauna have many of the components of the Mohave Desert. The flora and fauna are simply enough so that we can go out and pick up the same species of animals and plants and use them for controls in laboratory work to enable us to interpret our field data. The general level of this valley in here is about 4,000 feet. There is Bald Mountain up here that gets up to about 9,000 feet. This is on a slope. This gradually goes up to about 5,000 . These valleys are very characteristic of the Southern Nevada area and those mountain ranges.

In 1951, the first samples that were taken were in this general area around this lake. The first test series were predominantly around this lake and were predominantly air drops, and the degree of residual contamination is residual particularly in the areas which we decided to study in 1952.

In 1952, we established one area up here, and another one here (indicating); another one here and another one here. Those areas were chosen because they were what we considered to be in the path of a fall-out in case of future tests. We could expect the areas to become contaminated over and over again.

We established the areas by footing in 2,000-foot intersecting transsects, and along these transsects we plotted the perennial species of plants that were there. We ran trap lines for establishing the population structure and abundance.

In this valley to the north, the predominant association is salt wood, atroplex, of which there are two species. There is the atroplex folia that tolerates heavy soil and occurs down here in the dry area. Atroplex connoscense is another salt bush which tends to occur up here more on the rocky slopes. As you get up into the mountain ranges, you begin to run into artamecia, the sage and the pinyon pine and the juniper. We have a great variety of habitats, and they are quite sharply defined. You can look at a topography sheet and you can tell what species will occur on this side of the line and this side of the line. It is a good place for ecological studies.

There are joshua trees, yucsha folliage. At the present time, they are only found on the base of the high desert ranges to receive something like eight to ten inches of rainfall a year. Eight to ten inches of rainfall in this area evaporates in one day, so it is a rather arrid place. I can see a couple of people smirking out here over that statement. We have just been participating in this last test series, and most of the areas we were covering were under snow, and between fighting the rainstorms and the blizzards, we had quite a time of it. You would never believe it was a desert situation.

We have been dealing with the following species of animals out there: the jack rabbit, the kangaroo rat, the pocket mice, the parathomases, the general group of white-footed mice, paramiscus, etc. We have been unable so far to find any effect in the radiation area outside of what the total body burden would be. In other words, how much activity is in the body of the animal as a result of contamination by fall-out. These have really been below anything to worry about.

It is very important, however, that we take advantage of this situation and learn as much as we can about metabolism of this residue before an emergency really does exist.

On the Alamagordo survey, it was noted that the greatest amount of uptake by plants and animals did not occur in the area adjacent to ground zero; rather, it occurred some 20 or 30 miles away. In other words, it more or less corresponded to this hot spot that Dr. Larson showed.

When you start looking at the mechanics of fall-out as we have been picturing it where you have your column in a a cloud and this material drifting out over the landscape, you begin to wonder if this area in here (indicating) would not be carried some distance from ground zero before it is really deposited. This type of observation suggests that the availability of these bomb residues may vary with distance.

Following the test series in 1953, we had an off year, so in 1954, we tried to re-establish or tried to find the mid-line of fall-out resulting from one of the detonations, as it was indicated by residual contamination a year later. In other words, a fall-out generally went in an easterly direction. By going out a year later, making transsects along these paths, we were able to find out what the area of greatest contamination was. Again, we had to use very delicate survey methods in order to pick up this radiation. In each of these localities we might try to graph it to some extent.

At four miles, at about 12, 80 and 140 miles, we took samples of the surface soil of the predominant species of plants and of the native animals, concentrating on the jack rabbit and kangaroo rat. We were able to sample pretty much similar environments. The residual contamination on the soil dropped very sharply with distance at four miles, which is actually within the proving ground itself--in fact, on the edge of some of the target area. The contamination was 20 microcuries per square foot. At 12 miles out, it was down to about 6.6 microcuries per square foot. Incidentally, this 12 miles is still on the border of the proving ground itself.

At 80 miles, the contamination was down to .94 microcuries per square foot, and at 140, it was .04.

In graphing, you would find that the contamination would drop very quickly. We notice in all of the virgin or undisturbed areas the contamination was limited to the surface inch of soil. In one or two places, we were fortunate in being able to come across cultivated fields that were close to the mid-line. In this case, we found that the surface contamination was not as high, but the contamination was distributed, as you would suspect, all through the cultivated area. If you tried to make the corrections and account for the total activity for the activity covered by a square foot of soil, we find the total contamination is very similar. In a cultivated situation, this was being mixed in and being brought down more available to the roots.

The plant species that we sampled were atroplex, for one, chrysanthemum, which is a composite, for another, and laria. There were a few other species thrown in also. Even though the total residual contamination dropped very sharply, the level of contamination--this is not to the same soil but just to show the relative slopes--the activity was, at the most, three times greater than the background data we had established in 1951--again, very, very low.

In sampling jack rabbits and kangaroo rats, we find that the total body burden throughout this entire range was also very constant, and here was something that was rather interesting to us because while we had our contamination dropping off very rapidly, total body burden of the animals was fairly steady throughout the same area. As a matter of fact, there is a tendency for it to increase in distance for total body burdens. Expressed as total body burdens, it is a little hard to prove statistically.

Our previous work had shown that radio stontium was the one with which we were primarily concerned. Fortunately for us--unfortunately, perhaps, in the language--strontium-90 has a very long life, and we can use it as a tracer instead of in the persistence of this contamination in any particular environment.

We know that strontium is also primarily deposited in the bone.

If we express the activity in the animals, instead of in terms of total body burden, but rather in terms of strontium per gram of bone, we come across a figure something like this (chalk demonstration). There are these conversion factors. You can see that 47 is not much in terms of a microcurie, which is somewhere around the body burdens permissible of humans, but it is still very detectable. At 60 miles out, the contamination of the bone was running about 80 DPM; at 140 miles out, it reaches about 176, a definite increase of distance.

Of this, if we make the chemical determinations as to what portions are radio strontium, but of this total activity, we have 28 DPM in four miles; 37 at 80 and 64 at 140. This again indicates that the radio stontium increases at distances. 140 miles out is as far as we would work. It is a part of our program, and we intend to go out as far as 600 miles, if necessary, following this last series, to see just how far out this relationship will hold. Certainly it indicates in terms of the availability of the bomb residue that we cannot judge this in terms of meter readings alone or just environmental contamination. We are dealing with some fraction of the total fall-out that is not distributed in this manner but is distributed rather uniformly over a rather large area. This suggests the smaller particle sizes which will tend to be carried further and tend to diffuse a little further. Preliminary data seems to suggest that this relationship holds.

It is not enough to go out in an area and say this is how much activity is there. It has to be interpreted as to what it means or why it is there. Sometimes why it is there is sometimes easier than what it means.

At UCLA, we have more or less broken our work down into the phenomenology of what are the factors of different fall-out distributions which are the things Dr. Larson has just discussed, and then what is the biological significance of it. We have to have the two bits of data in order to get a proper prospective of this contamination.

In trying to explain why the body burden of radio strontium is what it is along these areas, we have resorted to the laboratory data which indicates that radio strontium is metabolized very similarly to calcium, and knowing the strontium-calcium ratio of diet for any particular animal, you can predict pretty well what the strontium load will be in the bone.

This work was done primarily by Dr. MacDonald of our group in the Bone Retention Section, and he has been able to show that the strontium is to the bone as the calcium is to the bone and as the strontium in the bone is to the calcium in the plant. On experimental diets, this value comes out to a constant for any particular species and this constant has been called a bone retention factor. This is a bit premature. We are still working on it, but it is a working scheme so far. If we determine the stable strontium--not the radio strontium but the stable strontium--and calcium in the plants that we assume these animals eat, and then determine the stable stontium and stable calcium in their bones, we come out with a bone retention factor that is in good agreement with our laboratory data. In other words, the bone retention factor for the white rat, the Dutch rat and the jack rabbit come out to very good agreement.

For the white rat, I believe it came out to .22; for the Dutch rat, it was .2, and using our observed data from the field, the value comes out to .19 for the jack rabbit. If we say that .2 is a good bone retention factor, and we know the amount of radio strontium that is in the diet, and we know the amount of calcium and the amounts of determinations, we should be able to predict what the body burden will be in any species of animals, and this works out if we assume that the total contamination we have measured on the plant at least 60 per cent of it is available. According to the figures that Dr. Larson just presented, this is not too impossible a figure.

In conclusion, we could say that our efforts in the field have been directed toward determining what type of contamination is present, where it is; second, why is it there, how did it get there, what are the factors that govern distribution? Third, what is the biological significance? And, fourth, if it is biologically significant, what can we do about the contamination?

We have generally outlined what our field program has been and will continue to be.

DR. DUNHAM: Dr. Lindberg's paper is now open for discussion.

FROM THE FLOOR: Taking your samples of your plants for this study, how do you do this? How much material do you take? How do you determine what is a representative sample?

DR. LINDBERG: We tried to take that part of the plant which we believe is eaten by the animal. In the case of the kangaroo rat, we stay pretty much to the seed plankton on atroplex. It looks like opium when you scrape it off. In the other plants, we take the top six inches of growth, and we take good quantities of it--about a bagful. We grind it, and then we express our activity in terms of dry weight.

FROM THE FLOOR: In a given area, would you take this material, say, from 50 different plants or 100 plants?

DR. LINDBERG: It would just about amount to 50.

The general procedure is to take a nice big bag and walk through the area taking branches off the various plants, filling the bag, and then taking tree bags in this manner, and I would say we sample at least 50 plants in the area and cover at least 200 or 300 square feet.

FROM THE FLOOR: Have you ever done any studies to determine the variation in plants in a given area?

DR. LINDBERG: Yes, we have, and we get a reasonably good correlation.

FROM THE FLOOR: You mentioned something about joshua trees. What evidence do you have?

DR. LINDBERG: I am only quoting the literature.

FROM THE FLOOR: I sort of doubt it because there are many places where it will fall where the rainfall is not 8 inches.

What about Gold Field? There are places around Gold Field that I doubt have 8 inches.

DR. LINDBERG: It seems to hold pretty well for the southwest and the Mohave area, the Joshua Tree National Forest and these general areas we have been working have been very similar.

FROM THE FLOOR: It has a funny distribution and some of it might be microclimatic.

DR. LINDBERG: I think that it probably is because it very seldom occurs in these areas. You have a mountain slope going like so (indicating) almost invariably the Joshua will be right in here. It won't go out this far or out here.

FROM THE FLOOR: In some cases, it will occur on the rocky ridges and reaches an elevation there of only 7,000 feet and that is about as high as it gets any place. I do not think we know enough about it to use it too much.

DR. LINDBERG: I won't argue that point. I was actually quoting.

FROM THE FLOOR: Can you tell us more about the work on small animals, particularly with respect to your job of detecting and clearing radio isotopes, and also how did you go about establishing the effects?

DR. LINDBERG: Take Area One, one of our study areas, as an example. We have these big 2,000 transects. In one section here we put in a 500-foot grid, 500 feet on a slide, and we did very intensive trapping with traps every 100 feet.

FROM THE FLOOR: For a particular species?

DR. LINDBERG: No, animals as a whole. At the same time, there is a road that runs through this area. Right after fall-out because the contamination is there, sometimes it is too hot to stay for any amount of time, and it has been convenient to run along these roads and place a Sherman type trap. The general procedure is one person will drive slowly down the road. We will have trap bins on the car and we will place a double row of traps down here (indicating).

We have found generally in the area we get as good trap yields this way as going across country trying to pick up suitable trapping grounds. By comparing these trap yields along this road, in 1952 to this year, we find that the relative yield of animals which will run sometimes between 15 and 25 per cent, the figure stays pretty constant and the proportion of species that show up in the trap yield are relative as far as effect on the animals themselves, the maximum body burden that we can pick up. Even very shortly after detonation there is a fraction of a microcurie and laboratory work seems to indicate that this is way below anything we should expect in the way of a result.

These tissues are taken back to the laboratory. The animals are sacrificed, autopsied--lungs, liver, muscle, bone, kidney, etc. It depends somewhat on the size of the animal, and the thyroids are radio-assayed for contents. Hence, there are two things in the field we have been determining--percentage representation of the species in the over-all picture, and we have not gone after any sort of index of population response in any one specie. This desert has very extreme climatic conditions, as you are well aware. For instance, just to the east of the test site, about 30 miles, there is a valley called Desert Valley. If you go into that area right now, it looks like a museum setting. It is a perfect habitat for kangaroo rats, but there isn't a one there. It is a persistent winter. It stayed there several weeks longer. At least that is my determination. At least the area has never been contaminated, which could be attributed to radiation. It is very difficult to pick up a radiation effect by population and study.

Partly, also, we do not have enough manpower to maintain a population study that would be definitive.

FROM THE FLOOR: Do you have any data on the rate of this radio stontium--what it would be on a radioactive-free diet?

DR. LINDBERG: We do not have it, but a lot of work has been done. It is a fairly persistent material. It has a biological half-life of around 200 days. Half of the material, in other words, would tend to disappear within that period, and then a certain amount of this will also be flushed out, you might say, by diet, but you cannot be too specific on it because the amount that will be replaced apparently is dependent on the time that these therapeutic diets are administered after the administration of the stontium.

FROM THE FLOOR: I would think your ratio would have a time factor.

DR. LINDBERG: This assumes there is an equilibrium situation and you are dealing with chronic feedings. As soon as you change this for any reason so it is no longer a chronic feeding, then you will have a change in the over-all picture.

DR. DUNHAM: The next paper will be given by Dr. Lowman of the University of Washington. Dr. Lowman comes from the School of Applied Fisheries there which has, as you know, been very active in studying the effects of the atomic detonation in the atomic tests out in the Pacific proving grounds.

DR. LOWMAN: I should like to ask first if I am correct in my understanding that the strontium data for Eniwetok has not been released?

DR. KLAUS (?): Specifically no. If you can make your presentation without it, it would be better.

DR. LOWMAN: Since the summer of 1948, the members of our laboratory have made periodic observations on a colony of rats on one of the islands of Eniwetok. From March 1954 to March 1955, a continued observation was made on these groups of animals with more emphasis being made on pregnancy rate and things of that sort.

The island on which this colony is located is roughly triangular in shape. It is fairly large, about 250 acres in size, and is of an average elevation of about seven feet.

The period of the present study which is over a period of about six years has seen four nuclear detonations near the island. The distribution of the animals on the island is governed mainly by the plant community in which they live. These plant communities are grasslands, matted areas of brush and no trees are found on the island.

In the grassland, the main grass is Leptaspis, Chloris and the sage from ? Dristalus ? Sanburs ? Conchrus, and in between these grow isolated plants or groups of plants such as ? triumpheta and ? thota. The matted areas of plants are usually found on disturbed areas or in sand areas and especially in disturbed areas where paving has occurred. In these areas, the morning glory and the triumpheta, one of the vines which grows flat on the ground, covers the area to such a degree that you cannot walk through the area at all.

The third area contains a beach magnolia. In between these brush areas are located individual clumps of sage or bunch grass. Several man-made structures have altered the native conditions of the island--airplane landing strips, concrete floors for temporary buildings, trenches, roads, etc. All of the coconut trees were bulldozed off.

The rats, for the most part, live in the grassland areas and, to a lesser degree, in the brush area. These rats belong to a group which supposedly, originally, migrated from the Celebes, in from the Hawaiian Islands. This species tends to break up into insular or atoll groups. The fact that the group does break up is indicated by the fact that there are at least nine (9) synonyms for the animals.

The rats at Eniwetok are herbivorous, composing a small part of the diet. The main part is made up of grass seeds, bunch grasses, sand-bur and the succulent leaves of the bushes such as triumpheta and ceda. The fact that these plants are found in abundance in the grassland area indicates to me at least that this is the principal reason that the rats are found in this area. The same plants are found to a lesser degree in the bush areas and the rats are found in a corresponding lesser degree in the same areas.

As with most rodents, these animals are nocturnal although during the day there is some evidence that the rats are above ground. In fact, there is quite a bit. If you survey an area of about 100 square feet, stand and count the number of rats you see in that area during the daytime, it will vary from 15 to 60 per hour, and since 100 foot square only covers about a sixty-fifth of the area occupied by the colony, the total number that would be seen, assuming an even distribution, would be about 1,000 to 4,000 per hour above ground during the daytime. Judging from the evidence of feeding, there are many more rats on the surface at night, although we have not made direct observations on this.

Since 1,000 to 4,000 would be above the surface of the ground during the daytime, I would hazard a guess that there would be one times or more in the total colony.

The openings of the burrows are usually open under clumps of bunch grass or sage. Occasionally they are found under boards, rocks, slabs of concrete or rubble. In all cases, well-marked runs extend between the openings to the burrows.

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In the case of detscrups for bunch grass, the runs will actually burrow through the bunch grass. The burrows are usually shallow, 6 to 12 inches in depth. Although they may go to 24 inches and the nests are usually f rom 18 inches to 23 inches in depth. They are round, about 8 inches in diameter with cut grass stems laid in the bottom of the nest. The number of openings over a single nest is usually two to five in number.

No observations have been made in this study on the number of litters per year per female since it would involve keeping the females in captivity, nor on the length of gestation period of these rats.

In other species as well as closely allied species, the gestation period is approximately 21 days. Since we know the instance of pregnancy in the females is around 18.5 per cent, there should be on the average 8 litters per year per female. Breeding is continuous throughout the year although there is a greater tendency for an increase in number of females pregnant during the summertime. Contrary to the conditions for more rodents in populations studied for ecological reasons, this has no ectoparasites--has only two endoparasites seen thus far, and these are tapeworms and hookworms.

The isolation of this colony from rats of other islands as far as we can tell is complete, which makes it another nice population of animals for study of this type. The closest island to the south of this island is about 1300 feet, which should not been too much trouble for a rat, except for the fact that since it is on the eastern edge of the atoll, there is a continuous flow of water over the reef. It varies from one to two-and-a-half per second and from six-and-a-half to ten feet. However, I do not think the rats could swim in that current. We have never seen a rat during six years of work in the area in the water. We just never see them in the water.

The closest island to the north is about 6,000 feet, but since there aren't any rats on the island there, there is no danger of immigration from there.

There is a method by which the rats from other islands could get into the island, and this is by boat travel. Boats periodically dock or land at the island, and it is conceivable that rats could come in on the boats. However, since the rat is ratus excellence, a field rat, we have never seen one on any of the boats. I do not think that the field rat is getting into the island in that manner. However, we do have evidence that the old world rats come in on the ships with the supplies and equipment from the States, and there would be a chance that these animals could be transported to the island by boat. However, since the old world rats are different in appearance, and the fact that we have not seen an old world rat on the island, indicates to me that if it has occurred, it has only occurred with one section at a time, so that the reproduction was not possible.

In general, the study of the effects of altering the environmental conditions with the test program can be divided into three parts. This is the effect of preliminary construction associated with the weapons test and the effects of the weapons themselves and the effects of the preliminary construction in modifying the effects of the weapons.

The construction phases that affect the plant population of the island are mainly those of the grading of large areas. Where large areas have been graded, a lush? area of grass occurs except in the instances where there has been paving. In that type, matted growing takes place. The building of trenches and dikes and bounds and mounds throughout the island also has an effect upon the plant communities. Where they are built on the edges of grassland, the grassland type of environment takes over in the trenches and in the mounds. Where they are built on the edge of the mat area, the mat takes over on the mound. The man-made structures, including trenches, dikes, bunkers--that reason that this has an effect, this rat usually builds its next in the flat grassland area and burrows into the ground at a maximum of 24 inches.

In the case of mounds, in those mounds where grassland environment has taken over, the rats burrow horizontally into the bank or into the mound, and the burrows tend to be much deeper in this case. They will go as far as four feet in depth. I think the main reason for this is that the soil of the mounds is much softer than the soil of the grassland since it has been loosened up so they can burrow into it more easily.

Another effect may be in the grasslands. The openings to the burrows always open under a bush or under a clump of grass; whereas the opening to the burrows in the sise of the banks are usually uncovered.

The provision of artificial nesting in burrow areas is another effect of preliminary construction. At first sight, it appears to be a light effect, although in the survival of the population it probably is a major effect. The artificial nesting areas are provided for the most part by cable tunnels which enter into the instrument bunkers. These tunnels are approximately eight inches in diameter. I do not know the full length of them, but it is considerable. They are under the ground from a depth of three feet to eight feet. The rats appear to be abundant in these structures when you are in the bunkers, because you can see them run from one tunnel to another. You can see as high as 15 or 20 rats in one bunker. However, the total number of rats is fairly limitdd with relation to the size of the entire colony.

This living of the rats in the cable tunnels might appear at first sight that they are acting more like house rats, but actually this is not the case at all because the tunnels tend to simulate the burrowing characteristics associated with the rats; and, secondly, there are no humans living in the bunkers. In fact, humans are very seldom around the bunkers, so rats are not following this type of behavior.

Some of the construction projects tend to isolate groups off from the main colony. These include many roads, trenches, concrete platforms and floors, larged paved areas and the airplane landing strip. The degree that these different structures isolate various sub-groups of the colony, of course, is dependent upon the size of the area and the shape of the area.

I think another great factor in the ability of these structures to isolate these small groups is the fact that these rats do not have a tendency to migrate as long as the food supply is constant. We do not have any direct evidence on these rats, but in two other races of the same species they found that within the first three weeks the rats did not migrate further than 60 feet; and over a ten-week period they did not migrate over a distance of 200 feet. This was in a so-called even distribution of plants. There weren't obstructing devices in the way.

The forces following the detonation of the devices that affected the rats may be divided into five groups--that of thermal radiation, initial nuclear radiation, shock, water, and the immediate high levels of residual radiation; these had the greatest effects upon the colony.

Individuals directly exposed to the nuclear radiation of some of these devices did not survive. Many of them who were exposed suffered second- and third-degree burns from the thermal radiation. In many instances, the burns were to the bones.

The residual nuclear radiations were very high. Animals above the ground would have received accepted lethal doses during the first hours, and those below would receive sub-lethal levels. Over a period of weeks, practically all of them above ground and practically all of the rats below ground received lethal doses. The rats in most of these cases were probably subjected to greater radiation than that indicated from the gamma radiation that I just spoke of, and this was due to the washington of radioactive material into the bunkers by the torrential rains that followed.

The initial nuclear radiation as contrasted to the residual radiation is that of gamma radiation put out by the detonation, we figure, within the first minute, although this is not actually the case but it is a convenient method of separating the residual from the immediate, and then the other immediate nuclear radiation is that of neutrons. Some detonations in addition to giving lethal doses of residual radiation also deliver lethal doses of the immediate gamma radiation. Those individuals above ground at the time of detonation, in some instances, had little or no chance of survival since, to survive, they would have had to have gotten 18 inches to two feet under ground in sight of a second of detonation.

The neutron radiation in the center of the colony, in some instances, was above the accepted lethal doses. Within the burrows, it would have been attenuated only by a factor of one-tenth to one-half, so it is unlikely that any of the rats in the burrows in these instances survived.

As far as can be determined from our post-shot observations, no effects resulted from the shock-wave. On each of the four series of tests, the rats were subjected to long-term residual radiation and this, in the long-run, will probably have the greatest effect of any of the forces released by the weapons.

The paths by which these materials may go into the body are that of inhalation, ingestion, contamination in or on the food or on the fur and through open wounds. The latter method of contamination is probably a very minor one compared to the others.

I-131 appears to have been the major isotope taken in by inhalation. The day after the detonation this was apparent. The I-131 that was concentrated in the thyroid was concentrated in amounts that resulted in excessive exposure to this origin. The radioactivity in the food levels was almost comparable over a period of a year with that level found for total body burden. The concentration of isotopes occurred in some plants. Some of them were dangerous. Some were and some were not from the health standpoint.

The specific radioactivity with respect to each organ changed with respect to increasing time. The first two weeks, the highest activity per gram was in the thyroid; in nine weeks, the activity was so low that it could not be measured with our instruments.

The day after the detonation, the specific activity of the skin was just below that of the thyroid or that of the gut and bone, yet lower, and the activity went lower, into the lungs, kidneys, muscles and liver.

The interesting thing about the activity on the skin is that very shortly after detonation, during the time when the rats were still sick from radiation sickness, the surface activity on the skin was very high. As soon as the rats recovered enough that they could get around and take care of themselves, they immediately cleaned off the contaminated areas. We noticed that particularly about these rodents, that the skin was very clean, with no dirt or residual material. They got this from going through the burrows which left it on the skin. The rats appeared to clean themselves continually.

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Those isotopes found in the skin include rare ores, about 50 per cent, with traces of niconium and niobium and other isotopes.

From the observations we made in the field, the population has been exposed repeatedly to amounts of initial radiation residual radiation and thermal burns thatshould have decimated the population each time. In addition, it has been exposed to levels of radiation which should have caused the marked genetic effects, and I fully expect to find genetic effects and reduction in the over-all pliability of the rats as a whole, but I didn't find them.

The pregnancy rate or frequency is about 13.5 per cent. In two other species, it runs 18 per cent, and in the other instance, 20 per cent. The average number of embryos per litter was four, in this case; in two other closely related races, the average number was 4.2, and I believe 3.7 and 3.8. The sex ratio does not vary from a 50-50 ratio, and except for those individuals taken shortly after detonation, all individuals examined by our people have appeared to be in good physical condition, and we judged the physical condition by the condition of the fur and eyes, length-weight ratio, fat deposition and the activity of the animal.

In addition to this, the main colony has, as of now, actually been continually expanding. They are beginning to take over grass areas in which they were not found before. I think this effect is due to the population pressure in the present colony area with a reduced amount of food; although a goodly amount of fat was found in the rats, there was no instance where we found evidence of degeneration. All embryos examined appeared normal. It boils down to the fact that you cannot find any effect in these rats from repeated exposure to these lethal doses.

There are two reasons that this colony survived under these conditions. One is that they are small colonies off from the main colony toward the periphery of the island. So, in case of neutron deaths, some of the rats in the burrows probably escape neutron death. Another factor, and I think the most important factor, is the tendency for a small percentage of the population to nest in the bunkers and in the cable tunnels. In fact, I am convinced that in almost every instance, the only survivors of the population were those who were in the bunkers or in the cable tunnels.

DR. DUNHAM: Dr. Lowman's paper is now open for discussion.

DR. ODUM: We talked yesterday about it. It might be good to comment on the thing that you have. Here is a favorable ecological situation counteracting or balancing with a very unfavorable environment due to radiation. In other words, you have increased habitat in the island due to radioactivity--increased grass areas--so even if you did have just one or two left, you had a very favorable ecological situation. The population has actually benefitted in that respect; therefore, the increase is not to be expected unless you had a nucleus of individuals, so there again you have the problems which we always have to distinguish from ecological change and a change due to radiation. You have a new decision like a new fish pond where you have a favorable situation with regard to space.

DR. LOWMAN: The thing that surprised me about this group of rats was that if you calculate the total dose received over the entire period of study, you would expect the mutation rate to have risen appreciably even in these rats that survived, and we would expect that to show up first in lethal mutations.

FROM THE FLOOR: There were, however, periods when they were probably in very shaky condition.

DR. LOWMAN: There is one disadvantage to using this population for an ecological study. It is not a stable population and we realize that. The population is fluctuating all the time.

At the present time, it is growing. If they have another test, it will be reduced again.

DR. BILLINGS: Is this grassland successional on the Island?

DR. LOWMAN: The longest time I have been there myself has been three months, but I have been there over a series of years. As near as I can tell, the bush area is the final area before the trees and eventually the trees would come in.

DR. BILLINGS: Are the rats found in those bush areas or tree areas, but not in great numbers?

DR. LOWMAN: That is true, and they are seldom in the tree area.

DR. BILLINGS. In other words, their succession is along with the grass and as long as their natural food supply is being increased on the Island due to disturbance, they are going to have favorable environment to increase there as long as there is a nucleus, as Dr. Odum pointed out. There are no native mammals in the bush.

DR. LOWMAN: No. The rats are the only mammals on the Island.

I am worried for fear that the old war rats will come in. If they do, we will have trouble keeping the population going.

DR. DUNHAM: If there are no further questions, perhaps we might ask Dr. Boss, who is in the back of the room, who established an experimental laboratory, to tell us a little bit about what is over there.

DR. BOSS: This is a typical AEC method of doing things in a hurry. It caught me by surprise. We have a small laboratory on Parry Island in the Eniwetok Atoll. Gene Odum can probably tell you more about it than I, and Frank.

It is a modest laboratory. However, we have it well equipped, I think. Two of the rooms are air conditioned with plenty of equipment in it for accounting purposes. The laboratory was put in there for probably three reasons. The first reason, of course, was to help the people who were on the program that you have been hearing about and the applied fisheries laboratory use it when they are there. The second reason is to have our contractors, like the universities, Dr. Odum, and so forth, go out there and work during the summer if they can. The third reason is to afford an opportunity for scientists in small colleges to avail themselves of this rather unique environment.

We have literature on it if you are interested in it. It takes about four months for us to get all of your clearances and paper work taken care of before you go. We fly you out there and the logistical part of it is very good. The food is excellent. There are fine places to live out there for about a dollar and a half a day. Write to the Division of Biology and Medicine and express a desire to go out there and indicate what you would like to do. We have set tentatively a two-month period as a minimum. It involves quite a bit of paper work, clearance procedures, and so forth, to get you to go out there. We do not charge anything for you to work at the laboratory. It is absolutely free.

At the present time, Dr. Hiatt, of the University of Hawaii, manages the laboratory for us. If you are interested in it, write to us and we will send you details on it.

(A short recess was taken.)

DR. DUNHAM: Will the meeting please come to order.

The next paper will be given by Dr. Edward Held of the University of Washington, Laboratory of Applied Fisheries.

DR. HELD: I am not going to be able to discuss any of the specific levels of activity, some of the specific isotopes that have been involved in some of our work at Eniwetok in the past year, but what I want to try to do is give you the broad picture of what we are doing, the areas of study that we are hitting and indicate some of the problems that can be profitably studied in this area, and also some of the problems that can be studied, making use of the fact that the area is contaminated with radioactive material--in other words, using these radioactive materials as a tool in placing the movement of materials in the lagoon and through various live cycles.

Our principal objectives have been to determine the degree to which particular plants and animals concentrate the radioactive materials or to which they are contaminated and the nature of such contamination; that is, whether it is simply a surface contamination, or whether it is material that has actually been metabolized and which is contained in the tissues.

Associated with these objectives, we also would like to determine the immediate source or the sources of the radioactivity to the various organisms; that is, are they getting the material from what they eat, or are they getting it directly from the water.

As far as the fish are concerned and most of the invertebrates, the matter of getting it from the water is probably of importance only during the very short period after the initial contamination.

Another objective is to determine what the redistribution of the radioactivity is as time goes by in the lagoon.

Then, finally, some limited studies on repopulation, reinvasion of areas that have been decimated.

I said we concentrated most of our work at Eniwetok Atoll, and we further concentrated our effort there in a section of reef near the north end of the atoll which is roughly three miles long and one mile wide. There are four islands in this section of reef with a total area of about 80 square miles.

In this broad area where we have made collections and observations, we further limited ourselves for the principal sampling program to radioactivity determinations to one island and the immediately surrounding reef. This island has an area of about 15 acres, and the width of the reef in this entire area runs between one and one and a quarter miles.

Unfortunately, I cannot show you the photographs, but I would like to diagram briefly on the blackboard the broad zones that we find in this area. It has been fortunate for us in that this is a good area as far as proximity to the test site, and also here we have represented most of the major life zones that are found in the Atoll. This is not true for any given section of reef in the Atoll.

First of all, we will start at the sea and at the reef margin on the seaward side where we have the grooved area. I believe Dr. Odum, this afternoon, will show a photograph. At this lip, we have the so-called Lithothambian Ridge. The Ridge is not too prominent in this area, but it definitely is distinguishable as a very slight rise. The Lithothambian Ridge is made up of coral algae, coral lithium principally, and it is generally considered as one of the areas of most rapid growth and cementation. Just inside this ridge is a particularly flat area. It runs about 200 feet in width and it is a pavement, a solid pavement. It is honeycombed underneath and not here (indicating) but in a reef not far away some of the honeycombed areas break through so that you have mole holes, and it is here that you find only those forms which are capable of anchoring themselves very thoroughly against the battering of the waves. You have encrusting forms of coral and you have small boring clams and some of the sea urchins which hang on by using every little available crevice.

In some areas, an area the size of this table, we have counted as many as 80 sea urchins where there was a sufficient number of fish for them to hang on, so it goes along here (indicating) with the relativity quite high.

As we move along here (indicating) which is about 30 feet wide, we run into a boulder area. This boulder area is raised somewhat over the general reef surface so that it is exposed at very low tide. We have not made any studies in this area. It is way out on the reef in the Island, and it is very difficult to get to, and it is not a particularly rich area from our point of view.

Just inside, we have what has been called a coral algae zone. This area even at low tide, which is covered with 2 to 4 feet of water--you have a rich growth of coral and large numbers of fish.

I neglected to mention that even out in this area, the tide is not too low. You have large schools of fish going over that flat, even sometimes when the tide is so low the fins of the fish are exposed.

Just inside this coral algal, it is not a sharp grade, but it grades down into what has been called a boat passage, inter-reef depression--various terms like that--and we call it a boat passage probably because we use it as such withour rubber boat. This can be thought of as simply a more luxuriant growth of coral than this coral algal zone. Even at the lowest tide, there will be places that run eight feet deep. In some areas, you get hugh coral hedge 25 feet in diameter. You have large numbers of fish here, and on occasion we have seen large numbers of frii hanging in this area.

To save space here, I am going to draw in an island. Relatively speaking, this would be a much greater distance, say, the distance from the Island to this point (indicating) would be around 1,000 to 1500 feet, and the total distance here (indicating) as I mentioned before, one to one and a quarter miles.

In choosing our sites for collections, it is not practical, especially when you have to get into areas where you cannot stay very long, to attempt to get out to all of these various zones, so we have done the next best thing--we have chosen for our collecting area or fish and vertebrates a place here off the Island where there is a deepening and a water current that corresponds fairly closely to the conditions that you find in the boat passage.

On the opposite side of the Island, you have a reef flat. Sometimes you will hear this coral algal zone referred to as a reef flat, but there is quite a difference. The reef flat, when you first go over it, gives you the appearance of being completely barren except for a few flat top heads and a little algae that is growing on the surface. Dr. Odum will mention more about that this afternoon.

This flat on the lagoon side drops off from a depth of six inches or so at low tide, gradually deepening to 2 to 3 feet in depth for a short distance just before the margin, and then drops off roughly 8 to 20 feet; around the Islands, you have areas of lithified beach rock. They might be on shore and sometimes they are off some distance, indicating the old shoreline of the Island.

The observations and the collections we have made have been carried out over a period of a year. Observations have been done on a monthly basis, and in order to get the general picture of change here, such as the shifting sand which occurs continually at the ends of these islands, the building up of sand bars in places, the building up here (indicating), we have used both direct observations, swimming with face masks, using our rubber boat where possible, and some work has been done with the aqualung.

In addition to that, we have taken a series of aerial photographs, making mosaics of these aerial photographs.

With respect to sampling for radioactivity, we did some sampling before the test period, of course, and then immediately after the test we did on a daily basis--we didn't have much time to spend the first few days on any one bay, so we occasionally missed some species that we were after but, by and large, we can say that the sampling for the first 30 days was on a daily basis, and then at progressively longer intervals until the last six months we were sampling on a monthly basis.

I will just briefly say a little bit about the general picture of the distribution in a radioactive form. On a weight-to-weight basis, the radioactivity was found in the algae and then in the invertebrates and then in the fish. Obviously, to make such a broad grouping, there is going to be a great deal of overlapping. The overlapping that occurs appears to be due mainly to the feeding habit of the order. In the fish, for example, the most radioactive fish that we find are the herbivors and then next the omnivors and, finally, the carnivors. I think you will see this afternoon and tomorrow that this ties in very nicely with work that has been done on the Columbia River and in other areas.

In the tissues in the fish, we find in the gut the content was highest, naturally, then the liver, then the bone or the skin. The position there varies considerably, with the skin including the scales, and then finally in the muscle.

Again, generalizing roughly, we have the same general position in the invertebrate, the digestive organs being the most radioactive.

I said the algae were the most radioactive. They also take up this activity very soon after a test, after there is contamination of an area. There is really what you might call a brother effect in this environment. This is not necessarily primarily due to the larger forms of algae that we see. In fact, perhaps more important are the smaller phylamentis and the algodymaceous and the bacterial sign that you find even upon these exposed beach rocks that are wet during high tide periods and in the sands there is this material all through the surface area of the sand. Coincidentally with this, we find that snails, for example, such as Nureta, this small white snail, feeds on these films on the beach rock and it quite consistently has the highest level of radioactivity. While our data still has a long way to go in analysis, the last group came in from the field just last month. We have high hopes and very good indications now that we are going to be able to tie together some of the animals with certain of the algae that they are feeding upon. We are certainl going to get the general picture of which are clearly herbivors, which are the omnivors feeding principally on algae, and which of the omnivors are feeding principally on animal material. We have not been able to see any visible effect that could be definitely attributable to radiation. It is perhaps not too surprising in marine environment where there is such strong competition at all times, but on some of the land plants, we might have expected to see some changes. Dr. Palumbo in our group has been studying these plants and has been studying the re-invasion of the islands. I think I can summarize that work very briefly by saying that the re-invasion of the islands which have been decimated due to nuclear device tests has in no way been any different from the re-invasion of islands that have been disturbed due to other activities in the air. As a matter of fact, bulldozing has a most serious effect because bulldozing pulls up the roots of everything. Whereas, with the blast and thermal effects, while it knocks off the tops of the plants, nevertheless, it leaves stumps and the roots remain behind. We find that within a few days after the shot that stumps of messerschmitia and magnolia begin to show aventaceous buds on the stumps.

Within six months time the appearance of our principal study island was actually much less than it was immediately before the shot. This was due to the fact that we had some particularly heavy rains and we were in the fairly long rainy period.

I should mention briefly that the Northern Marshalls differ considerably from the more southerly areas as far as the rainfall is concerned. We do not have a picture of a continual rate of rainfall throughout the year that runs about 10 inches a month in the southern part. Here you get that 10 inches per month--well, most of the time, except for the months of November, December and January. Even this is quite variable from year to year. Rainfall in the study of the plants is something that you have to keep in mind at all times in this area.

The fish population here, as far as we are able to determine, was not reduced or altered. These observations are based on mainly counting the fish, visual observation, sitting under water with an aqualung and counting how many fish go by. It is impractical here, particularly with certain transportation facilities, at the north end of the atoll to be able to do tagging experiments and netting, and so forth, and especially since we have these storms that come up periodically. What trapping we have attempted to do has always been spoiled by storms.

However, there is an area where the fish population was knocked out. Also, in this same area, all of the coral, clams, the entire reef, this particular small area was covered by sedimentation, and here Dr. Weilander has been making these periodic collections and measuring the fish, and he has found that the early populations were made up of very young individuals and now it is gradually increasing in size, but the population makeup is quite different from that of the main study island which includes this coral algal zone.

The population makeup in this area is principally that of bottom feeders and forms such as the goat fish which pick out the crustacea and other small forms in the sand.

As far as the corals are concerned, there was a shot previous to the one we have detailed previous to the sedimentation in the area. There has been an argument about how much sedimentation has to do with the killing of coral. Let us say there was a lot of sedimentation in the area and corals have been killed and also by overturning. Therefore, throughout this area, you find dead coral heads, and on them you will find these encrusting colonies, some of which judging from their size, must obviously--well, not necessarily--but appear to have re-grown from remaining portions of the old colony. There are some cases where this is true. There is always the possibility, of course, of the coalescing.

Then, we have also many evidences of the formation of new heads of achripora, coccalabra, and so on. We have marked a few of these coral heads and, more important in marking in this area to have your areas located from a central point by transsect but you cannot depend on buoys or anything of that sort staying in place. We hope that we will eventually get some information on growth rate.

So far, the indications with the coccalabra in this area indicates that growth runs somewhere between 40 to 50 millimeters per year.

Also, in these coral heads, you find dead clam shells, this cridacnicreses, this poor clam, and here again we find small individuals, largely shells of just the empty, dead shells, and we find many small individuals--two, three, four centimeters long--and we are following these in these marked coral heads, too.

I see I have run out of time. I would like to say that in addition to our studies just here at the north end, we also have eight stations distributed around the atoll where we make monthly collections of a few selected forms to get some picture of the general distribution of the activity and Dr. Seemower in our group, in cooperation with the New York operations office, has recently returned from a tour of the Pacific, and I think possibly this will tie in a little bit with this work.

DR. DUNHAM: Thank you very much, Dr. Held. Before his paper is open for discussion, I am going to ask Dr. John Harley of our New York Office, who is responsible for almost all of the radio-chemistry related to these things, to say a little something about the radioactivity levels related to this area.

DR. HARLEY: As you all know, following the opening of the Pacific test series, last year, one of the Japanese fishing boats was in the area and a considerable amount of radioactivity was distributed over them, and their catch, and so on, and the

Japanese have a very considerable interest in fishing, particularly the tuna fish industry. From about the middle of May until about the middle of June, they had a ship out in the area, Tsikatsumara, taking in essentially Bikini and Eniwetok, then it came down and made some traverses across these areas (indicating). I believe they came down here and went back to Japan, the distances varying from 150 to 1350 kilometers. The activity of the sea water was determined and plotted roughly around the first of June. Naturally, they found at that time their highest levels in closer to the test islands and lower levels at the other traverses. They measured sea water not only at the surface, but in depth. Their findings are a little hard to interpret because at that time they were not standardizing counters, and they were reporting everything in terms of counts without being able to relate it very well to the findings that other people would have. However, using some estimates of what their equipment was like, we felt that there was a possibility that even as late as a year after the test that there would be measurable activity in the Pacific Ocean. Therefore, with the cooperation of the Applied Fisheries Laboratory and Scripps Institution of Oceanography, we planned a survey of the Pacific Ocean, and, in some ways, we nearly covered it.

We started from San Francisco, went to Honolulu, and from there down to Kwajlein. We did not start taking any particular measurements until we were well on the way to Kwajlein. Our system was to run hydrographic stations every 180 miles approximately where we dropped 12 Hanson bottles to a maximum depth of 600 meters. In between, we used a shallow test with four bottles. Every hour on the half hour, in between, we took a surface sample of water. These samples were measured aboard ship by a rather simple calcium c(?)ber. This was not advanced radio technique, but we could perform this as we went along. Our route was predicated somewhat by our findings in connection with the cart streams, and so on. Approximately, we continued on down Kwajlein for about four degrees and came up by truck past the Marianas down to Mindanou, the fairly southern end of Mindanou, and over to New Guinea up along the Philippine Coast into Okinawa, and from Okinawa into the China Sea out just below Kyusu and back into Japan.

On the return trip from Tokyo to San Francisco, we could not follow this same program, but we did take surface samples every four hours on the return journey. Rather remarkably, we found the levels of activity about in the range which we expected. I would say that an average activity might be in the neighborhood, speaking of the surface now, of 40 to 50 disintegrations per minute per liter of water. It is of interest to note that this compares with 600 disintegrations per minute due to potassium--I mean the normal potassium in sea water--so the increase in level is not very remarkable, but it is definite.

You see, you have your north equatorial current coming in, coming upon, of course, the Kyusu current which returns the Japanese current to the States and the equatorial counter current coming back in this direction (indicating). Within the main current streams, your profile would show an increase but nothing terrifically marked--perhaps a factor of 3 or 4 at the tails of some of these traverses where we were down to values of the order of 10 or 15 disintegrations per minute per liter and maximum values perhaps of 75 or 80. I mean the increase within the current streams was not very great, but it was marked.

We planked them just as had been found in other areas--had about the same activity as you would find in a liter of sea water.

We do not have our work on fish by any means. That is just getting started out there and this proved to be a poor fishing expedition. This was not a fishing vessel. This was a Coast Guard cutter. We did make some stops at atolls and collected samples, largely they are being worked over by the Applied Fisheries Laboratory.

There is no evidence for any particular heavy concentrations. It is possible, of course, that we may have missed something in this sort of thing. You cannot cover every square mile of the Pacific Ocean, but apparently there is a very widespread contamination.

On the return trip, we were well past the Hawaiian Islands before we began to feel that the activity was really down to zero on the average. In other words, there is this widespread activity which is apparently moving in the current streams. Some of it at least has made the turn into the Japanese current, but it has not yet reached the United States.

DR. DUNHAM: Thank you very much. These two presentations are now open for discussion.

DR. LINDBERG: Was there any indication of the depth?

DR. HARLEY: I am somewhat of a spade about that. With this equipment that we had, or almost any accounting equipment, our accountengers were very high. Just our standard deviation to counting alone may have been as high as 20DPM. I am speaking in terms of large numbers of these (indicating). If you take a long profile and average that out, you realize that that does not do much and you will get a fairly nice falloff with a logogriphic activity, and you get sort of a tail, and this is down at a very low activity where you would get one of these averages which is quite definite.

In other words, I believe there is some activity between the thermocline and below the ordinary mixed layer. It is rather low and only twice did we make measurements much below 600 meters. Below 600 meters down--and there are several cases of several bottles--the activity was negligible, but 600 meters was definitely below the thermocline.

FROM THE FLOOR: I don't know the Pacific very well. It wouldn't be below the permanent thermocline.

DR. HARLEY: We are trying to get some oceanographic information on this from Scripps.

DR. ODUM: This is a good way to check the known knowledge on your currents.

DR. HARLEY: It would be much better as far as checking currents would go if the activity were perhaps a hundred times as high. I do not think we would like that.

As I say, individual measurements are not too reliable.

DR. ODUM: Did you try measuring Cecille organisms? Did you pick up any corals?

DR. HARLEY: Yes, and you will naturally find coral everywhere will be picking up traces of activity.

DR. LOWMAN: Didn't you find higher temperatures than in the ocean?

DR. HARLEY: I would say the reactions do not follow the books too well. We ran into a number of things that looked unusual as far as temperatures are concerned.

FROM THE FLOOR: What sort of temperatures did you find?

DR. HARLEY: There is a very large number watching just the boiler or condenser intake water. Many times it was running around 70 degrees, perhaps up to 75 degrees.

FROM THE FLOOR: If you take the average you have given here, and if I followed the traveler's advice and take the average depth of the ocean and take what has been released, don't you get way too much activity here? Have you done the back calculations?

DR. HARLEY: It depends on what you consider this tail to be--I mean whether it is a real effect of some type of background which we are not considering, and so on.

In other words, if you consider only this steep slope, it is not unreasonable.

DR. DUNHAM: Are there any other questions?

Thank you very much, both of you.

The next paper will be given by Dr. Dunning.

DR. DUNNING: I will yield to Dr. Dudley.

DR. DUNHAM: This obviously is a combined effort of some sort. They are going to discuss the distribution in the biosphere of radioactive materials from the weapons tests.

DR. DUDLEY: Like any other speakers this morning, I haven't done anything that I am talking about here. I just looked over other peoples' shoulders.

We are talking on the subject of the observation of bomb debris in the environment from the weapons test at considerable distances from the test site in order to develop close-in problems. I mean the orders of thousands of miles, in particular I am going to emphasize the eastern half of the United States where we have quite a bit of data and which is in the distance ranges with which I am concerned.

I am going to speak about the observations on current contamination and some of the information which we can obtain from those observations about the mechanism of contamination, and then Dr. Dunning is going to supply some of the context here, the interpretation both in terms of the natural background radiation levels which are found and also the levels of radiation which are generally considered to have some biological hazard implication.

There are many varieties of radiation hazard including the external radiation both beta and gamma, and the possibilities of uptake of individual isotopes, incorporation into the body and radiation from within. There is obviously not time in fifteen minutes to go into all of those in detail, so I am going to select two or three which we consider the most important and I will spend my time on those.

Observations have been made by our New York Operations Office on the amounts of radioactive fallout in the eastern part of the United States and elsewhere from which it is possible to determine roughly the external gamma and beta radiation which are found in environments as a function of time during the last few years.

The dose levels have, of course, fluctuated with the weapons test, and I am not going to try to go into detail on that. They have put out an article in Science a few weeks ago, and you can get more details there.

Generally speaking, to nail down a few numbers, one can calculate that the order of magnitude of the beta radiation dose produced in the soil, if you assume that the radioactivity, the beta radiation, affected primarily the top centimeter of the top soil surface as it would before it sank in much with reaching rainfall, it works out that the total dose in this one centimeter of soil in the United States is of the order of magnitude of one rad. This is from all weapons tests. This is just an order of magnitude. It depends on the soil rate of leaching, and so on, and one can get more data from the published data.

The external gamma radiation which is not confined nearly so close to the radioactive isotopes because it is more penetrating has been at the level of three feet above the ground; that is radiation of human beings of the order of magnitude of the total of 0.1 rad, total dose to date.

FROM THE FLOOR: What is the square area?

DR. DUDLEY: The rad is a unit of dose which is independent of area--energy dissipation of per gram per matter. It is throughout the centimeter an average of about one rad total dose.

Dr. Dunning will speak considerably more on the matter of natural radiation and hazard levels, so you can get a better interpretation of what these numbers mean from him. I would like to emphasize rad, and that is all I am going to say about external gamma radiation. I am going to talk about the radioactive strontiums which are more hazardous than the isotopes. It is possible to detect the radioactive strontium with very sensitive techniques and many measurements have been made in the United States on the amount of strontium now found. Since this was just declassified this morning, I cannot resist giving you a few numbers here. I do not want to confuse you, however, with numbers. I will try to summarize it all in a sentence at the end so that you may still get something out of them.

I am taking a particularly set of data which we have that has a bearing on the amounts of activity found in environment and successive steps along the food chain showing how the activity is propagated. I will write these numbers down on the blackboard here. Observed strontium, 90. I should say this is for samples taken in the Chicago area in September of 1953. I will say a bit more about the dependence on time.

With respect to rain, the average value--disintegrations per minute per gallon--there being 9 gallons, the minimum 2 and the maximum value 43 DPM per gallon. (Chalk demonstration) Tap water at the same time in Chicago, a value of 0.4--just one single measure.

Soil in that area, 353 disintegrations per minute per square foot, there being 12 samples ranging from 100 to 588 disintegrations per minute per square.

Another way of expressing it is in terms of the ratio of strontium activity to available calcium in the soil. As you have heard, there is a correlation, and I will work with that in the subsequent part of the chain. If one divides this activity by the amount of the available calcium in the soil, it works out at

5.4 units, which I will define in a minute--2.1 to 11.7 S.u., which, for historical reasons, we call the "sunshine" unit. One strontium unit for present purposes is strontium-90, the calcium ratio being equal to one-one thousandth of the level which is considered maximum permissible concentration in man, and it works out to 2.2 DPM per gram of calcium. This, for reference purposes, I think, is your best figure--one-one thousandth maximum.

Vegetation growing on this soil--average value of 8.5--12 samples ranging from 2.3 to 20.9.

Calf bones in the area, just one sample--it turned out more or less representative of what you would expect to find as an average value--perhaps a bit low--1.2.

Milk--1.4 Sunshine units.

There are a couple other samples thrown in--14.0.6 to 2.2.

Finally, we had some human bones in Chicago--an average value of 0.13, with 113 skeletons ranging from 0.04 to 0.3.

These happen to be stillborn skeletons which were a current source of human material. We have reason to believe that a growing child, having a somewhat different calcium intake, would be somewhat higher.

From other data, one might estimate a growing child would be about 1.5 growing Sunshine units there.

I wanted to go into the distribution of activity along the chain here in detail, but I am not going to have much time.

There are laboratory experiments from which one can compare the discrimination between calcium-strontium along the chain against the values that are observed in the field. The correlation between the laboratories and the field experiments is quite good except at this step, and this level down here (indicating). At this step, the laboratory experiments show in general the strontium units or the Sunshine units and vegetation would be one-half or one-third. This is somewhat proximate but a good average value.

Here (indicating) the vegetation is higher than the soil. The reason for this, we think--and we are getting more information on this now--is part of the activity which was on the vegetation had not come from the soil and roots, but rather from direct retention on the leaves--not getting into the soil--so the correlation between the soils is a spurious one.

On the human bones, the activity is low because the fetuses have partially filtered through the mother, whereas with a growing child it would be lower. As Dr. Lindberg mentioned, a strontium-calcium ratio relative to food--he said .2. I have worked with .3, but it is in the general range.

There is one further step (chalk demonstration) made on stable strontium to ratio in bone.

(Illustrating) This is an equilibrium condition which has been in existence since the beginning of time--1.6 units, which I will not define for you now, 0.4 to 3.5, 50 measurements on soil, 1.6 units of calcium-strontium ratio.

Down here in the skeleton, 0.1 units--42 skeletons, 0.01 to 0.3. Most of these samples were taken from east of the United States. The ratio of discrimination here is by a factor of 15 soil to bone. If you work out over here, you get sort of a spurious correlation because the relationship between the vegetation and the soil--the bones are low but the intermediate steps work out so as to give you approximately the same factor of discrimination.

We have observed since 1951 a continuous check on milk by our New York Operations Office, and there has been essentially no change in this value. It has been running roughly one to one and a half Sunshine units right up to the present time.

Soil activity would be somewhat higher now, probably not as much as a factor of 2.

I will say one word now about the situation with radioactive iodine. We are just this test series--TEAPOT--making measurements on the radioactive iodine contamination again in a semi-integrated set of samples, including vegetation, cow milk, cow thyroids and human thyroids. While they have not come from the same area, the figures have shown that the correlation is rather uniform, which is not too bad.

Observed value--vegetation, cow thyroid, milk, human thyroid --very roughly, these figures are quite rough and they are for April of 1955, and not all of the data are in or evaluated--

vegetation down on the east coast of the United States is running about 4 DPM per gram dry weight of the vegetation; cow thyroids in the Memphis area, also East Tennessee, are running in the range of 1000 disintegrations per minute per gram wet weight of thyroid; milk down in the Southeast is running about 100 DPM per liter, and human thyroids seem to be down pretty nearly unmeasurable, but, say, in the range of 4 DPM per gram wet weight. Again, these values are quite rough, but there are laboratory experiments which permit one to estimate what the discrimination should be along the chain between these values and these values within a factor of 2 or so are about what one would expect.

DR. LINDBERG: What is the time interval on that determination?

DR. DUDLEY: This is during the month of April. These are sort of gross averages. It appears that under normal conditions it is certainly ingestion rather than inhalation that produces the contamination in man and in animals.

FROM THE FLOOR: Is it a correct statement that about one-fourth of the strontium that gets into the food will get into the bone or the herbivorous animal, and about one-sixteenth in the soil will get into the bone of the same animal?

DR. DUDLEY: I guess with bone retention it would be somewhat less.

DR. DUNHAM: Dr. Dunning will now carry on.

DR. DUNNING: As Dr. Dudley just said, I have done none of this work myself, either. I have tried to evaluate other people's data. I have put on the side here some booklets which you may find of interest.

This may be a bit superficial inasmuch as we did not anticipate strontium-90 being declassified this morning, so Dr. Dudley has really given you the heart of this.

I would like to spend just a few minutes on some of the broader aspects of our problem of nuclear testing. I want to limit our discussion to the testing and not to warfare.

These data, I emphasize, are not precise. They are subject to change without notice, but there has come to be so much misunderstanding and apprehension about our testing program that I feel it is essential that we get a few of the facts and interpretations before the public.

As I go through this, I hope you will forgive me if I stumble once in a while, but I am working from raw notes which we are currently working on at the present time.

As an introduction, as you all know, during the past decade the worldwide political situation has made it obligatory for the United States to initiate a program of nuclear weapons testing and development. The release of millions of tons of energy together with millions of tons of activity must be accompanied by some degree of risk. Since the continuation of our nuclear program is mandatory for the security of our country it is one of defining them and evaluating them as to what is best for the people of the United States. With what are we concerned? What are our problems on these tests? One that I will leave to you people, and you know more about it than I do, is the ecological aspect.

Going from there, we might break it down into the external radiation, body gamma doses. When we are speaking of somatic effects, we are concerned with the individuals; therefore, it would not be well to quote averages. What has been the highest exposure to any person in the United States from our nuclear weapons test? The answer is about seven roentgen. As you know, about 25 roentgen will produce some minor and not considered serious changes in the blood picture. About 100 roentgen would cause radiation illness with some people.

I might also mention that external is based mainly on gamma. There is also the beta problem. If the actuall fall-out material comes in contact with the skin and remains in contact, it could produce high enough dosage to produce a typical radiation burn. It has been observed in cattle and horses in 1953 and also natives in the spring of 1954.

Concerned with the external radiation is the genetics which is something we cannot get into too deeply here. Again, briefly, sometime ago I made a statement of all of the test series to date the average exposure, and we are concerned here with averages because we are concerned with population; the average exposure to people in the United States is one-tenth of a roentgen. When I made that statement, I did not intend to give the preciseness to it that has been ascribed to it by newspapers, et cetera. It does give a little feeling now of the magnitude of what we are talking about. What does this mean in terms of genetics? Again, it may be interpreted in many ways.

Incidentally, all of these interpretations I make--if you do not like them, you can make your own--but I will give you the basic data and proceed with a preliminary interpretation.

It has been estimated that it requires something like 30 to 80 roentgens to double mutation rate. This value, therefore, is in the range of one three-hundredths to one eight-hundredths.

In terms of genetics, we also speak in terms of how much radiation we get in background causes and also possible increase in genetic rate.

The latest increase by Moler is that the background may account for some ten or twenty percent of the mutation. Except for these numbers, the arithmetic comes out that this might cause an increase in natural mutation rate.

Suppose we continue our testing program every year. What then? If we assume that we have a fall-out in the United States every year, that is equivalent to the greatest fall-out we have ever had in previous years, and then the arithmetic comes out this way. (Chalk demonstration) These percentages now become 1.4, 2.8 percent, for a continuing program. That will be the percentage increase that might be expected in the natural mutation rate. It has been estimated that possibly on the average there might be 0.4 mutations produced in an individual from natural causes. If so, then, this increase would mean instead of carrying on these many mutations, we might have these many mutations (indicating) if we continue our test series indefinitely at as high a rate as in the past.

Also, we might mention in terms of external the . . . activity that falls on the ground, and at the time of some of the larger detonations in Nevada from a tower, you might find gross fission activity on the ground, and you would find within the gunnery range half a million microcuries at the time of fall-out. If you follow one of the . . . of the fall-out pattern, out to 40 or 50 miles, you might find something like 10,000.

Then, going out to, say, around the test site, 200 miles, roughly, on around there, not counting this actual path of fall-out, you drop off to something like 10 microcuries per foot. Then, at time of fall-out in the country, there might be some small fraction of microcuries per foot.

In Cincinnati in the spring of 1953, it was estimated to be about half a microcurie. At Brookhaven, it was estimated to be about a tenth of a microcurie. Those numbers do not have a great deal of meaning except in trying to relate them and come up with data. You have to go to the individual isotopes to get the meaning, at least for this group here.

Strontium-90 has already been covered very nicely by Dr. Dudley. I would like to add to it, before we realized we could talk specific numbers about Sunshine units today, I had made some estimates, having made them, and the original estimates were made by the New York Operations Office in their report which has been referred to. Highest--and this is in New Mexico and the Texas area--runs around one times ten to the minus four microcuries per foot. This was as of January 1, 1953, and there has been a small amount added in the current series this spring. However, it has been appreciably less than 1953. These values might be somewhat higher (indicating). The lowest value in the United States--in Arizona and New Mexico--are running around--those are round figures--and the average for the United States being about 6.

To try to get a little meaning into these numbers, I made a comparison with radium based on 1000 microcuries per radio. If you accept that number, then the arithmetic comes out (indicating) and that compares with strontium-90 ratio to the radium in the soils, and this ratio comes out so (indicating). This, so, and this, so.

What is even a more difficult extrapolation that we have attempted is an estimate of how much strontium-90 in the soil exists so that if one were to subsist off the soil entirely over a period of years, they would build up a maximum. . .
I think you recognize this problem is inherently very difficult.

The attempt has been made, and as of now if there be a quarter of a microcurie per square foot in the soil and these soils contain about one thousandth per acre of calcium, as has been mentioned previously, this is important in the uptake, and this, then, might eventually lead to the maximum permissible concentration of the body of one microcurie. If we accept this number (indicating) and if we compare it with this standard number, call it whatever you will, the arithmetic comes out like so (indicating). Once again, that is based on things as of today or pretty much as of today.

Suppose we continue our testing program indefinitely. What we might lead up to, because this is a problem of build-up since Johnson has a half-life of twenty years, assuming we have a fall-out, the average for the United States--the equilibrium value would be about ten times this value here (indicating). This is equilibrium value for the average of the United States, again making this same comparison with radium, and this arithmetic comes out so (indicating) and so, with the base of this comparison here (indicating). I repeat that those are very difficult estimations to make, and this whole business and these numbers here could be low by a factor of 2. I think it does give a little perspective as to what has happened and what might probably happen, or perhaps I should say what might possibly happen.

With respect to the iodine picture, that has already been covered very nicely. Just briefly, the greatest concentration of iodine that we have ever found in any animals was in sheep grazing near the Nevada test site in 1953. Grazing animals will pick up appreciable quantities of activity from the surrounding areas, as you know. The iodine did concentrate in the thyroid, as we estimated, and the estimated dose to these sheep was about 2,000 reps. That is the highest that has been observed.

During the 1954 series, reported by Van Millsworth, there were found cattle in the United States--I will quote his figures. (Chalk demonstration) This was as of June 12, 1954. Again, making many assumptions and estimates, we come up with probably those cattle during the spring of 1954, which might have gotten something like 40 reps from the total fall-out. From there, our numbers drop down rapidly to such things as Dr. Dudley was just indicating for the current year.

Also, on humans, the measurements have been made. Those data are even less firm. The highest one that we have gotten on record here now is for some people living out around the Nevada test site. There is some question as to whether the reading they actually found there was due to contamination or actually coming from the thyroid, but if you accept that all of the activity measured did come from the thyroid, then the people out there had a maximum dose rate of about 3 millireps per day. This was in the middle of April of 1955.

I have considerable other data here on air, water, et cetera that we are apparently working on and getting in shape, but I see that my fifteen minutes are up.

DR. DUNHAM: Thank you very much. I know you have a lot of questions. Just fire away for a while.

FROM THE FLOOR: What was the basis of the comparison?

DR. DUNNING: A thousand. This was on the Curie basis.

DR. DUNHAM: Why did you compare it with radium?

DR. DUNNING: You know why I did. It is a common comparison. I do not like it so well as some people do, shall I say, because I do think there is a difference in the uptake. I think there is a difference in the uptake of radium and strontium, but since this is a commonly quoted comparison, I have shown it in the pot along with the other comparisons.

DR. ODUM: (Inaudible)

DR. DUNNING: I couldn't give an answer to that problem. This is based entirely on weapons testing. It is going to be many weeks before I get this story put together, and I have not attempted to go further.

DR. DUNHAM: Dr. Morgan, did you have a comment on that question?

DR. MORGAN: I was wondering about 1970 or 1975.

DR. DUNHAM: Wasn't there something implied in the question about this contributing to the activity in the atmosphere?

DR. ODUM: We discussed that yesterday, the changeover from weapons to power and what that might mean in regard to environmental contamination. You would not have fall-out, but you would have other things. I wanted to know whether anyone wanted to stick their neck out and say whether it would increase or decrease the contamination.

DR. MORGAN: I think there is little doubt that the equilibrium amount--50. . . of the power would be much more than we are getting now.

FROM THE FLOOR: I would like to point out in the case of atomic tests, all of the strontium that is formed is broken into the air and, consequently, will come down in our environment; whereas, the strontium that is formed in our reactor remains within the chemical waste resulting from the processing of the slugs or whatever the primary material is. While it is completely impossible to do any type of chemical processing in a completely closed cycle, some is bound to get out. Nevertheless, there is no reason to feel that any appreciable increase in the stontium level needs to take place; I mean in the sense of orders of magnitude greater than what have been quoted here. If it does, then we had better do something about our chemical engineering.

DR. ODUM: Then, there would be shorter life--

FROM THE FLOOR: No, I think it is chemical processing and keeping the activity contained or else dispersing in such areas--in areas other than the bias? field.

DR. DUNHAM: Are there any further questions? If not, I want to thank the essayists for being so careful to stick to the allotted time. I hope by being a little rough it has not interferred with the usefulness of the program.

We will now adjourn and return at 1:45.

(The Convention recessed at 12:30 o'clock to reconvene at 1:45 o'clock on the same day.)

FRIDAY AFTERNOON SESSION

. . . The Friday Afternoon Session convened at one forty-five o'clock, Dr. Boss, presiding. . .

DR. BOSS: This afternoon the program will be a little different from what it was this morning. This morning it was directly connected with the testing program, you might say, at the sites. This afternoon, it will be a little more diversified. Before we get started, I see Dr. Odum is getting organized.

I should like to mention that we have several brochures on this Eniwetok Laboratory that I mentioned this morning. Gene is going to talk more about that. He can speak about it since he has worked there for several months.

In addition, on the tables, you will find abstracts of most of the talks. There will not be abstracts of Dr. Lowman's or Dr. Held's paper, but if you want them just speak to these two gentlemen and they will send you copies of their talks, or at least abstracts of their talks.

Dr. Dunning has placed some more reprints on the side table near the front here which may be helpful for you in addition to the abstracts that he talked on this morning.

After the meeting closes this afternoon, which we judge will probably close at 5:00 or 5:30, we will then take a break, and then we shall all go out to the Navy Medical Center.

. . . Additional announcements . . .

DR. BOSS: If Gene is ready, I am not sure how to introduce him, but he says he is from Savannah River and Eniwetok. I think his base of operations happens to be out of the University of Georgia. In fact, he is the one who has educated me with respect to what ecology is, so he happens to be my favorite ecologist.

DR. ODUM: I am going to pass these papers out because I am going to have to talk to some extent from these abstracts.

Since some of the specific information is on this program as well as what we have accomplished, I would like to take part of the time to generalize, if I may.

We have been very specific so far on the program and we have had a lot of very fine, interesting facts and figures, and I would like to go to the other extreme for a moment. Maybe I am oversimplifying, but I sometimes think we need that to see the entire picture; then I will show some slides which will break the monotony.

All my pictures have been cleared. We did not go near these bombs, so we can show what is there.

Although these two projects we are reporting on are on opposite sides of the world, you cannot imagine two environments that might be more different than the coral reef and the terrestrial reef which is located in the upper plane as the Southeast, in the area generally known as the Sand Hill. Ecologically, of course, if we have any principles that are any good, they should apply equally to both places.

We have been interested in learning whether there are any common features or things which we can measure that can be done at one place or another since both of them are concerned with atomic installation.

The approach, then, that we have on both of these projects is somewhat different from the approach you have heard about this morning. If I may be just very elementary for a moment, we think of life as organized at various levels. In other words, we have protoplasm, and you can go beyond that, but we will not do so at the moment. We have cells, we have tissues, then we have organs, organ systems, and so on. So many people stop in their thinking at that point. The tendency has been to end with organisms and we find out the effect on the organisms, and if we go through one by one we will find everything. The ecologist, however, believes that that is not the complete story. Conventionally, we talk about populations, and then we talk about communities or biotic communities, and then we talk about ecosystems, or whatever you want to call it, and some of these philosophers would like to go one step further and speak of a system organized by mankind and his higher mind or something of the neurosphere. In other words, those are real levels. Physiology usually is concerned with organisms from here down and ecology eventually overlaps and is concerned with populations of communities and ecosystems. We are both functional investigators, we are both interested in dynamic processes in things that happen, and we can use a great many comparable methods, but we have different units, so that means we have to modify techniques.

Studying the effects of anything on this level of organisms is going on at a great rate, the genetic effects on the individual, the lethal effects, and so on and so forth. The question is, what can we do about those levels. The trouble is we know so little about these levels that we are handicapped at the beginning. Logically, it seems we should study organisms, population and communities and ecosystems to the future. There is also reason to start at both ends of your chain just as biologists have worked together for years in both directions and have come together. We can say, "Let's try the ecosystem for a while." We know we are in deep water but it does not hurt. Maybe we can learn something without knowing, for instance, all of the names of the organisms. I do not think it is possible in the coral reef to know all of the organisms, so we have to keep working up and down.

That is one point I would like to make. Unless we have measures, unless we can measure the ecosystem, we cannot possibly really know what the effects of anything are on the total function assaying total function--that is the philosophy, the theory, the purpose of both of these studies. In that sense, it is diametrically opposite from any other studies which are concerned with the organism level.

The next big problem facing us in this field is the problem of distinguishing between ecological change and change due to some factor you are trying to find out about, say, radiation, pollution, bulldozers, whatever it may be. Here is a factor and you think it is changing something, but we have this poem today. Did the rat population change to the population or did it change due to ecological change--so that means we must have means for distinguishing between these things. For many purposes, we should start with stable ecological situations in order that we can study changes due to something else. If we start with the young ecologic community, it changes so fast we cannot keep up with it unless we know that change pretty thoroughly.

May I make one analogy, and then we will go to one specific thing. If we assume that the ecosystem is comparable in one sense, the idea is that here we have characteristics, for example, rate of productivity, that does not mean a thing except in that level. Food change structure or ecological pyramid does not mean anything at the organism level. Mortality does not mean anything at some levels. We have quantative things which we can measure such as the rate of productivity. We can measure the structure of the whole just as we can take the muscles of the human being. As we made an analogy yesterday, if a medical man who is attempting to find out what is wrong with the human individual uses, of course, physiology of the whole measurement, he takes metabolism, blood rate, blood pressure, body temperature and so on. There he finds out whether an individual as a whole is normal. He knows when he does that what the basal metabolism is, and the blood pressure is supposed to be at a certain level, but yet we are called upon to answer some of these questions. We do not know the basal metabolism or blood pressure. How would a physician know that this person has a BMR rating; he wouldn't have the slightest idea. We are forced to do whatever no laboratory scientist does, and that is, work out these problems without the answers. We need to put as much emphasis on finding out the normal function as we do on studying the abnormal, and we can never interpret the abnormal until we know the normal functioning organism as well.

We know the rat, but do we know the normal functions of these higher systems and things of that sort. That is the broad philosophy.

Now, let us turn specifically to the Eniwetok work first. I will briefly mention the Savannah River and make a comparison between the two.

The coral reef represents apparently an ultimate in a stable, natural community with a history of thousands of years of adjustment between the organisms and environment. Since, of course, we have the nuclear testing going on in that area, this is an ideal place for critical assays on whole populations and entire ecologic systems in the field.

When we went to Eniwetok, we went with a different approach than what other people have when they are concerned with this or that organism or whole series of them. We are looking at the whole reef as an ecosystem and measure something that can be compared with that. Admitting that our success of the ecosystems will depend on the people who are working down in here and ecosystems will not be defined until you get this done (indicating) but you can get orders of magnitude. If it is a bit change, it can be measured by some of these disturbances. Consequently, instead of going to the north end of the atoll, once there, we look it over, but we stayed away from there and we selected for our study a reef in the south end which has not been affected, or could not conceivably be much affected by what is going on out there. We did this in order that we might be in a position to determine some of the normal functions.

We will now skip down to the summary. The paper on this work is in press, and it will be out shortly, the ecological monograph, so I will cover just a few high spots.

We attempt to do things on this one reef--measure the standing crop, that is, what is present at any one time at this particular time and, being a coral reef, it is going to be the same pretty much from year to year. There will be some seasonal fluctuations, particularly in some animals like fish. We attempted to measure the community metabolism of this reef and we picked a windward reef because it is a beautiful setup for that because your water is passing over it in one direction. By making chemical measurements here and there, you do the same as you do when you go and sit in front of that BMR machine and put it in your mouth and get an oxygen consumption on your part. We have our oxygen machine on this reef and we measure the differences in oxygen. In the daytime, it would be a measure roughly, of course, of production, and at nighttime it would be a measure of respiration.

Those of you in aquatic biology know there are some flaws in that area, but we wouldn't go into that at the moment. We are just looking for something that can be done very quickly. People have done that once for coral reefs and we have done it for other situations. People have also attempted to get a standing crop. As far as I know, no one has done the same thing on the one area at the same time.

Theoretically, here is the theory: If the community is stable, then you should have a balance between your growth and your production and between your consumption so that it should be balanced. You should then have a relationship between your various parts of the community--you plot these as producers, meaning those organisms that are making food--that would be your green plants but it would include some non-green plants. Then you have another level such as consumers, and then we have these herbivors, and then the carnivors.

Here, looking for things to measure in the total community (indicating). This is something that refers to the total community. It happens that the coral reef looks something like this (indicating) and an old field in the Savannah River area which is a changing community--they have different ecological structures. This one (indicating) has a small animal crop as compared to the plant crop, and this one (indicating) has a large animal crop compared to the plant crop. We could call that an HP ratio; the ratio of herbivors to the producers in the coral reef is about 20 percent. This is by weight. As you can see, numbers are nonsense in a thing of this sort.

(Indicating) This ratio down here on this area was about one percent. Theoretically, then, suppose we have this coral reef. Suppose we have two measurements made, which we have done. We have measured the total community metabolism and we find that is so many pounds of glucose per day, or so many calories, or so much carbon fixed per day, so we have the measurement. We also have the structure of the groups. Supposing something comes in this community that does not kill it at all, nothing is killed but perhaps something is affected down here (indicating). Perhaps this rate of production is slowed down in some way that you won't detect it if you are just looking for dead animals. You could shove this out of equilibrium and you might go back to this (indicating) situation. You can see that right near a bottom crater. What do you have? You have algae and no wild animals which will eventually creep in. First, then, you might have just a pure animal plant community and then you build up your animals, and so on, so an unstable community will have a differen ratio here (indicating), we think, than a stable one, and the measurement might be a measurement of stability.

This productivity, we think, is correlative with standard crop only in the standing community. There is no reason to think it could be correlated here.

Take a pasture, It has very little grass, which you can see at any one moment, because the cows are eating it up all of the time, so you have an unstable situation. These are the theories, then, that we have these two methods.

There is a third method of testing the community, the one that Miss Patrick has used very successfully. I do not have a slide on this. This goes with the small paper prepared for this International Conference, and it is supposed to show three ways of assaying total functions in communities, and we have illustrated it with the measure of production on the coral reef, and we illustrated the fundamental difference in the fields and a coral reef, and we have used one of Miss Patrick's bar diagrams and we have indicated a third way to get at the species structure change. If you know what species will decline with a certain degree of radiation and other species may increase in number, you will probably find one taking the place of the other. This works only, as Miss Patrick will tell you, if you have a team of people, and this requires, for instance, one person working on those methods, which can be done by amateurs like myself, on a coral reef, or my brother, of course, who has worked jointly with me on this project.

I think if we just look at some of these slides, we can illustrate some of these things better.

(Slide) The first series were taken out at the Eniwetok area, and they will show some of you what the possibilities for research are, and, as Dr. Boss told you, we hope more ecologists will go out there. If you do not know anything about a coral reef or vegetation there, you have lots of help out there, a lot of people in Hawaii who do know the stuff who can help you if you need help.

(Slide) This is the front of the reef. This is the seaward side and this is the reef stretching over here (indicating).

Here are those surge channels which were mentioned. The water comes up here and breaks across this ridge and rolls back and dredges out this channel. Other water rolls over this reef. From here to here (indicating) is one solid sheet of organisms. It is one solid sheet of algae in different forms. You can pick up a chunk of reef or coral and squeeze the chlorophyl out of it, and you come out with the same order of magnitude, so it is a big sheet of lettuce with all of these animals feeding on it. It is growing rapidly and the animals are eating it rapidly.

(Slide) This shows the rest of the reef from that ridge. You have the area where the corals are low and small, and as you get back into deeper water--it will show up in the next color slide.

(Slide) This is a reef off of Perry Island, and this is an example of an island reef. Here are the breakers out here. This is a good example of a community which is senile. This community is decaying faster than it is growing because it is thought that--geology evidence is that the water level has dropped some in the last few thousand years and these island reefs have been exposed more than the other reefs. The result is the coral reef is all dead in this area and it is covered with the skum of algae, but it is eroding down and probably the erosion is greater than the reef.

(Slide) Here is a reef where this is not true. The whole thing is living, thriving coral colony. There are no dead zones in it. This is the so-called algal ridge which is just as much coral as reef.

(Slide) This slide shows how you can operate with simple equipment. This is the ship known as SS Production. It consists of two inner tubes tied together with boxes which are slotted for bottles, each one for a chemical measurement, oxygen and so on, and here are batteries and searchlights. We just pulled this thing out, tow it out, and we have stakes driven in the reef and we tie it up and we work from this float. You do not have to have a million dollars worth of equipment to make these measurements.

(Slide) This shows a little better the two floats with two boxes. We had to turn over twice. It could have been a little better, and we lost a few samples that way, but mostly it worked out very well.

(Slide) This will be looking down through the clear water. The water is just a few inches deep. This is the zone of encrusting coral. This is one species here (indicating). This is the so-called stinging coral. In between the coral, there is this bluish area which is actually a hugh mat of algae of all sorts, and the production here is terrific--gas bubbles forming at a great rate, and a lot of food is made here which flows back and is used.

(Slide) Here it is going down the small edge. Here is one of the didiacnaclam, and the mantle is green. When you take that mantle and extract it, it is full of chlorophyl. When you grind up the shell, it is full of chlorophyl. Even the animals in the coral reef are producing food and everything is full of algae. It is amazing.

(Slide) Here is the deeper water and you can see the branching form appear. In very low spring tide, these bigger heads are exposed. (Indicating) Here is the blue coral. It is blue all over, but if you grind it up, you will get a beautiful green solution. We cannot tell by looking at these things what they are doing ecologically. Animals here, at least many of them, are behaving more like plants.

(Slide) This is three kinds of coral broken up fresh.

The only kind I ever saw before I went out there was this clean coral. The animal part lies as a coating on top, and you can see the green layers. Coral is just chuck full of green material underneath. This stuff here (indicating) has been called boring algae by previous workers. It is considered parasitic.

On ecologic ground, we feel it cannot be. These are important because they are making food and they are necessary for the survival of the coral, because as soon as one or the other dies, the other one dies.

Also, here again, radioactivity produced beautiful checks on that. We took one of these corals and made a radio autograph. We expected the algae to be black. There wasn't a sign of algae at all. The surface was black, showing it was radioactive, but the inside was not. The only possible explanation we can see for that is that the algae are actually not in contact with the environment, but are receiving their nutrients and swapping nutrients with the surface layer and, therefore, a lot of the things in the water are not getting down. It is not being circulated, and not hitting here.

I have radio autograms here if you want to see them, and you will see there are no signs of the outer layer at all.

Let me call your attention to two or three of the statements in this sheet, and then I shall comment briefly on the others.

Number 3 up there on the top of the page--we have pointed out that because producers of algae are so intimately interwoven with animal and dead skeletal material, the chlorophyl extraction method appeared to be the most feasible means of estimating producer biomass. This is open to question in many ways, but if anybody can think of another way of doing it, we would like to know. We found no way of comparing it with the amount taken from the coral. The measurement is checking very well. As far as I know, this is the first attempt to quantitatively determine the poundage of plant material anywhere in the plants and animals which cannot be separated by hand.

(Slide) Here is a way of getting around that little difficulty. We found on a horizontal basis, if you take a horizontal basis, that the content of producer material, the poundage of algae, if you want to put it that way, is the same almost anywhere on the reef, if you assume that that lettuce is the limiting factor. In other words, branching coral, you might think that must have more algae because it has more surfaces, but it does not. If you take the crusty type, they will come out the same. The branching part is a part of the animal part because the animal part is able to get more. . . The ocean around the coral reef is very low in productivity, about one, one-twentieth in food production.

We have here one of the most exciting things I can think of as an oasis in a desert. The reef is an oasis in a desert and the only way it stays that way--it does not let anything go. This close association between plants and animals facilitates the

I think man has less to learn there. We put ourselves apart from our plants and the further we get away from our plants the more less we have in interchange between them.

Now, let us look at the last few slides. Some of the other results are diagramed here and can be covered in that way.

In the Savannah River area, we are doing the same thing. We are working with old fields. Mr. Herde has some information on Savannah River, the type of habitats, and so on, and you can pick up that information, if you wish, afterwards.

We also have summarized it on this sheet for anyone who is interested. We invite people to Savannah River who are intereste

We have wonderful help from consultants, and some of the things we would not have been able to do without them. If we want a fox census, we get a man who has had experience with foxes.

Here is the atoll. The work reported on previously took place up here (indicating). The wind is here. The zonation is a little different. It will vary around this atoll, according to the prevailing wind direction, the strength of the current and things of that sort. This is where the laboratory is located and this is on Perry Island. The facilities that were mentioned are excellent, the food is wonderful, and since I like to eat, we really had a wonderful time out there. I never saw such food. They keep piling steaks on a mile high.

(Slide) This reef here is the one we picked because it was convenient, undisturbed, and we could get to it and, therefore, we could spend more man hours on this than if we had to get transportation to this one.

(Slide) Here is an aerial photo of the reef. Here is the ocean and here is the outer ridge, and here is the encrusting. We like to use these names. The physiography is more constant.

This is a cross-section of our reef. We don't have this boat channel on here or anything of that sort in this sort of relief mainly because the current, I think, is stronger. We have this gradation from this ridge to the smaller coral formations to the larger ones and the zones here, certain channels, crusting zones, small heads and large heads.

This is some data showing you the current velocity.

At real high tide, you can stay out there.

(Slide) Here are some figures showing the way the reef conserves things. This is Plankton collected at those spots. This is collected at several points on the reef. There is not much organic matter arriving on the reef. This goes up very high but the stuff breaks off and can be quickly grabbed by all of the organisms. There seems to be some loss here. These are crude figures. The same is true with plant material. That material is grabbed off rapidly. Sand is stirred up on that point and this low level of radioactivity, which is present on the thing, is higher in the front and is very low in the back.

Again, illustrating that water arriving there with anything in it is quickly grabbed off; therefore, the radioactivity on the reef. . .

Here are the quadrats that we made. We made very careful quadrats just as a plant ecologist would use. This is the ridge. These are corals. Notice there is a lot of coral on that outer ridge. These are corals, and so on. This is back beyond the zone of coral. This is what the coral looks like. This is what we consider the most exciting discovery, if we may call it that, and this is an ecological thing now, but it has an importance in all of this. As far as I know, it refers to actually determining how much material in a coral is plant and how much is animal. We did that in various ways, which I will not go into at this time, but the final result is that the coral is three times plant to animal and plant material is in two forms--the zone and. . . which are the small cells in the coral endoderm itself. The same is true in hydro. This is the most exciting part. This is the stuff that must be manufacturing more food.

This is for one particular coral of the reef and this is for the whole reef. This is the kind of pyramid we got when we add up all of these things, and this includes the coral, the dead heads, the bunches, the shingles and the algae in the sands. Everything has algae in it. The herbivors have fish in them. They are feeding on algae.

These are crude estimations, but when we do all of the reef together, I think this has meaning here, this final thing, and this is the figure that I gave you--20 percent ratio between those two layers. I think it is a significant figure.

One other significant figure we got is the annual turnover of the community. That gives us the third single estimate of the total community. That is something we want that we can put down in black and white so we can compare that community with something else, and that is the ratio of the standing crop to the amount of production. In other words, how often has the community replaced itself during the year, and the turnover there is twelve times--in other words, for this (indicating). We need a lot of other people to get the same turnover ratio in other communities. I know it would be entirely different for a non-stable community when compared with a stable community. With the non-stable community, you would have loss of a standing crop there.

This is on the same page that I sent around. This is the oxygen difference translated into grams of glucose--that is, assuming, as we sometimes do, that all photo-synthesis starts out with glucose. This is our curve. This is one that Sargent and Austin did on another reef. Our's looks to be considerably more productive than his. Also, we have a good many more points on that.

This gives us a figure when translated of 74,000 pounds per square meter per year as productivity, which is pretty high.

Sugar cane production in Hawaii will be equal to that; but I do not know of many agricultural efforts of man that produce as much food as the coral reef. The catch is that the coral reef is eating it up as fast as it produces it. One is demand and one to nature. Here is a factor. Anything that messes up the production of this coral reef or changes this figure is bound to affect the community as a whole. Conceivably, we can change the metabolism of this community without killing an organism. You might kill certain organisms and not affect productivity. What difference would it make if we wiped out one sensitivity organ itself? It might not make a difference, and then again, it might.

DR. BOSS: I have just one comment. The ecologists whom we sent to Eniwetok are hard workers--and they are big eaters. Gene's brother, Tom, made a lot of the boys out there acquire an inferiority complex when it comes to a knife and fork. Gene didn't tell you too much about his Savannah River Work, which we are very much interested in, but I believe it is well documented in the press. I imagine you can watch for his articles as they come out, and that will fill you in on that side of it.

Are there any questions that you would like to ask Dr. Odum?

DR. HELDE: If the areas where we had higher levels of radioactivity--

DR. ODUM: I would assume it must be early in the game. The point is that this reef we had, the activity, I should have mentioned this, was coming from Bikini. That is on the windward side. We are not getting any activity from the northern side of the atoll. Therefore, what we are getting is a low level thing which is what most of the people out there told us, or thought anyway, was coming from tests 400 miles away at Bikini and flowing over the reef. Therefore, being a low level and passing these longer lines, isotopes which don't metabolize so readily, but as you say, if you have a lot of phosphorous, I am sure that would go down in there. There is a case where we could use this little slight radioactivity to advantage.

FROM THE FLOOR: Sargent and Austin--was that the Bikini reef?

DR. ODUM: No, that was done some years ago. Their full paper on that has not come out.

I have one more thing. This paper summarizing this graph, there is an abstract of that if anybody is interested in that.

FROM THE FLOOR: How deep are those algae into the coral?

DR. ODUM: I have a slide on that. They vary with the corals whose skeletons are very porous, and they go 2 or 3 inches, and they carry these bands.

One way to interpret the carrying of these bands is that the nutrients are being used up and they collect again. The band got fainter. Sometimes they have five or six bands of this algae underneath the coral.

DR. BILLINGS: What is the light situation?

DR. ODUM: Extremely light, way down in there. You can go in four feet of water and you can take a light meter and it almos blinds you. The skeleton is quite porous. The little layer of animals on it is very thin. Actually, those algae are killed by direct sunlight if you scrub the outside and the algae die. They are embryonic.

If you take a coral head and put a belge on it and measure the oxygen, in the daytime, the coral produces oxygen and at night it balances. If you put plastic bags over the head, counting the bubbles that are produced and the coral would produce bubbles in the daytime, and it would be gone, and within 24 hours there would be no bubbles. If you put your bag over a dead head, then you get nothing but bubbles. So, the three-to-one ratio is just about enough to balance the books. In other words, how many plants it would take to support. Those algae must be pretty active metabolically because, in most communities, it takes five-to-one ratio to make. . .

Our next object on the Savannah River is to shove our bomb in a bomb calorimeter and come out in calories instead of grams. If anybody thinks there is anything wrong with that--I think it costs $750.

FROM THE FLOOR: What percentage of the normal reflected sunlight did you have?

DR. ODUM: My brother could answer that question better than I can. Six percent. It is high, although it is low to a physicist; he has an article in the American Scientist where the theory is to be productive the community has to have a low efficiency in order to get it done. It is analogous to a speeding car. You may have to go 70 miles an hour to get someplace, but you can't expect to save money on gasoline. People can do a little thing and get 30 or 35 percent efficiency, but as soon as they put an acre of algae out, you don't get high efficiency.

The coral reef, we think, is about as good a thing--that six percent is pretty rough. I don't want to change that because that is his figure.

MISS PATRICK: Do these corals eat these?

DR. ODUM: No, not directly, but they may take in some that float off; but we think it is a nutrient change. Corals have. . . They have these little bits of protoplasm that wind out around in there, so I don't see why they can't swap stuff. Here you have 40 percent of the reef covered with coral. If coral is an animal, then ecologically you can't have that. There is not enough Plankton. The quantity of Plankton would be nothing compared to the whole area, so this whole mass of coral has got to be supported--in other words, the total community approach shows you that it has to be that way.

Now, it is up to somebody to figure out how it works.

MISS PATRICK: Is there any way of measuring this?

DR. ODUM: Somebody needs to go out to Eniwetok who is interested in this problem and work with the laboratory and study this embryonic relationship. Nobody knows what those algae are. Personally, I wouldn't classify them. They are part of the coral. We have names for the coral.

FROM THE FLOOR: I think the radioactivity that absorbs the outside--now, that couldn't be explained by just recent exposure. Several months after the detonation--so the fact that the radioactivity is not getting in agriculture and adhering to the algal cells as most of it is traditionally expected to be, indicates that these algae are living in a pretty much isolated environment there and are completely screened from the outside and all of the nutrients that they receive are either from the by product of the corals or isolated environment in which they are surrounded. They go ahead and reproduce without ever coming into contact with the outside.

DR. ODUM: The algae on the outside are potentially radioactive. In other words, it is six months since the last test out there, so all of the short-lived metabolites are gone, so this is probably a rare instance which is sticking on there and is not being used, so, to me, you have a cinching argument.

DR. BOSS: Our next speaker, Miss Ruth Patrick from the Academy of Natural Sciences in Philadelphia, has informed me that she is going to exert a woman's prerogative and change the title of her talk.

MISS PATRICK: It is a runover, shall we say, of the work which we have done at the Savannah River plant, and I shall outline to you the various phases of this work.

Our program at the Savannah River plant divides itself into three parts and was to, shall we say, accomplish three things.

One was to conduct baseline studies on the River so that we would be able to know if anything happened to the River in the future. By anything that happens to the River, I mean any deleterious effect due to heavy organic load, toxicity or temperature.

The radioactivity work was made into two separate studies. We were to keep a continual check on the River after these baseline studies were made, and, three, we were to conduct special studies such as the temperature studies that I will discuss briefly.

Now, the purpose of Part I was to establish a baseline for the aquatic life in the River. In order to do this, we laid the greatest emphasis on those forms that lived in shallow water, usually, but mainly those forms which cannot move very far or are touched, for it is these organisms that have to take it. They cannot move up or downstream with changing conditions as can, of course, Plankton.

These organisms have light history, thus, by studying the structure of the population, you are able to get an idea of how long ago a bad or deleterious effect occurred.

We use all groups of organisms in these studies, for different groups tell us different things. The algae and protozoa most closely reflect the chemical changes in the constitution of the water, whereas insects and fish will tell you more concerning oxygen contents--at least, sometimes this seems to be true.

Also, we feel that the more lines of evidence that you have for any conclusion that you make assure your grounds for these conclusions. Our quantitative measure in these studies are the numbers of species which we find of the various groups existing in a stream. We have found in ecologically similar streams the number of species stay remarkably similar. From our some 350 surveys, we find that this is the most reliable index to use as a quantitative measure to judge the condition of the stream. This work has recently been supported by the work of Usinger and Neider, who took a . . . as uniform as possible as they could see. They used a square foot sampler which they had developed. They conducted these studies on insects. They found that to get a statistically reliable number as to the number of species present, it took only two or three samples; whereas, to get a statistically reliable number as to weight, they took 133 or more samples, and a statistically reliable number as to numbers of individuals took at least 70 samples. They were working on a uniform riffle, and you can well see in a collection stream the difficulties of sampling to get something that would be statistically reliable. I feel sure we will develop, along with the methods of Dr. Odum, statistically reliable methods for this, but at the present time, we believe that numbers of species gives us the most statistically reliable number for judging condition. I am not talking about total productivity, but the condition of the stream. In other words, the condition of this ecosystem of which Dr. Odum has recently spoke.

Of course, we consider other factors. We consider the chemical composition, we consider physical compositions and bacteriological characteristics. You can see from this brief account that it is extremely necessary that one carefully choose the areas to be studied. They must be as ecologically similar as possible. They should all exist in the same region of the river. In these large coastal plane rivers from numerous studies we have conducted, we find that there is usually a region of steep gradient and an intermediate region of gradient, and before it enters the ocean, an area of negligible gradient.

Now, let us look at some of the results of these studies. We considered, as I say, everything from protozoa to fish and algae.

The protozoa of the Savannah River constitute a normal assortment of species in all of the major groups of protozoa. A total of 229 species were identified. The larger number of these species were sciliates or flaglings. When we look at the number of species found at each of these stations on the four surveys, we found that we had an average number of 23 species at Station 1, 24 at Station 3, 25 at Station 5, and 23 species at Station 6.

However, if we consider the kinds of species, we had only 5 species that were found on all of the surveys. This was probably due to several reasons. One is the life history of protozoa, which is very short; two, that there are many, many species that can occupy each of these ecological niches; third, protozoa are very fragile and easily destroyed and can easily be replaced, and then seasonal variation means that you get a change in kinds of species from season to season although the number stays remarkably similar. This seems to indicate in a natural stream that has not been adversely affected by pollution, you have a so-called number of ecological niches, being a lot of species to fill those niches, and these niches are all filled and, therefore, from season to season, another will get in and out, and the total number of species will stay remarkably constant.

In the lower vertebrates, we found that the River was rather limited as to the lower invertebrate form. This was due to the shifting bottom of the River which has a very heavy bottom load, the trabid of the water, the very restricted liberal zone and the fact that most of the bed is sand. Those of you who have been on the Savannah River know that the River well fills its channel and that in strait courses of the River, if we have a strait course here, and you were to take a profile, it is something like that in shape (indicating). On a meander, you will have a zone of this sort coming out on one side (indicating) and this back will be very steep.

Now, the Savannah River at the time of our baseline studies had a very fluctuating water level. That meant that this area was being continually in deep water and shallow water, and during our first survey, the areas around here were very barren. The bottom organisms that would become established on this area (indicating) where you had mud deposited. You see, your current picture goes from here (indicating) back to here; your current is slower, the mud is deposited here and here (indicating); the silt load, and you do get numbers of worms. That is the richest area in the River, but these rivers are very narrow, so the lower invertebrate form was relatively limited.

As in the protozoa, we got only 13 species present during all four seasons.

However, if we take the number of species which we find at all stations, we find that in Station 1, for all seasons, we had 16; Station 3, 24; Station 5, 20; Station 6, 15. If we average these stations together and treat the area as a unit, we had in the summer 55 species, in the fall 56 species, in the winter 58 species and in the spring 59 species, showing the remarkable similarity in numbers of species at different times of the year and at the various stations during all seasons of the year.

Insect fauna likewise was well diversified. There were 304 species, representing 9 orders. Most of the insects were-- that is, 40 percent of them were-- directly algae feeders; 38 percent were carnivors and the rest we could not be sure about from analyses we have made and from the literature.

Due to the high trabid of the River, the algae form literally is the grass of this River and the majority of our insects were found in association with algae.

When we look at these insects, we will find that there were 58 percent of them taken on only one survey. This is surprising because most insects have relatively long life cycles. However, when we consider that of this 58 percent, 20 percent were beetles which can only be determined during their adult period, and, of course, they become adults at varying times, it is natural that they would add to this number that were found only during one survey.

Likewise, at certain seasons, insects are extremely small after they have just hatched and the chance of collecting them is difficult. Also, many of them hibernate under stones and logs during cold weather. However, again, the number of species is very comparable at all the stations during the various seasons of the year. The average at Station 1 was 39; Station 3, 47; Station 5, 52; Station 6, 49. When we consider the surveys as a unit, we find that the summer survey had 85 species, the fall

83 species, the winter 39 species and the spring 127. Here we did have increase in the spring due largely to the fact that it is in the spring that many insonots mature in their larva state and emerge into the forms which were not found in the River, the adult insect form.

Now, with respect to the number of fish, the numbers of fish in the Savannah River were relatively limited. This is what you would expect in a river of this type. They were not limited to more than one which is typically found in this coastal range region. Their high trabidity was a deleterious effect, and the deep holes which many fish seem to prefer occurre on the outer bends of the River, and those bends of this River were very unstable. Cave ins were continually occurring, with the result that with this continual falling, it would be natural that it would hinder the development of these habitats for fish.

A total of 53 species were taken; 32 percent were found on only one survey. This is probably due to several factors-- one is since the species populations were relatively small in the Savannah River, it is difficult to always catch the species. Number two, the size of the fish during set seasons enable them to go through the maximum number of our traps; and, thirdly, the question of migratory fish. These factors were undoubtedly responsible for the shift in the kinds of species present.

The numbers of species, however, for the various surveys, again, were very constant. The mean for Station 1 was 18; Station 3, 17; Station 5, 18; and Station 6, 17. So, we find a similar picture with the fish and the algae also showed us the same type of picture as did the various invertebrates.

Our method of evaluating the condition of a river--that is, its freedom from pollution or deleterious effect--is not only based on the number of species. It is also based on the kinds of species and the size of the population.

Typically, in a river which is well-balanced, you do not find any very large populations of species. They are more all of moderate size. Of course, you may find two or three that are fairly numerous, but, as a whole, most of your species are represented by very moderate populations.

The bacterial studies on this River showed a very high total count and Holliform type count. This we do not believe to be due simply to the sewage in Augusta, Georga, but rather, we believe the fact that the drainage is swamp forest drainage in this area and this has a very profound effect upon the bacterial content of the River, and other people have found this to be true also.

In concluding, we concluded from these studies that the Savannah River was very similar to other coastal plane rivers which we had studied that had not been adversely affected by pollution. As compared with headwater streams, or even this lower gradient area, it was very low in productivity. This productivity would have been much less if it had not been for flood plane consequences in the swamp during low water stages which acted as a breeding ground, particularly for aquatic insects. One only had to go out on the Savannah River at five o'clock in the evening and run into a swarm of May Flies to find out what this situation was.

There are many oxbows in this River at this region. In these, the current is slowed down, the photo-synthetic layer extends down to a much greater depth, and it is here that they find the feeding grounds of fish. If you want to make fish collections, the place to go is to the sluice. These factors, together with the fact that the run-off from the swamplands contributed a great deal of organic debris, these three factors have undoubtedly increased the productivity of this River over what it would be if they were not existent.

Thus, we see in this River that we have identified some one thousand force species. Although we have turnover in the types of species and the numbers of species of the various groups, nevertheless, there still remains this relative constancy through the various seasons.

Since those baseline studies were made, we have made one other survey. This was made last summer after Clarke Hill Dam had been in operation for some time. This was to determine whether Clarke Hill Dam had influenced greatly the structure or the aquatic populations in the River. We found out some very interesting things. The effect of Clarke Hill Dam physically has been this: One, the retention of water behind the Dam means the water flowing into the River below the Dam which is much clearer because the settleable solids have been taken out of solution; and, two, the Clarke Hill Dam has been able to maintain a relatively more stable water line in the River.

Now, how has this affected the aquatic life? One, these banks which were very unstable previously are now getting an algae film on them and also plants are growing down on them, so they are stabilizing and the fall-in and trabidity due to their continual erosion is being stopped.

Two, the stable water level is enabling occupancy of these shallow water areas. Previously, these were barren. Now, these meanders at Station 1 and Station 6, particularly, and Station 1 also, equally, I should say, but these meanders are more pronounced at number 6. At number 1, we have a series of pilings and these shallow water areas are between these pilings.

Feeding on this algae are many invertebrates and small fish. The operation of Clarke Hill Dam--the total amounts of individuals of algae particularly have come up and, in turn, as Dr. Odum pointed out, this whole food change seems to be picking up. We have found more species in certain groups than in other groups, and the relative numbers of species are the same. I would say in all groups they are the same order of magnitude. However, we are getting larger populations because we have more habitats, and we have a larger fundamental basis for productivity.

So, then, the effect by the clear water and the stable water levels, or more stable water levels, has been to increase the photo-synthetic zone of the River and to allow those shallow water areas to be better occupied. It is interesting to note that the caddis flies, which are feeders, were not established in the River, and now we find considerable population of the caddis fly in the River.

It is also interesting to note that for each fish, because of the increased algae, is becoming more numerous.

Now, as a continual check on the River, we are using our diatometer. I do not know whether this group knows what our diatometer is. This diatometer is a method for measuring the structure of the diatome population. You may well ask, "Why do you use diatomes to measure the aquatic structure of a river?" The reason is that we have made quite a number of river surveys. As I said earlier, some 350 sections of river have been surveyed. At the University of Pennsylvania, one of the Professors, Dr. Strawbridge, took this data--at least, some of it--and analyzed the behavior of the total numbers of diatome insects and fish to see how they behaved, and he found that there was a very high coefficient of correlation between the behaviors in species numbers, or changes in species numbers, of diatomes, insects and fish. Likewise, in the laboratory under grants from the United

States Department of Public Health, we have been able to determine that the sensitivity of diatomes to temperature and also to chemicals are very similar. They are in the same order of magnitude. They are a little bit more sensitive than fish, but they are very similar. Therefore, there is some logical reason, or we think good, logical reason for thinking about diatomes. Now, our problem was, first, if we were going to study diatomes, our first problem was to design an apparatus which would collect all kinds of diatomes, free-floating ones and attached ones. Secondly, we had to determine how long this apparatus should remain in the River or how long the slides should remain in the River in order to get a representative sample of the population. We also had to determine how many of these instruments should be put in the River. We devised this little apparatus here. These have now been changed. They are cork floats now because hunters like to take pot-shots at these. This floats below the water, and this vein (indicating) is adjustable to the amount of pressure passing over the slides. We have found that you do not have to treat these slides, and the reason is that all diatomes are surrounded by a film of pectin jelly, and therefore, they can stick to the slide. Once a diatome dies, it sluffs off so you get the living, growing population. This is not just a trap. The diatomes actually live and grow, and you can see them dividing on the slide.

The slides are withdrawn every two weeks. This is the time which we have found to be advisable. Of course, by staggering the slides, you can have one slide ready every day if you really want to.

When you withdraw these slides, since diatoms have a cell wall of silicon, no preservative is needed. They can be put on a shelf and kept for 50 or 100 years, if you want to keep them that long, before they are analyzed.

The next question was how we were going to analyze our data. Some time ago, I read an article by Preston published in the Journal of Ecology wherein he set forth that natural populations conformed best, or the statistic that fit them best, was the normal curve. At this time, people thought that the . . . of description was the kind to fit the population best. Fisher and Williams and others have come over to believing that the normal curve is probably the best statistic for compressing the total universe of a natural group of organisms.

We were the first ones to show this in the plant. The curve has been tested. We have tested against our data, against the negative bionomial and against the Pawson distribution, as well as the truncated normal distribution. These mathmatical tests were done by Dr. Lawson who is head of the Mathmatics Department of Temple University in Philadelphia.

It is perfectly true that the negative bionomial will give you a fit. It will not give you quite as good a fit as the normal curve will. Also, I would like to say that Dr. G. Evelin

Hutchison of Yale University has studied these and criticized these interpretations, and Dr. Hutchison agrees that the normal curve is the best fit for this data. Therefore, the question was, then, how many of our diatomes to count. The number you count depends upon how reliable a structure the population you want. If you want a very reliable, like a 95-percent confidence or 99-percent chance, then your mode will stay the same, and it should be over here at about the fifth octave. However, if you want a reasonably sure statement, if you got it over here in the third integral octave, it is changeable. The abcissa are the number of individuals. The ordinates here are the number of species, and, thus, by calculating first by counting and then by calculating the normal curve for your data, you get these types of graphs.

These graphs which I will show you are for the Savannah River. From this one, you can see for yourself how remarkably constant they remain. This is one from our Station No. 1 and the Savannah River. The solid line represents October 1954 and the dotted line was July 1954. Here we have the one for October 1953 and January 1953, a slight change, but remarkably similar.

Here you have one for April 1948-July 1954, and this is the one for January where, again, it is down again as it was over here originally in the wintertime. Essentially, it is the same curve, and a normal distribution without any enrichment whatsoever, and usually the curve covers about eleven intervals.

In the case of where you have a bit of an organic load, you get a lengthening of the tail. In other words, you get a few species becoming extremely numerous. This, of course, fits into our general idea that in a stream which is natural, that has no pollution of any sort, as we usually think of pollution, you have a great number of species and not any too large a number of any one specie. And the first effects of pollution, particularly organic pollution, is to make a few species become more numerous, and then as pollution gets worse and worse, we have a decided drop in species numbers.

You will see here that on one of these which is in the four-mile creek, a pollutant went into that stream, and this is what happened to the curve. So, by keeping these continually in the water, and then by changes and then, of course, we are not relying entirely on that, but we are going in again this summer to check the River. We do have a continuous method of diagnosing river conditions.

Now, as to the temperature test that we have done, our temperature tests were aimed at determining what temperatures a stream could stand and yet remain normal. At once, we realized from our experiments that in a natural environment you must think of optimum temperatures because of complication of species and not maximum temperatures at which organisms can live in the laboratory. This is clearly illustrated by the fact that we took some slides from one of the rivers near the Academy in Philadelphia, and we put these in good nutrient solutions at 20 degrees. Diatomes were dominant. We then gradually raised the temperature to 23 degrees when greens took over on the same slides and became dominant. We then gradually raised it up to 35 degrees when the blue-greens took over and stayed dominant until 40. Incidentally, this took months to do. Gradually, they were brought down again. When we hit 35, the greens came up. In other words, there were a few greens left that were alive. When the temperature conditions became right, they could outdo the blue-greens, so the greens came up, and then, when we came back down to the lower temperatures, the diotomes came up again, thus showing very clearly that you must think about optimum temperatures when you are thinking about preserving a normal area.

The fish experiments were of two types or, really, three types. One type of experiment was to show the temperature at which the fish could leap over a gradual climatization. We found that we used channel catfish. We used a pin perch fit, bluegills and bullheads. The channel path, the highest temperature by very gradual accumulation, could withstand 33 degrees. We got 100 percent survival for 49 days. Here the experiments on the bullheads were at 30.5 Centigrade, and we got 100 percent survival for 58 days, and here at 34, we got 100 percent survival for 40 days, and then 80 percent survival at 50 days.

For the bluegill, we were able to get 35.5 degrees Centigrade. We got 100 percent survival for 27 days. These tests were not run as long as the others and maybe it is a little too high a temperature. As one might expect, the pin perch was the most sensitive of all, because the pin perch is an inhabitant of the sea and naturally has a more stable environment that heats up less and they are evolution-wise, adapted more to high temperatures. This experiment was run in Texas at 27.7. We got 100 percent survival for 8 days and 90 percent survival for 17 days.

At 30.6, we got 100 percent survival for 5 days, and 90 percent for 13 days, showing that it was more sensitive than the fresh water fish.

The shock test which we did, wherein we took a fish from a given temperature and then suddenly raised it to a much higher temperature, showed that acclaimation will alter the flight of the temperature shock that a fish will stand. This is in accordance with the work of Frye and Duderoff in this relationship.

The third type of test we did was to shock them and then keep them at the high shock temperatures, and this was similar to the one where we shocked them and returned them to the origina temperature. These experiments are, of course, relatively rough experiments, but they do emphasize the magnitude of the problem. This temperature problem is one of the greatest ones that faces us in our rivers with our expanding, shall we say, use of atomic energy.

We must study the effect of metabolism on these organisms because we have found that these fish reach these high temperatures while they are alive; they become ravenous eaters, but they still become thinner and thinner. Likewise, their susceptibility to disease and their problem of reproduction should be considered. We know very well that many fish in reproduction have to have cold water to stimulate them, so we believe that this is one problem that needs further investigation.

DR. BOSS: Thank you very much, Miss Patrick, for your interesting presentation. There will be more about this temperature problem on the Columbia River, and that will be given tomorrow by Dr. Foster.

As Miss Patrick has pointed out, we have recognized that it is important and a lot of research has been done on it.

DR. ODUM: You talked about taking more samples to determine reliability on the basis of weight. In the work that you are talking about, it is restricted to one part of the community. When you consider the entire situation, you would not necessarily apply some of this to insects. Insects may go down but something else might come up.

MISS PATRICK: I think we should statistically test and see.

DR. ODUM: I would not agree with you that the Savannah River is a swamp. That comes from the Piedmont.

MISS PATRICK: The immediate drainage of that river is all through swampland.

DR. ODUM: You mean in high water it flows through swampland.

MISS PATRICK: And when it rains.

DR. ODUM: Do you have an estimate--

MISS PATRICK: It is increasing the bacterial load and this is true of all rivers of this kind. The US Public Health Service has also found this to be true and characteristic of coastal plane rivers in that section. When you talk about main body of waters and you talk about seeding, you are talking about two different kinds of problems.

DR. BOSS: I doubt that some of the people in the back of the room could see your tables. You might put them out so they may look at them.

We will now take a short break.

(A short recess was taken.)

DR. BOSS: There is a little difference in the program here. Dr. Morgan, the Director of the Health Physics Division of Oak Ridge National Laboratory will give us a short talk, and I believe he will introduce Dr. Auabach who will talk at a little greater length. Dr. Morgan, will you carry on.

DR. MORGAN: I would like to begin what I have to say by giving you a conversion equation that might be useful to the previous discussion and what will follow later. I believe these figures were placed on the board this morning, but the dose rate can be given as equal approximately to 300 times the concentration, times the energy, and this is approximately equal to about 100 times the concentration in which the dose rate is given in rads per week and the concentration is given in microcurie per week, and the energy is given in MEV per integration, the MEV absorbed in the MEV per integration. Then you end up with a simple relationship times 100 of the relationship, times the microcurie of grams. The program at Oak Ridge National Laboratory began about 12 years ago, and during that period, it had many problems of radioactive waste disposal. The high level of radioactive waste and the material that is to be recovered--that is, the uranium waste--is stored underground in tanks. The low-level activity waste, the fission products, primarily, is discharged throughout the White Oak Lake into the Clinch River system. We will hear more about this later. I will try to lay the background for several of the following discussions.

In addition, we have the gaseous waste, and we might symbolize our operating situation in this manner: We have, then, the plant and our stacks from which we have the radioactive waste. Downstream, below the plant, we have the White Oak Lake. We have the gases coming out from the stacks and the satisfactory control of operation is that at places where people are likely to get the maximum permissible dose, the dose should not exceed the maximum permissible value. In the early days, we used the tenth Roentgen per day, so the range from 7--we will call them rads--rads per week, down to the .3 rads per week as the permissible level.

Now, beyond White Oak Lake, of course, we discharge into the Clinch River, and we add a factor of safety of 10 in general at this particular point; that is, down here (indicating) we try to operate at about one-tenth the maximum permissible dose rate. It turns out that you are very much on the safe side if you assume the permissible concentration is ten to the minus eight per gram of water and this would correspond to one-tenth of the maximum permissible value of ten to the minus seven, or ten to the minus eight, which is the value that we shoot for. These are above the contributions to the natural background.

We do not maintain these levels in occupied areas at all times, but we, in general--the integrated dose rate is somewhat below those values. We have carried out many different types of surveys throughout our area. I will only mention a few that have taken place during the past dozen years.

One interesting type of survey has to do with the determination of the activity of the mud in the Clinch River in the Tennessee River System. We followed this mud at varying distances, sometimes up as much as 500 and 600 miles, and we found a characteristic pattern with an exponential drop as you go down the river and approach the dam where it builds up, and just below the dam where the velocity of the dam increases you get a sudden drop in the concentration; then it builds up again as you approa[ch] the next dam and you get a series of dam loops in your curve as you get further on down the stream. The faster the water, the less the contamination contained.

I do not believe in this discussion we will have much time to mention our water vile studies, but they have been revealing in that these birds have been marked and trapped and located in many of the states east of the Rockies and in Canada. Most of the activity that they contain is P-32 and they concentrate at many folds.

Dr. Krumholz will discuss tomorrow the TVA-ORNL ecological studies in White Oak Lake, and I will not do more than describe the locality at which these studies were made.

The principal program at our laboratory has to do with reactors, and during the past several years, our prime interest has had to do with our power reactors, atomic reactors for power as against production of plutonium or tridium or some other material. Our particular specialty is that of homogeneous type reactors, and here we mean you have a reactor in which the fissionable material is a fluid and this, we feel, offers many advantages for future power uses in that with such a system-- we might diagram it somewhat in this way. (Chalk demonstration) This is the reactor itself and then we would have to have a heat exchanger. We put some big boiler in here (indicating) pass water through it, forming steam or salts or something else, metals, to carry out the heat from this point. This is the fissionable materials circuit which you see. This does not require any expensive fabrication, and in this part of the cycle, of course, we can place our generator and so on back to here.

Here is where our problem begins primarily. At this point we take off a certain amount of material continuously in a process plan and we can continuously process all of the material, or part of it, as it passes through and, of course, from this, then, we have our waste disposal studies. We have been spending a considerable amount of time in studying various methods of waste disposal. At the present moment, it appears that various types of salt detention offer the greatest promise. We have experimented with numerous type hits, some of them lined with various types of soil. Others are cool pits where we could cool down the soil. Perhaps the most exotic situation is where we would permit this million-odd microcuries per day to reach an equilibrium where it would boil the complete dryness in this open pit and melt the soil and form a somewhat volcanic mass of rock, and we feel that this offers certain advantages. On the other hand, it transforms the liquid waste disposal problem to one of air contamination, and we have a rather large group studying this particular type of problem.

We have had a large group for the past six years studying soil chemistry and various related problems, but this work is all published in the open literature, and I refer you to that for these considerations.

Dr. Stroud is in charge of this particular work. He, with a very fine group of US Public Health Service people, is working with our men in conducting this research. It is a very fine working relationship.

The ecological studies per se that are being considered at present are under the supervision of Mr. Strumness, who was ill, and for whom I am pinch-hitting this afternoon, and Dr. Auerbach will discuss this in some detail, this particular program, and the plans we have here.

In conclusion, I would like to say that we are interested for the present in fundamental ecological studies. For example, if one takes a balanced environment of some type in which you have bacteria, arthropods and plant materials and other materials, you have this food web and these interrelationships between these various components to find out more and more of the details of such a balanced system.

In our particular studies, we believe that we need to find out something about the effects of radiation on this valid system, this little universe here. We feel that regardless of how this power plant is operated we need to have those answers, because the pressure is now on to produce power from atomic energy at the lowest possible cost. The cheapest way to produce it, if we did not have the waste disposal problem, would be to dump it immediately into the river system and forget about it. If we could do that now, we could compete with all sources of power. However, the waste disposal problem and the separation and disposal of waste is one of the principal factors that is keeping up the cost of atomic power. As we tell physicists, ecologists and engineers, if we can solve this problem, we can be sure that within the next few decades nuclear power will make a contribution to our source of energy.

This question will be raised sooner or later, so I might as well dispose of it here. I say, why worry about the levels of radiation you get in these cases? We know it takes ten to the fourth or fifth roentgen to knock out some of these organisms. Who cares, then, because here you are going to keep the levels of radiation down to a very low rate.

For example, we gave this figure of one-tenth a moment ago. If you take one-tenth of the permissible dose rate of 0.3 rads per week, then we end up with a very small figure of .03 rads per week out here in the environment. It takes a very large amount of radiation to destroy these particular organisms.

We are not interested in just the elimination of certain essential components from the environment, but we would like as soon as possible to get detailed information on the doses required to interfere adversely with their function in this food web system. We do not know what this figure is. Let us leave that for the unknown at the moment. We do know it takes about 400 roentgens to give man a mid-lethal dose if it is hard gamma radiation over a short period of time, so here we have a factor of about 100. If we multiply this figure by 100, then we do know that we can kill some of the bacteria and fungi at 40,000 roentgen, so we might compare it with this 400 roentgen.

The next consideration is, how then can we get 4,000 roentgen here if we are going to have this dose rate? How can we get a dose rate there that will be a consideration when we are going to stay down to these very low levels?

Since we have a factor of a hundred between these, let us multiply this factor with a hundred, and we will come out, then, with a figure of 3 rads per week which might produce comparable changes in this system to the .003 here.

You still say there is a factor of 100 between them. We know under certain situations that certain of the elements in the environment selectively absorb and concentrate the radio-active material. So, you have a factor of ten to the third, or ten to the fourth, and then you see immediately that if you multiply by this ten to the third, then you are up in a region where you are very much concerned about the problem, where certain of these organisms might be knocked out completely or certainly their function can be interfered with, and that is our immediate goal, to learn something about these interrelationships, the factor of selection absorption, the long-time chronic effects of radiation on these enclosed environments.

In our small program, we are not able to study all types of systems, and we are beginning our efforts in studying the forest type universe, and Dr. Auerbach will discuss some of the details of our program.

DR. AUERBACH: As Dr. Morgan said, we are just starting as of this last September, 1956. In deciding what type of approach that we were going to use, we considered the various types of environments that were available wherein future power reactors would be placed, and we came to the conclusion that since our waste disposal studies are all concerned with using the weathered overburden--that is, the upper soil in terms of waste retention, we felt we should concentrate our efforts on the residuous forest floor. Because of limitations of time and power and all of that sort of thing, we decided to use a model that we know something about, a sort of model ecosystem, to quote Dr. Odum. This model ecosystem around which we are building our program is a small niche or micro-habitat or micro-environment, if you want to call it that, which we call a tree hole. I will outline it.

It is a decaying material found within the heart of a tree induced usually by some external agency producing an agency in the tree where fungi can begin, and you get a decaying out of the woody parts of the tree, and other arthropods move in which support a micro-community. In fact, this environment--we think it is suitable because it has a high population density which we have been following for a number of years. It is a relatively stable micro-environment changing seasonally, but not too much from day to day. It has a food web about which we hope to learn more.

The animal populations are organized in a pyramid of numbers. We do not have the information to construct a pyramid of biomass because the producer level--that is, the fungi feeding on the cellulose, the bacteria breaking down the cellulose are a big void in our knowledge, and we would like very much to get information along that line. Perhaps we can in the future. Because of its restrictions located within the tree and each species of trees seems to maintain at a given time the same population density, it reduces our sampling problem greatly and, as you all know, when you get out, one of the big problems in ecology is sampling--getting adequate samples. That is going to be the ecotope, or ecotype, I should say, or ecosystem that we propose to use, unless perhaps we see that we should make a change.

Our program is designed for two major phases which I want to outline.

The first phase is a lab phase in which we are, one, subtracting this substrait with its sustained orthopods, and we are limiting ourselves to that because that is all we have the manpower for; to conduct cobalt-60 gamma radiation for dosage level to determine differential specie levels, radiosensitivity, and then, in terms of immediate kill, how much it takes to kill them immediately, and whether there is a difference in species population based on that.

Secondly, we propose to subject these substraits to direct radiation and levels below probably these levels and put them way back into suitable containers and determine long-term delayed effects and see again what effects we can determine. Perhaps we have a problem here of determining the best biological parameter. We have been thinking in terms of numbers of individuals, but after listening to Miss Patrick today on the number of species probably this would be a good way of approaching it. We have been thinking also in terms of growth rates and numbers of individuals in the population.

The third step in our laboratory approach is to use tracers to work out as best we can the food web of this ecosystem--in other words, on a purely logical or purely ecological system, what is the food web? When we have accomplished this, we propose to go to a field phase in which we will mount cobalt slugs. Our people can mount cobalt slugs outside of these trees, give them a predetermined level of radiation for as long a period of time that we feel we should--a year, two years, five years--on enough trees, again, so that we can sample periodically and determine the effects.

Secondly, we propose to introduce into these ecosystems single fission products either in aqueous solution or in the type of solution that comes from the chemical processing plant which varies with what they are doing.

We will see two things -- if they are moved and concentrated through the food web and pass from one level to another, or simply if they have a toxic effect.

And, number three, since there is some evidence of synergistic effect, mixture of products again with the same product aims in mind.

Since the inception of this program, we have begun the direct irradiation of tree-hole mold. We have used six species of trees from various parts of the United States and subjected the samples or allotropes to dose ranges ranging from 15,000 roentgen to one million roentgen, and then the sample for immediate kill by placing these samples in Berlese's funnels to extract the surviving arthropods. We found, which correlates with existing laboratory studies, that it requires in excess of 50,000 roentgens to produce an overall appreciable mortality.

On a taxonomic basis, we do not have much information yet. We have not begun much taxonomic work yet. Even at these high levels, there were indications of species population differences.

One of the small common families of beetles found in the forest floor--feathered wing beetle--required dosages up to 200,000 roentgen to knock them out; whereas, a good many other things were knocked out much sooner.

Also a slight indication of some interesting differences is in the akarinus, which are the most abundant group and which make up over 85 percent of the faunistic population of the tree-hole substrait as they do in the forest floor. In the akarinus number, we found again on a hole basis, we were getting adults and young, the so-called hypophe of the akarinus.

Without going out too far on a limb, it has been an accepted proposition that the sensitivity of organisms to radiation is inversely related to their age and period of development. The younger are more sensitive than the adults. In terms of these particular studies, we found no difference in sensitivity between these young ones and the adults. It might also be assumed that the material, since it was in the funnel for five days, these eggs might have hatched and the young have been born in the interim. Again, however, these high levels apparently did not affect the akarinus eggs.

In addition to these studies on natural arthropod populations we are beginning a pilot study on earthworms. Earthworms are abundant again through the soil. They are important in soil structure and soil fertility, and one of the presumably important organisms in the soil is the earthworm. The pilot studies we have been doing have been strictly uptake experiments using strontium-89, one of the critical fission products. We have just completed a series of using a species of earthworms in which it was subjected to a range of soils which contained-- I don't know if this means much--400 counts per minute per gram per soil to 1,750 counts per minute per gram, which is well above background.

In this series of studies, these worms did not take up this particular isotope. Of course, this means that we still have a lot of ground to cover.

I think that about covers it.

DR. ROSS: Are there any questions of Dr. Morgan or Dr. Auerbach? Thank you very much.

The last paper this afternoon will be from the third largest production plant that we have. We have people from the Savannah River plant and Oak Ridge, and now from Hanford General Electric Company.

Dr. Pearce who has a very long title--Manager of the Biophysics Section of the Radiological Sciences Department of General Electric Company--Dr. Pearce.

DR. PEARCE: A geochemist, Ladies and Gentlemen, is seldom separated from his rocks, and I have brought two samples here which may be of some interest to you. I will have something to say about them later.

The dense one here is the more typical. The vessicular one is found frequently at the top of lava flows and, of course, our country out there has an abundant lava flow.

I would like to pass these around for you to look at.

My objective in this talk this afternoon is really two-fold: Firstly, to set the stage for my two associates, Dr. Foster and Dr. Hungate, who will speak tomorrow morning, and whose talks will be very much ecological, I am sure. Secondly, I want to give you a general picture of some of the problems that we have met in this broad field of waste disposal at Hanford during the last ten years or so.

I should like to recall to you first, of course, that the Hanford Works, of course, is a large plutonium producing manufacturing facility.

The problems that I will want to discuss really are threefold: First, the problems of waste disposal of very low-level liquids to the ground; secondly, the disposal of very low-level liquids directly to the river, the Columbia River; and, thirdly, the emission of certain materials from the stacks.

First, a word or two about the geography and climate. The Hanford operations office occupies an area of over 600 square miles in South Central Washington, lying between the Yakima River on the south, which enters into the Columbia at Richland, our residential community, the Rattlesnake Hills on the west and related ridges, and the Columbia River flowing this way (indicating), on the north and on the east.

Here we have the White Bluffs, an outcrop of a lake and river deposited semi-consolidated material which will show up in some of the photographs, I think, of Dr. Foster's tomorrow morning. Over here (indicating) in the Rattlesnake Hills, we have the outcrop of the bausalt rock that is being passed around, the lava flows. These are the Columbia River bausalts. They have a thickness 6,000 feet or more. They really never have been sounded, and, of course, this whole Northwest, for thousands of square miles, is underlaned by these flow rocks. At this particular point here (indicating), they outcrop in the form of an ante-fold. They then dip beneath our area, rising in Sable Mountain and in this region here (indicating) and here they form an anticline. They dip below the Columbia River and rise up here (indicating) again in the Saddle Mountain. The sale here of ten miles will give you the idea of the size of the area.

The reactors are located generally around the river to the north. The chemical separations plants are generally toward the center of the area. I will mention later, and others will mention, the old Hanford Town from which our plant gets its name, which is located in here (indicating). The towns of Kennewick and Pasco are here (indicating), and are the first points of withdrawal of water from the river for drinking water purposes.

(Slide) This gives us some idea of the grain size of the Hanford soil. This is the soil overlying the lava flow material that I referred to. Gravel, coarse sand, fine sand, silt, clay, a rather small clay fraction, but one which is very significant in the removal of radioactive materials from which any waste solution that we might put into the ground.

(Slide) The mineral composition is reflected here (indicating). All I will say about that here is that these soils in general are mechanical decomposition products of these volcanic flow rights and of these materials upstream so there is some quartz and related materials from the more granitic rocks to the north.

We have done a good amount of work in recent years in the laboratory with the average soil or, in many cases, with the specific soil in which we were interested with the individual radio isotopes and with mixtures of them.

In the case of ruthenium-106, which is an important product, when this is used, we find there is ready adsorption readily over a wide range. When we go to process wastes, we find there is little or no absorption. In the case of plutonium, we find that plutonium is nicely fixed by the soil over a wide range, very quickly absorbed as much as three and a half milligrams of the stuff taken from volume. It is fixed very nicely. There are influences, however, on the retention of plutonium. The adsorption in the first place and the retention from salts. However, sodium salts and strontium or salts containing cation like ion, have very little effect on this plutonium retention. However, salts of aluminum or salts with a cation of that general type seem to have an effect and do cause an increase in the retention or an increased removal after the material is already fixed on the soil.

There are important influences of complexing anions due to anions complexes with negative rather than positive charge. These materials do mobilize plutonium or prevent its adsorption on the soil.

In talking about adsorption and in using that term, let me say that I mean both physical adsorption with all that that implies and also the chemical adsorption, the ion type of thing.

In our particular type of thing, the ion exchange phenomena are of more importance than the physical type, but both do pertain. With the ion exchange, you know that the ion under study enters into the crystal lattice replacing an ion from that lattice which goes on its merry way.

The influence of the charge of the ion and the size of the ion and the concentration of the ion and the concentration of the hydrogen ion, which can compete, the concentration of other foreign ions and so forth -- all of these ions come into the picture.

(Slide) This is plutonium showing some laboratory results where we have percent removal plotted against the increased concentration of plutonium, the increments of plutonium, and we find over a wide PH range here, we have an essentially complete removal of the soil. There is a sharp break-off here with the low PH's due undoubtedly to the competition with the hydrogen ion, and up in here (indicating) we have a fall-off not completely understood, but undoubtedly it is due, among other things, to the formation of various types of anion complexes.

We have also studied cerium-144, which illustrates several points which have been brought up during the day. The plus 3 and plus 4 valences state different immediate adsorption, again the wide PH range, again little affect on salts.

In the case of caesium, strontium, and yttrium, the increased adsorption increased in increased adsorption on the soil. There are various effects here. Some of them are shown. Sodium salts do have an effect and low PH will remove the ions significantly after they are deposited.

Here is an example of how the sodium nitrate concentration does actually enter into the picture. (Indicating) This is a case of caesium which indicates that it is lower than in the case of the high salt content solutions, so we do have to watch out for this effect and in the disposal of any kinds of liquid waste to the ground.

I will point out right here that our concentrated fission product solutions--and we do have lots of fission products out there, of course--these are stored in large underground tanks in much the same way as they are at Oak Ridge. In fact, I guess Oak Ridge taught us how to do that in the early days of the Manhattan District, but only lower level materials are considered for disposal to the ground. I will expand on that thought a littl later.

Here is something about strontium-90. This is interesting because it shows what soil can do for us. If we have a changed capacity in the soil of 500 mil equivalence per gram and we use only .001 of it, with a cubic foot of soil weighing 4.5 by 10 to the fourth. . .

A soil column by one foot by one foot by 200 foot long, and we do have a very deep water table at Hanford which would be able to take care of 4,000 curies of strontium-90. This is a very significant thing. I think we oftentimes overlook the extreme ability of nature to take care of some of these situations.

Here is a picture of a situation that might exist around the crib.

I might point out that we have various types of waste disposal facilities with very low level, essentially in active water like a condenser water, or something of that kind, we might dispose of it by what we might call a swamp. These might be acres in size and these themselves are very interesting things, or at least their effect on the ground water is very interesting in that they cause bunks on the ground water table. We call these ground water mounds. Underneath one of these very few swamps that we have now, we have a ground water mound, the elevation of it being established by wells of about 60 feet or so above the surrounding level of the ground water. As you might imagine, this causes our hydrologists some very perplexing nights, because, you know, when you pour water down on a hill, the water runs down the hill, but if you pour water down on top of a water hill, what happens? Well, it is very complex; I am not sure I understand it all myself, but it is a very interesting thing, and you might think about that.

Swamps, then, as one means. Caverns are essentially enclosed swamps. Artifically repaired as to lining material, sand and gravel, sized for the percolation rate of the water down into the soil and down to the ground water table.

Cribs, such as we have here--a crib is a relatively small structure and perhaps half the size of this room. It is a timbered box, if you like, into which a waste line might lead.

Now, if we do not know the geology and the soil science pertaining to the material around about the site of this proposed crib, we then drill wells to inform ourselves about this. The wells serve to give us samples of what we have there, which we may want to later use for ion exchange experimental work. These wells also fill with water and serve as a fine sampling point later on and, in general, we establish for ourselves the rule that we must have these monitoring wells drilled about the near neighborhood of a crib site. That is a preamble to these colors.

We might find, then, that this would be the plutonium held by the soil very close and around about the point of disposal. (Indicating) This could be strontium and that is about the right place for it. This could be rare earths and yttrium and related materials. (Indicating) This could be caesium, for example; this could be the ruthenium which, as I pointed out, is very poorly retained by the soil. Fortunately, it is not too bad an actor, and this could be the nitrate ion as an example, or the sodium ion which moves on ahead.

The appearance of these various radio isotopes in our monitoring wells would serve, of course, as a warning that the material is moving. Essentially, you see, we have a soil column in the application of this method of waste disposal.

We have for ourselves the rule that we do not contaminate the ground water with radio isotope of half life longer than three years. This, along with our data from the rate of underground water movement itself, which is rather variable, gives us, we feel, an ample safety factor particularly since one is ignoring in this the fact that the soil under the ground water is also, and equally so, a cation exchange material and just the matter of fact that you have a little bit in the ground water, although we are rather particular about the ground water, is not any indication that the ion is free and is going to go to the river or elsewhere.

I think this is a very important point.

One other point I want to make there--here is a case where contamination around the crib actually occurred. In 1948, we put in this particular crib and drilled eight wells around about it. In those days, we were still finding out the fundamentals of this business, and we were quite prodigal at that time with our wells. We don't get that many wells nowadays around the crib. We don't need them. Four of them were ten feet away and four of them were twenty feet away. They were all drilled to a depth of 150 feet.

Skipping 1951 and going to 1952, we discontinued the use of this particular crib as a disposal point when we found from a neighboring well that there was an indication of contamination with ruthenium. At that time, we had put in something on the order of ten to the sixth valence, a total of about 700 curies of mixed fission products containing quite high sodium nitrate. We calculated from this that contamination of the water table had reached to the water table back in 1951.

In 1953, we deepened one of these wells to 250 feet, leaving the ground water at 240 feet, and that was the point I think I was trying to recall a moment ago--the ground water table is very deep here (indicating). At that time, we detected ten to the minus six and five times ten to the minus nine in uranium in a
 ?   ?
lily letter. By this year, these two elements had come up and in concentration to two times ten to the minus five and eight times ten to the minus nine and spread from the small original value to an area of 1,000 by 1500 feet, giving you an approximate idea of the rate at which such material will spread.

Another point I wanted to mention before leaving this particular topic of the underground waste disposal is the point of rate of movement of the under ground water. This varies a great deal over this very large area. As I pointed out earlier, the disposal points of these chemical processing plant wastes are toward the central part of the area. We do have some areas of very rapid flow of underground water, particularly north and south of that Gable Mountain structure, the anticline structure towards the middle of the area that I pointed out. These two underground water streams, if you like, and there may be others in the area, are due to the origin of the sedimentary materials, the sands and gravels.

I pointed out that these were water-deposited largely, some due to deposition on the bed of an old lake, and others due to the deposition caused by the wandering of the Columbia River across this area (indicating) at about the same time in geological history.

So, there are many questions that come into this question of ground disposal of the liquid waste. We treat each proposition on its own merits. We discourage mere dilution in order to meet a specification. We strongly encourage the removal of all possible radioactive material from the solution before we think about putting it into the ground. I repeat that we are talking now only about low level activity materials in the first place. The high level materials are stored.

Turning now from the question of ground disposal to questions of Columbia River disposal, we have the reactors along the river, of course, because it is the cold water of the Columbia River which we use for pile cooling, reactor cooling. This water is put back into the river after it has been held for a few hours to cool down radioactive materials mainly, but also thermally to some extent, since it comes out hot before it is put back into the river.

Here we have some very interesting problems. (Slide) Here we have the flow rate of the Columbia River at Hanford. You can probably read those numbers better than I can, but during the month of April, May, and so on, over to about August, we have a terrifically increased flow rate--very many more thousands of gallons per second with a greatly increased current, of course, and, therefore, a greatly decreased travel time from the various reactor effluent lines to the point of first use at Pasco downstream.

(Slide) The United States Public Health Service worked at our place a few years ago from 1951 to 1953 and they have recently published their report on their study of the Columbia River. We do a great deal of this work, a great deal of sampling up and down the river, all the way down to Portland and it is done on a definite schedule. These data, however, are from the Public Health Service. These show the activity density in units of microcuries per milliliter found at Station No. 362 which is the site of the old Hanford plant that I pointed out on the map. It is very well upstream before the intake to the Pasco filter station. The total beta activity density was in the range of three to eight times ten to the minus six, and you can convert that and these numbers here also are percentages. You can convert these numbers by means of Dr. Morgan's equation to the rads per week, if you wish.

We talk more about MPC--maximum permissible concentration--I think, for this kind of thing, more so than we do for the rad expression. I can give you those MPC's and maybe I should for radio isotopes. We restrict ourselves, as they do at Oak Ridge, to one-tenth of the handbook, 52 MPC values for the off-site use of contaminated water, if you can call it contaminated water.

For example, in the case of sodium-24, the National Bureau of Standards Handbook No. 52 value is ten to the minus three microcuries per milliliter for drinking water. We would get extremely alarmed if we came up near to ten to the minus four, then, applying this extra ten percent. For arsenic-76, the Handbook 52 value is ten to the minus one microcuries per milliliter. For copper-64, which is rather abundant as a radio isotope at this station No. 262, the MPC handbook value is ten to the minus two, and so on. Magnesium-56, which I think will show up in some of Dr. Foster's river data tomorrow, has decayed at this point to very much smaller values than it would have at the fall-out of a reactor effluent line.

Finally, one other point, the third point that I wanted to mention-- the emission of radioactive materials from stacks. Of course, in a plant such as this, we have extremely tight specifications. Our ventilation is terrific. It is a terrific problem, but it is pretty well worked out so that we can talk about 99.99 percent efficiency of removal of particular material from an error in the plant.

Nevertheless, there are certain materials--fission products-- which are rather hard to catch from an error. Iodine-131 is one of those. We do have small emissions from the stacks of the separations plant. Occasionally other fission products also in addition to iodine-131, but if everything is going well in the plant, we find that we can hold down very nicely the iodine-131 emission to one curie per day. The resulting concentration of iodine-131 on vegetation generally averaging less than five times ten to the minus six microcuries per gram in the immediate environ... of the plant area is well below the permissible amount.

Finally, I should point out that there is a very important role played by the sampling and monitoring team in checking on the environments in cases such as this. We depend a great deal on these people and on the radio analysis laboratory which routinely analyses many samples per month and the statisticians, of course, since these are all statistical problems. It is our aim eventually to have automatic analyzing equipment, sampling and analyzing equipment on every waste stream whether it leads to the ground, to the river, or to the air.

When you have as many variables as we have, this is a difficult problem, and we have chemists and engineers and electronics people and physicists, and so on, working in these fields. I think we have made noticeable if not notable progress in the field of automatically monitoring waste streams and we plan to do very much better. Thank you very much.

DR. BOSS: I believe we have time now for some questions.

FROM THE FLOOR: How did the permissible minimum doses get established? Is that the minimum permissible for humans?

DR. PEARCE: Dr. Morgan is the national authority and also the international authority in this very field, and I am very happy that Dr. Morgan is seated right behind you, sir.

DR. MORGAN: I am not the authority on it. I just happen to be the Chairman of the two committees.

The maximum permissible values are based on two criteria--one, that the amount taken into the body by air or water cannot exceed that amount which would give any organ of the body a dose rate in excess of .3 grams per week.

The other is, if it is an element that behaves somewhat like radium, the integrated dose to the skeleton must not exceed that dose given .1 of microgram per radium.

In this country, the main committee that determines permissible exposure is the National Radiation Commission; Dr. Taylor of the Bureau of Standards is the Chairman. The other important committee is the International Commission on Radiological Detection. Dr. Bates is the Secretary. It is the human being on which it is based, but, in most cases, the biological data is obtained from animal cases. In some cases, we have to make extrapolations from one case to another. Many of the biological half-lives that are used are determined by finding out the equilibrium condition of stated elements.

DR. ODUM: I suppose it is up to ecologists to figure out minimum permissible doses for a community in the future.

DR. MORGAN: We make no claim for that factor of .1. As I mentioned a moment ago, if you have a factor of selected adsorption of some of these organisms tenths to the minus three or fourth, then the factor is not sufficient.

FROM THE FLOOR: Is that table of active water at old Hanford taken to the point of below discharge?

DR. PEARCE: Yes, sir, below the lowest point, most downstream point of discharge. It gets rather complicated to calculate these things because you have several points of entrance; some reactors might be going great guns at that period, and you have the variable flow rate, and that sort of thing.

DR. ODUM: What is the chance of accidents in these plants, such as someone turning on the wrong valve?

DR. PEARCE: There are safeguards upon safeguards piled on those.

DR. ODUM: When the power plants become interested in making money, you may have some difficulties.

FROM THE FLOOR: Perhaps someone should look into that.

DR. ODUM: I think we need to start training some young people who know something about radiation and ecology. There should be good money in this because they ought to pay these boys good money.

DR. PEARCE: There are lots of challenges in the field.

MISS PATRICK: Do you feel that this discharging into deep wells---I suppose you can't answer this because this may be too general a question--but the thought occurred to me that these shallow basins which are used versus this deep well discharge--are there any problems there?

DR. PEARCE: I think you should try to keep away from contaminating the gound and you should use as much of the soil column as you can. I would say a high point of disposal in the overburden above the ground water would be desirable, but there are so many factors that come into this whole question that you have to get pretty well to the point of considering each waste on its own merits. It depends on what kind of soil you have; it depends on your climatic conditions, of course. We are fortunate in that our rainfall averages abut seven inches a year, and we have a very deep water table; therefore, a very logical soil column available. We prefer not to put it down too close to the water table. There is another factor that comes in there, too. We do not find it acting, and that would be the rise of material from the point of disposal to the surface by capillary action. We have watched for that very carefully, but we do put our cribs down, usually 20 or 25 foot, something like that, and the soil science people tell us that capillary action is not likely to act that far down. It will act down there, but it won't pull the stuff up to the surface.

DR. BILLINGS: Do you have any pheodophytes in that region that might actually have roots down in the region?

DR. PEARCE: Our vegetation is sage brush and thistle, small desert grasses.

DR. BILLINGS: You don't have grease wood?

DR. PEARCE: No, no grease wood that I know of.

DR. BILLINGS: I imagine you have those up-cut plies, if there are any plies in there, so that you wouldn't get deep-rooted phreatophytes that would have the roots down where these wastes were moving down toward the water table. That might bring it up.

DR. PEARCE: Around about the point of disposal we keep the area pretty clean. We take the vegetation off and try to keep it off. I do not think we would run into that effect. I do not know whether you would get 20-foot roots or not.

DR. BILLINGS: With some of those deeper soils, perhaps you would. Grease wood would certainly be related to the water table. It would be closer to the table than that.

I would think the topographic locations of these cribs would be very important, particularly in desert country where, in some basins, the water table does get close to the surface and you do get evaporation and you leave the salts behind.

DR. PEARCE: The water table where we allow disposition of those materials into a crib would be several hundred feet down.

DR. BILLINGS: Did the ability of the soil to absorb these materials decrease in time?

DR. PEARCE: Decrease with time.

FROM THE FLOOR: Do you reach the saturation point where-- (inaudible)

DR. PEARCE: No, I wouldn't think so unless you got some sort of breakdown of the lattice itself. I cannot see any conditions which would cause that.

Do you recall my colored picture around the crib there? I should have mentioned that there was this mobile situation, and it is not static. As you put in more stuff at the crib, these various layers shift down, but there is no evidence at all of the lattice breaking down sufficient to cause a change in the capacity. When you are using an ion exchange resin in a laboratory, yes, I think there is evidence. We do not do much of that in the laboratory, but I think you can find evidence of the breakdown of the exchange resins. I imagine if you load them up with some highly radioactive materials, you might get a breakdown but in the inorganic materials, no, I do not think that has ever been shown.

MISS PATRICK: This does not relate exactly to Dr. Pearce's paper, but it is a general question about radioactivity.

We have done some experiments at the Academy with radioactive zinc on fish. We have found that at first the mucus and the muscle are very radioactive. It then moves into the kidneys and I do not know whether the pancreas is involved or not, but we know it moves into the kidneys and reproductive areas, and then, after about a month and a half, it moves back up into the blood system. Has anyone else ever found anything like that?

FROM THE FLOOR: I do not know the details, but there has been work done on zinc up at Chalk River. I do not know who is working on that. There have been several papers published on that. To my knowledge, they have not worked with fish. That is, I do not recall that they have.

I know they did not get concentrations in the testes and reproductive organs. I do not remember the details, but it would be worth looking up.

DR. BOSS: You can write also to Dr. Walter Chipman Rice at his laboratory; they have been working, or have worked, with zinc. I think he has worked with the lower invertebrates. He has worked with plants, and they were going to take it further than the invertebrates. I do not know if they got into the fish with radio zinc.

The Japanese are particularly interested in the transfer of zinc. They have done quite a bit of work on it and they are pushing it quite hard. John, do you have anything to add to the question that Miss Patrick raised?

DR. HARLEY: Their interest in that is continuing.

DR. ODUM: Are they using it in zinc?

DR. HARLEY: They reported in their literature that they found some after the last test series, around Nagasolo particularly.

DR. BOSS: It just happened that Dr. Person and I had dinner with Dr. Nagasolo, a pharmacologist. We have his data on the amounts of zinc in the different parts of the organism of fish, if you care to look at this. They are interested in and they have found evidences of it so he has a paper which is just about ready for publication, and he gave us the manuscript last night.

MISS PATRICK: We talked about this because we wanted to find out whether it was accumulated in fish. Two of . . . at the Hahneman Hospital in Philadelphia. They are taking this course there and the Professor told them, "You can do a research problem," and they did this research problem with zinc, and they are still going on it, and they are wondering where it is going to go next.

DR. BOSS: I gave a talk at the University and five of the graduate students came up and started asking a lot of questions about it, and they have an ambitious program out there in it.

MISS PATRICK: Would you be interested in the results of this?

DR. BOSS: Yes, I certainly would. We still have a few minutes. Dr. Sears, I understand that you have to leave tomorrow mid-morning. As we have said, we are open to constructive criticism or suggestion. The rest of your group will have that opportunity, but so that we will have your thinking on this, we would certainly welcome any of your discussion right now.

DR. SEARS: I do not have much to add. I came in on this cold. I learned a tremendous lot of things. I am impressed with the general picture and the fact that we have not yet solved the problem of adequate disposal of the by-products of our conventional source. It is still a grave problem in places like Los Angeles and elsewhere.

As early as 1891 the Germans were taking adequate stock of all of their industrial wastes in places like Essen.

There is one other thing on which I would like to express an opinion. A very fine statement of fundamental principles has been made, I think, and I think a little analogy might be helpful.

You are concerned with avoiding or dealing with pathology of the landscape and that sort of thing, and you might take a little lesson, I think, from medicine. Medicine knew a great deal about disease and pathologic conditions before it began to understand the human body. It was not until it understood what was normal that it started to move ahead. I think that is a justification for intensive work on normal communities.

DR. DOSS: Thank you very much, Dr. Sears.

I believe you realize that we have brought together some of the representative groups of people who are taking part in the testing activities that are on the different large production plants, and it is a cross section of the people who are working with us and many others, but we could not bring them all in for obvious reasons, especially time.

Tomorrow morning, we are going to have a very stimulating program. It is in marine biology, especially Dr. Krumholz, Dr. Foster and the rest, and also we knew that Dr. Odum would bring this problem of young people up, so we have Dr. Shoup on the program to answer Dr. Odum's question involving young people, so we hope we have covered some of the questions.

We have two minutes, and if you will bear with me, so there won't be any confusion and no one will be left out, I will give you the details on transportation facilities this evening.

. . . Announcements . . .

(Whereupon, at 5:00 p.m., the Convention was recessed to reconvene at 9:00 a.m., Saturday, May 21, 1955.)

UNITED STATES ATOMIC ENERGY COMMISSION

TRANSCRIPT OF PROCEEDINGS

In the Matter of:

ECOLOGY CONFERENCE

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UNITED STATES ATOMIC ENERGY COMMISSION

ECOLOGY CONVENTION

Saturday Morning Session

May 21, 1955

SATURDAY MORNING SESSION

. . . The Saturday Morning Session convened at nine o'clock, Dr. Paul D. Pearson, presiding. . .

DR. PEARSON: Gentlemen, shall we come to order.

Our first paper this morning will be given by Dr. Louis A. Krumholz, of the Lerner Marine Laboratory, and the title of his paper is "Ecological Studies of White Oak Lake."

. . . Beginning portion of Dr. Krumholz' not recorded. .

DR. KRUMHOLZ: There has been a lot of hemming and hawing in the literature back and forth with respect to the validity of making a population estimate and marking recapture. I think from the experience here at White Oak Lake and elsewhere, it does give you good qualitative information under most circumstances, but the quantative data must be accumulated in very special ways, depending upon the kind of fish you are dealing with and depending upon the terrain with which you are dealing.

When I speak of the terrain, I speak of the bottom and White Oak Lake was pretty much something like these (indicating) all over the bottom. There were sticks and brush. The layer was not cleared at all. As the trees died, they fell over, and it was really -- I wouldn't say simple to set a net or do any sanding, or anything like that, in the Lake -- but after we gathered all of this information, the problem came up of preparing some kind of final report. We tried to get everything into one report. It was written up in three parts. This is a little note we handed the AEC, one part of that introduction being on botany, one part limnology and one part vertebrates. We included all of the other vertebrates included in the Lake except the ordinary birds. We did not do anything on those, because we did not have the time or the people.

We did a little work, however, on amphibians, reptiles, mostly turtles, of which there is an abundance at White Oak Lake. We also did some work on water fowl which is being continued, and we did quite a bit on fish because that is what we went there for in the first place.

Some of the results I can give you, I think, are pretty good evidence that the radiation accumulated to which the fish were exposed in White Oak Lake showed there were harmful effects. In the first place, from the best of our information which we could get from other Tennessee Valley lakes, the fish grew at a rate of about 25 to 30 percent more slowly than those in the surrounding lakes.

Furthermore, those species of fish had a life span of 30 to 40 percent, which was a shorter life. We must keep in mind that White Oak Lake was much more fertile than the other lakes, so the fish probably should have grown faster although they may not have lived longer, because there is some question as to whether a fast-living fish will live as long as a slow-living fish, and so on, and I believe there is some truth in that. By the same token, fish that grow -- real fish -- get large in a short time, and they are the ones that we consider the fast-living fish. The ones that reach the same relative size in a matter of a year longer are pretty much of a slow-living fish. These fish grew more slowly and also did not live as long, which is good circumstantial evidence to me. Based upon the amount of radiation they received, there was good circumstantial evidence of deleterious effects from the radiation. I do not believe that the deleterious effects could have been traced to chemical waste dumped into the Lake. Somebody will probably ask that question, and they will probably want to know why I do not believe it. Well, it is an accumulation of observations made over the years while we were there. Usually, when anything like that was dumped in a lake, it was not dumped in in any quantities. It was not a dribble that would come in all of the time. Somebody would hold the plug and we might have something come down that creek that was poisonous. We had several minor kills in the upper lake due to things like that which we traced out as being due to that.

I do not believe that the disappearance of some of the fish from the population was a direct result of these kills. I do not believe it was turbidity because the Lake was turbid as we were there, and it gradually cleared up and proceeded to emulse the area.

Based upon our population studies, we found the black crappie , the bluegill, the large mouth bass, the carp, the shad and the bullhead were doing what we might call reasonably well.

They maintained their populations from year to year and season to season, and everything looked to be pretty much all right, but they seemed to be growing more slowly and not living as long.

In the white horse and crappie , we had pretty good populations of both of those when we went in there in 1950. When we killed the lake in 1953, we got 37 red horse out of the lake and didn't get a single white crappie. We had taken white crappies in the net six weeks before we poisoned it, so those two fish had disappeared from the population, or we may have missed a fish or two because we handled 250,000. We checked all of these fish for marks which we had given them in population study so that we could get some idea of the completeness of our study. That is, from the marked fish, we get some idea of the total pickup on the job.

We figured we got 60 percent of the marked fish. That holds true for all of the fish but carp, because the carp had some kind of a disease. We do not know what it was. We tried to isolate something from the ulceration and we couldn't, and we didn't have time nor facilities to follow this thing up like we should have, because time was drawing to a close and we just couldn't work beyond the first of July. So, we had to close up shop. These carp had some kind of a disease. The ulcer would form as a hole in the scale and then it would invade the skin and the muscle tissue right on down to the bone. At times, you could pick up a carp out of the net. He would still be alive but he would have this ulcerous lesion anywhere from an ince to an inch and a half in diameter. You could count the ribs. They would go into the nets like that and they would die off, so we know our estimation of the carp in the spring of 1953 was no good.

For the recovery of carp, we have no good guess because most of the fish we had an opportunity to mark could have died before we treated them.

The white crappie and the red horse we are quite certain disappeared from the population during our tenure, and also when we got there in 1950 there were no minnows in the lake except the carp and the goldfish.

When White Oak Lake was built, Watts Bar Reservoir had a small embayment. When they raised the dam and put in new culvert in there and embayed or impounded White Oak Lake, they did not bother to clean out anything there where they put in the dam and they put the fish inside. That is an ideal habitat for many of the small minnows which occur in the Watts Bar Reservoir, and just downstream there are a good many of those small minnows, but in White Oak Lake, when we got there in 1950, there were no minnows at all. In fact, in 1949 when the Tennessee Valley authority had gone over there at the request of the Commission and made surveys of the lake they, too, found no minnows as early as 1949.

In other words, there is circumstancial evidence that minnows had disappeared from the population within six years after the lake was impounded, certainly within seven years after it was impounded -- because they just spot-tested the thing when they were over there in 1949.

I think they were there in October of 1948, but I am not sure that they did any of this there at that time.

Based on the figures that I have from the Health Physics Division, the fish in White Oak Lake each received from external radiation an average of about 1.1 rep per week, which is a pretty good figure that means he got the equivalent of one roentgen of radiation from external radiation. In addition to that, based on our work on the accumulation of radioactivity in the tissue, we found that the average crappie or bluegill in the summer of 1952 carried about one microcurie of strontium inside the body. In addition to that, there was the small amount of radioactivity from caesium, cerium, burrs, and so on.

The point I want to make is that these fish were carrying a total body burden of more than a microcurie and, at the same time, they were getting constant low-level radiation to the extent of about 1.1 rep per week during their lifetime, so if they would be real conservative and say the internal radiation, which I have no way of measuring and I am just guessing, if we were to take that and say that that was ten times the external radiation, I would say that is a fair guess. (Chalk demonstration) Say this is ten times that (indicating); that woul be reps per year or 50-R per year from the external stuff -- 500-R from the internal. There is a pretty good dose when you consider the fish lived to be about two years old. I do not think this is an exhorbitant figure, but if we get to the point where we cut it in two, we have 750 to a thousand hours over a life span, which is a pretty good dose, and I think that is because the fish, to grow more slowly -- I think it is what shortened their life span and I am not at all sure but what it is what caused the white crappie and the red horse to disappear from the population.

There is one other thing I would like to say before I quit. If all of you have copies of this, I would like to have you open to page 39, because here we have this picture of the seasonal accumulation of radioactive materials in the fish. That is a story pretty much in itself in that when we first started out in the fall of 1950, we found that the fish were carrying a reasonable amount of radioactivity in the tissues.

The following summer, they had doubled or tripled and immediately we thought here was a thermal animal and it would change its metabolic rate as the temperature of the environment changes, so let us see if he does not have a seasonal variation in the accumulation of radiation materials. We cut up three black crappies and three bluegills each week from late in August of 1951 until early in the spring of 1953. I do not remember what the dates were, but it was sometime in February or

March that we stopped. In that, we took those individual counts and lumped them together by weeks, and then treated these weekly averages by a moving average of five to smooth out the curves, and we got curves that are shown in that book.

(Indicating) This is April and this is October. We have a temperature curve -- I will call this 55, but the temperatu[illegible] curve of the water in White Oak Lake races gradually from April -- something like that -- it reaches a high of 80 degrees and then back down to 55 degrees, sometime in October.

At the same time, when this water reaches a temperature of around 55 degrees, the radioactivity of the fish is running about 3,000 counts per minute per gram. As soon as the temperature gets to 55 degrees, this thing starts to rise very gradually (indicating), and it will triple itself by the time it gets up here to the maximum temperature from 3,000 to 9,000. These figures do not mean anything, so there is no point in copying them down, except that this triples while this does not even double itself. As soon as it reaches this peak of greatest magnitude in the middle of the summer, when the temperature is at its peak, and you must remember this is one year, then the thing breaks off very sharply and comes down and makes this sort of a curve on the far side.

Now, the question arises, why is it that this fish does not continue to accumulate radioactivity at a temperature which is shown to be optimum or near optimum during the early part of the season, but it just quits accumulating radioactivity and, by the time October comes around, he may come back and make a small accumulation that year, and then continue on through the winter at a low level. It is my guess that at this time the fish here hit a period of summer dormancy in which the physiological processes change quite markedly, and I think that that will explain, too, a lot of this business as to why the fish do not bite so good in August, but yet they do in June. There is a lot of reason for making this one assumption. The fact that they could get rid of the radioactivity is a problem all in itself. So far as I know, it is the only fish that can get rid of strontium out of the bone just like that. He has this terrific burden of a microcurie. He can cut it down to a third in a month.

The physiology of that fish bone is a tremendous problem in itself. There are a good many ways, but I won't discuss any of them. I thought about it a good deal, but it would take a lot of controlled experiments and you would have to have a pretty good physiologist and you would have to look at the bone.

Before I quit, I will mention just a word about the water fowl in that we found that water fowl in White Oak Lake were accumulating quite large amounts of radio phosphorous in the breast muscles and would probably also be tri-phosphate.

These are primarly bottom feeders. They carry a total body burden of 5 to 6 microcuries. I think we had one up to

10 microcuries in total body burden. We dissected the animal, adding up all the tissues and trying to get a hundred. This is in the record and will be out in another report where we have all of the tissues in the body weighed and the percentage count position in total weight of the fish. That is how we arrived at our total body burdens. We take a sample and by using that against the average of the total weight of that particular tissue in the body weight, then we can arrive at what the total body burden was for that particular tissue.

Thank you.

DR. PEARSON: Dr. Krumholz' paper is now open for discussion.

DR. ODUM: Why wouldn't your decline in radioactivity--why didn't the fish stop growing?

DR. KRUMHOLZ: I can't stick my neck out that far, because there was no marked damage to the fish. However, the fish were growing more slowly.

DR. ODUM: This slow-down in growth will slow down the metabolism.

DR. PEARSON: No, I think there is much better evidence. We know that fish become lethargic when the temperature rises. The fishing is no good in the middle of August. I do not believe they metabolize the stuff. The fact that there is sluff off, if they get rid of the radio strontium --

DR. ODUM: You are postulating something you don't know.

DR. BILLINGS: With respect to this 35-acre lake, you point out that this is 38,000 fish per acre, or 1,000 pounds of fish for every acre. I should think this would make it evident as to why these grow slower. He says here that the slower -- excuse me for quoting me --

DR. KRUMHOLZ: Excuse me, but he knows more about this thing than I do.

DR. BILLINGS: It says here, "The slower growth rate among the fish of White Oak Lake may have resulted from over-crowding."

DR. KRUMHOLZ: I do not think there was overcrowding. That is one of those things that I do not agree with that I said. I said that two years ago.

DR. BILLINGS: On page 26, the second paragraph here, you said: "Thus, with the exception of the foreshortened life span, and perhaps the slower growth rate, the fish population of White Oak Lake is to be believed essentially the same as that of any comparable body of water in the region.

Do you want to comment on that as of a year ago?

DR. KRUMHOLZ: I said that a year ago. That is not true in the summary. This is an abstract. This is not your preliminary report. This is the abstract of the big report that Sam sent you. It is an abstract of this report. It is about 34 pages long.

I said a lot of things in this -- six and a half pounds chilled up there -- that was said without having a chance to look at any literature. The AEC wanted this report within three months after the work was finished, and I told them I would give it to them when I got it written and I would work on it, and it took about six or nine months to do it and get this stuff all compiled. We had not even compiled data in 1953. I did not have a chance to look at any literature. I may have gone off half-cocked on a good many things I said in that report.

DR. BOSS: Don't you think the 11,000 conditioned your thinking to say overcrowding?

DR. KRUMHOLZ: No, I have seen that time and again.

DR. BOSS: I know we find anything like that at all in Wisconsin. We open up the fishing season and let the people take whatever they can get because you simply do not get the growth of fish.

DR. PEARSON: This is the fishing season. How about that, Dr. Curtis?

DR. KRUMHOLZ: Of that 11,000 fish, 60 percent of them are gizard shad about four inches long.

DR. PEARSON: They still have to eat.

DR. KRUMHOLZ: They can only eat as much food as there is. When the food is there, the lake is tremendously fertile. The population will supply itself with the food that is there. The population won't get anything bigger.

DR. ODUM: You should not get as high a growth rate in a southern species as compared with what you would get in the north.

MISS PATRICK: In line with that line of thinking, we took channel bass from Pennsylvania and Georgia and tested them as to temperature tolerance and there was no difference.

FROM THE FLOOR: On this productivity, out in Washington where we think we have pretty good trout growing, we figure if a lake produces about 300 pounds per acre, this is very nice because this is trout water. The classical example of tremendous production is the term of carp in the orient; some of the milk fish in the Philippine Islands can be considered. Those poundage rates go up on the order of 1,000 or 1500 per acre where you get a short cycle. I would say your 1,000 pounds is really phenominal.

DR. KRUMHOLZ: We got that in ordinary Illinois lakes.

DR. ODUM: But you deal with trout.

FROM THE FLOOR: I am saying 300 pounds of trout --

DR. ODUM: That is equal to more than a thousand pounds of carp.

FROM THE FLOOR: With respect to this toxicity, I hope you won't be unhappy if I take exception to the statement that the chemical toxicity here may be a real factor. I think there is probably a greater species difference among fish for chemical toxicity than we would normally expect from radiation damage through the animal kingdom vertebrates. This LD-50 ranges are not really too great in orders of magnitude.

(Indicating) Here is the figure that I wanted-- of the 391,000 fish, 324,000 of them were gizzard shad. Of that 324,000, there were 322,000 of them that were less than four inches long.

When you got 300,000 of these things, you got a lot of weight. They were counted by the bucketsful. We measured 20 buckets of them. After about the fifth day of this thing, you would go along with us. (Laughter)

There were ten tons of small gizzard shad in the lake -- 20,000 pounds. Those little fellows were about four to five inches long. That was all one-year class.

FROM THE FLOOR: It seems to me this might be another good place to go back to Frank Lowman's talk on the rats at Eniwetok where essentially the population was killed off. A very large percentage of them were killed off by radiation damage, or they should have been, and very rapidly came back to a full population density under radiation levels which were terrifically higher than Louis got.

DR. KRUMHOLZ: Let me quote a Foster, et al, 1948 or 1949 in growth. Dr. Foster found that when Greenville trout were given 100-R of X-ray, there was a slight damage. There was quite noticeable damage at 200-R and there was considerable damage noted at 500-R.

Now, let me quote Mr. Kurt Sternmoss -- a fair to middling genetic system. He says that it does not make any difference whether they got this radiation as one shot or spread over a long period of time. You will have the same effect on the population if you radiate -- say, you have a population of 100,000; If you radiate 1,000 of that population with a 1000-R, or you spread it over the entire population, you will get the same overall effect.

FROM THE FLOOR: In one case, you are talking about physiological damage and in another case you are trying to compare doses.

DR. KRUMHOLZ: I am not comparing it on the growth rate. Yes, it is a physiological effect, but that is what he found.

I think if you give a fish 500-R or 600-R of radiation in a year, you can get good physiological effects.

FROM THE FLOOR: In many cases, that has not been found.

DR. KRUMHOLZ: We did not have controlled experiments. I am guessing at this. I will go along with what Dr. Dunning said yesterday.

FROM THE FLOOR: I think this is possibly and certainly something that conceivably could be. It is different from the results that have been observed in other populations where there is a continuing dosage that is picked up and there does not seem to be any kind of comparable damage.

DR. KRUMHOLZ: That is true, but I do not know that anyone else has ever studied a population which has been exposed for such a long period of time to such a high concentration of radioactivity. There has been nothing comparable done.

What I would like to see done is to have this sort of thing continued so that they can run controlled experiments along with known doses of radio-strontium in the body. When you consider the skeleton of fish, you have the gonads here (indicating) and the ribs here, and these things are receiving a tremendous dose of radiation from these bones. Admittedly the penetration is not very great with beta, but it is a very constant thing. I think there could well be some effects from it.

FROM THE FLOOR: Is it possible to make measures for sterility and sperm?

DR. KRUMHOLZ: This, we did not do, but --

FROM THE FLOOR: It seems to me that would be a rather controlled type of experiment.

DR. KRUMHOLZ: What I would like to see done, and I don't care much who does it, is someone giving these species a shot of strontium up to a microcurie in the skeleton, and then see what the fecundity of the thing was, how it was compared against the controlled population. I would like very much to see that. All I am doing is guessing on the basis of circumstantial evidence.

FROM THE FLOOR: In relationship to this apparent quick removal of strontium from the skeleton, are you positive that you were measuring just strontium there?

DR. KRUMHOLZ: Yes, sir.

FROM THE FLOOR: How much phosphorous would you have in the skeleton?

DR. KRUMHOLZ: We tried this year-round and our radio chemical analyses on the samples of bone showed that no matter what time of the year the sample was taken and analyzed, we got about 80 percent strontium and 20 percent phosphorous. We did the analysis in the spring, in the summer, in the fall, and in the winter, just to see if we were getting the same ratio.

The ratio of strontium to phosphorous to the bone was the same during all seasons.

FROM THE FLOOR: It has been indicated that phosphorous came up with warm temperatures and high feeding rates and pretty quickly receded, then, following the peak temperature at mid-summer, so I was inclined to be just a little suspicious. Possibly part of this apparent removal of strontium might be the loss of phosphorous.

DR. KRUMHOLZ: Based on our radio chemical analysis, it was not. This was strontium. There was very little 89. We figured it in equilibrium.

FROM THE FLOOR: Some of these symptoms -- I don't know whether Foster agrees with them or not -- but it sounds like the lake is loaded with columorus.

DR. KRUMHOLZ: We didn't find any.

FROM THE FLOOR: That is usually the symptoms which are noticed with salmon.

FROM THE FLOOR: These bacterial --

DR. KRUMHOLZ: Our guess is that 25 percent of the carp population was infected.

DR. ODUM: Are you sure that was not radium damage?

DR. KRUMHOLZ: The scales were hotter than a firecracker.

FROM THE FLOOR: Did anybody look for viruses?

FROM THE FLOOR: On the assumption that only strontium was being taken out, it occurred to me that the strontium is retained in its malleable bone and of the carcinogenic effect -- this might be a promising lead.

DR. KRUMHOLZ: That is what I would like to see. That is why I said the fish were losing this radio strontium with a normal picture of the malleable bone. I think it is one of the hottest things we uncovered.

FROM THE FLOOR: It would be an interesting thing to see research on a Tennessee lake having a bearing on cancer.

DR. PEARSON: It is apparent that we have a lot of loyal fishermen depending on what geographical area they come from. It sounds like Tennessee is a good fishing area, but I think we should find out how the fishing is in the Northwest, and we will now call on Dr. Foster to discuss some of these problems and also you might include a little on atomic energy activities.

DR. FOSTER: We have the Columbia River out in the Northwest which Dr. Pearce talked about a little yesterday. It is not at all strange that we might have a radioactive contamination problem in the Columbia River. The site was originally selected because of the variability of very large volumes of cold water which was present in the Columbia, which was variable and cool, for the plutonium-producing reactors.

(Slide) The Columbia River, because it is cold and clear, makes very excellent water for cooling an industrial plant, and it has characteristics which also make it very excellent for such things as the Pacific salmon. The salmon at the present time is valuing something on the order of $17 million. Most of these fish are caught down in the mouth of the river.

The spotting area or the reproductive area includes the inter-Columbia River. Most of the Chinook, which are the principal fish in that area, utilize the Snake River, which is on the main branch. In addition to these commercially valuable salmon, we also have a very extensive sport fishery in terms of black bass, small mouth bass, and white fish. These are important locally. They are entirely different in terms of a problem of the salmon. The salmon are born in the areas and migrate out to the ocean at an early age and do not come back until they are adults.

The other game fish with which we are concerned spend the entire year in the particular area, so they are exposed to those conditions in a transient manner, but in a continuous, chronic type of exposure.

The fact that we have an atomic energy plant setting up here on the river makes us rather vulnerable to any public relations aspects, you might say, in terms of any depreciation of the salmon downstream in spite of the fact that the salmon populations have been going downhill for the last 50 years and we have only been on the river the last few years. We fully appreciate that the layman sees a smaller return of salmon into his lakes and he may quickly jump to the lake since we do not know much about the atomic energy plant, and this must be what is depleting the supply, so we must know the effect of the river so that we can properly evaluate this sort of thing.

The layman, again, is concerned with the end product. He wants to know what happened to the salmon or his sport fish, and he is probably not concerned with the biological factors that go into producing this end product. As biologists, we are interested in the whole scheme of things. We know that the fish do not have to be affected directly; that if we eliminate the food web that the fish may be eliminated or change some ecological factor, we knock the temperature out of an optimum something of this sort, that the effect could be indirect.

The fact that we have an atomic energy plant there also brings up problems in hazards with respect to human beings.

This, we are interested in also. Dr. Pearce yesterday gave you some indication of some of the measurements which are made in the environs of the Hanford Works. We have people who are catching fish and eating fish. We want to be sure that the fish they do catch and eat are not especially contaminated, and we want to avoid any possible hazard to man.

Also, conceivably, we might think of another hazard there. If the river Plankton concentrated the radioactivity materials from the water, it might be conceivable that someone's drinking water might be a little more highly contaminated than otherwise, but if you think about the problem for a moment, you have to realize if you start out with a given activity in a quart of water, even though you are picking up the organisms, you are not going to have any more activity in this quart of water than you started with.

Our program, as far as the aquatic biology is concerned -- one is plant control, to measure the amounts of radioactivity present in these various forms and relate that perhaps to a human hazard and also the hazard of the organisms themselves. While we are doing this, sampling many species over certain periods, it also gives us an opportunity to pull out a good deal of the ecological relationships which are involved. In other words, this essentially is a long, large-scale laboratory type of function.

Our area includes not the entire Columbia River watershed but there are some 120 miles of the river which is all that is involved, and this is a straight flowing river in here (indicating. A year or so ago, the McNary Dam was completed. This is partly a reservoir and dam back here (indicating). This brings in some new problems that were not otherwise apparent.

Now, what does the Columbia River look like? This (indicating) indicates what we are dealing with -- a river a quarter of a mile or more wide, very clear and cool water. The bottom is mostly cobblestones. There is very little sedimentation and silt deposition here. These are these white bluffs that Dr. Pearce was mentioning yesterday on the east side of the river. The Columbia River, in spite of its large size, has marvelous characteristics of a mountain stream, more so than it does of a slow moving river. I think the difference between this and the Savannah River that we heard about yesterday is about as different as you can find. I have not seen the Savannah, but from the description I have heard, it must be considerably different. The types of bottom forms that we have to deal with are the swift water mountain forms, lots of May flies, lots of snails, sparse Plankton population in a rapidly flowing water stream, as you would expect, and so forth.

The source of our radioactive materials in the Columbia River originates in the reactor and this perhaps recalls some of the things that Dr. Pearce mentioned yesterday. A typical reactor area pulls water out of the River, pulls it through a water processing plant in order to purify it and make it a little better as a cooling water, and this is run through the reactor, and I would like to make one very clear distinction here between the kinds of isotopes that we have been talking about up to now and the kinds of isotopes that we get out of this far side of the reactor.

We have been talking mainly in terms of fish and products, the results of splitting uranium atoms and after they are separated from the waste how they may get back into environments.

If you explode an atomic bomb, this may contaminate an area from fission products, too. The fuel elements in the reactor here are completely isolated from the water. We get no uranium out of these fuel elements, no fish and products into the cooling water which is merely going by and keeping things cool.

The kinds of radio isotopes which we get result from minerals which are present in the water, to begin with, and as these minerals go through the pile, they are bombarded by the neutrons and become radioactive because of the neutron activation. This is the same sort of a process as Dr. Morgan, I suspect, would have at the Oak Ridge where they are producing some sort of isotope -- taking some material, putting it in a pile, radiating it for a time and pulling it out, and it produces materials in this manner. As the material comes out, it contains a large amount of short-lived material. By allowing this material to decay for a short period of time on a retention basis, much of this is dissipated before it is actually discharged back into the Columbia River.

With respect to aquatic biology, the Biology Section at Hanford has been concerned with what this material may do to the Columbia River life. We have been studying the problem since a few weeks after these reactors first began operating back in 1945. We have a laboratory which is located in one of the production areas and one of the ways in which we study these effects is to essentially plug this laboratory into the line. We have a continual sample of water which is coming out from this zevlon which is going into the river, and we continually biologically monitor this waste with acquatic organisms, particularly young salmon since this is the one that has the dollar sign attached to it. These are run in terms of toxicity type of studies.

The second phase, of course, is our field monitoring program which is a sampling of the organisms downstream.

Just a moment on this laboratory sampling program.

(Slide) This is a continual flow process we have where we have a number of troughs which are set up, and we bring in river water which is uncontaminated essentially -- also proper and appropriate amounts of the pile water directly from the system, set these up in a series of dilutions ranging from these which may exist in the Columbia up to dilutions which are many, many times those which will occur in the river, and this gives us an opportunity to see that the higher levels where damage may begin to appear; and if we run it high enough, we can see at what level this occurs, and we can extrapolate back into these much lower, lower levels.

(Slide) This is the sort of result we get. I would like to point out that these are not short-term experiments. These are long-term chronic studies. The same sort of thing that these organisms are actually subjected to in the Columbia River is shown. We like to use salmon,again, because of the economic value, but also because this species is apparently the most sensitive we have in the Columbia River. We started out with the eggs in the fall at the same time the eggs are deposited. We incubate these and we travel them through the larvae and into the fingerling stages and we keep them into the troughs until normal migration time, until time to go downstream.

Obviously, the eggs are very little affected. We begin to see effects during the fry stage in the high levels, and these continue into the fingerling stages. The concentration of the Columbia River Range must be below this area in through here (indicating). These are considerably greater than we have in the Columbia. This is a growing effect of the effluent. It includes all of the factors. It includes the temperature ranges. It includes a factor which I did not mention as far as the chemical toxicity is concerned. It is water that is fed to the pile. Of course, they are interested in maintaining corrosion rates at an extremely low level, so dichromate is added to the water to control the corrosion -- dichromate is a toxic material -- one chemical rate, one chemical toxicity, and one might be toxic -- we can attribute those three things.

We have run this type of thing many, many times. We have also compared this with the water just before it goes into the pile. In other words, we brought a sample line down from the chemical treated water just before it goes to the pile. We run those same dilutions at the same temperature and the result is identical. In other words, if we eliminate the factor of radiation from this water, we get the identical same picture which tells us that the radiation is not the major factor with which we are concerned,

Secondarily, we have been running, this year, a dichromate test to see what the effect of the dichromate is per se.

(Slide) This represents the parts of dichromate which we have added. This, again, is a chronic study. I think this brings up a point in terms of conventional studies on toxicity.

If you look through the literature, you generally find that dichromate is supposed to be toxic to one-fifth -- somewhere around 50 parts per million. If you run these for a few weeks at a time, you can see whether you start to get a mortality or not

In our test, we know that 50 parts to a million was way too much. Five parts per million was way too much. The strongest we used was half a part per million, and this is the result that we get, and dichromate runs for months at a time at a half a part per million, and these are the mortality figures which we get. The eggs -- fine -- practicality no mortality even at this level (indicating). During the fry stage, we see mortality and during the fingerling stages this mortality increases. This is in terms of mortality (indicating); a slowing of the growth rate is even more spectacular, and we see again the control. Almost all of these are slightly less. This should be four. The two and the four, these are not statistically significant, but from the .06 on down, it is statistically significant in terms of a slower growth rate, So, we find out that we are -- and in terms of a conventional type of toxicant -- we have a factor of probably one one-thousandth of the assumed toxicity of a conventional type of chemical. This brings up a point where I am not sure that we know enough about radioactivity, and its effects, as we do about some of the old chemicals that have been around for a good many years in terms of these chronic studies.

With respect to temperature, the species with which we are most concerned are the salmon. We know that we can put carp or something of this sort in the straight effluent at very high temperatures, and they get along very fine. We also know that

that the Columbia River salmon are also very temperature sensitive. The young migrating size salmon can stand 20 degrees Centigrade, which is fine, for a number of results. The adults are probably even more resistant unless you get up into temperatures around 35 Centigrade where you have bacterial action taking over and disease coming in.

This is not so with the eggs. We noticed in our regular monitoring experiments that we thought we could see some mortality in eggs which we could attribute to fair increases in temperature. We ran a temperature test on eggs of Chinook salmon, and these 16 and 17 degrees you might compare with Dr. Patrick's quotation of yesterday for some of the warm water fish.

(Indicating) The blue area here represents the range of normal temperatures in the Columbia River over a period of years. We happened to run this during 1952 and, obviously, this was probably a good or bad year to run the thing because it was the warmest year that we have actually seen out there. These are experimental levels, and we set the experiment up to follow the temperature of the Columbia, introduced salmon eggs at the time the fish were spawning and observed the mortality on the eggs. This was actually carried through into the fingerling stages.

Rather interestingly we found that the lowest mortality was below that normal for the Columbia River. It was a little surprising to us. We started to get appreciable mortalities when we raised the temperature perhaps two or three degrees Centigrade, and these temperature raises are considerably greater than those created by the effluent going into the river. I would like to point that out.

Also, since we have run that, we found within this species there is a considerable temperature difference. This has been shown, too, with the work at the Applied Fisheries Laboratory. We ran those on Puget Sound because these eggs are a lot easier to get and we got these from strains that inhabit the Columbia River. Our local strain are used to spawning in warmer water than is characteristic of this particular species by a matter of a few degrees.

The tolerance of the local fish is slightly greater than it is for these.

A second experiment shows that these mortalities -- we can shift this (indicating) up a couple of degrees from where it is now.

Enough for the temperature factor.

Let us now get back out into the Columbia River. These sampling stages have been selected for several reasons. One is to try to catch the spot where concentrations of radio-active material are high. Also, we try to select them so that hydrographically they are as similar as possible. We try to select shallow, gravel river areas where the current speed was approximately the same and the bottom rock was about the same. Consequently, in comparing one station with another, we would have something closer to look at than if we picked a lagoon area in one place and a fast river in another.

These organisms which we pick up in the Columbia River do contain radioactive materials which they have gained from the water. As you have heard from the previous speakers, these things vary rather tremendously according to species.

This particular kind of a distribution will change in different parts of the river as we go downstream and also change with season, but if we pick out the late summer period close to the pile, this is the type of distribution that we get.

This (indicating) may be surprising to you. The mid-larvae are often more radioactive than the Plankton but, characteristically, through the major part of the area, the Plankton will be the most radioactive part of the substance. The organisms that feed close to these bottom forms are the small organisms which are much more radioactive than the ones which might be characterized as the larger fish which are further up the food chain. The white fish is a bottom feeder. It usually is quite a little bit more radioactive than the bass which is a . . . type of fish. This relationship becomes apparent later where the bass drop off in their feeding habits. The white fish continue feeding later in the fall, and we often see white fish two or three times more radioactive than the bass.

Looking at this on a gross picture does not really mean much in terms of concentration because this represents all types of isotopes which are present in the effluent. We aae interested in specific isotopes, how much of an individual thing is concentrated. This will give you some idea of the appreciation of that, perhaps. The distribution of the radioactive isotopes in the water at about the time they are discharged from the river (indicating) and this is similar to the figures which Dr. Pearce gave you yesterday with a predominance of the shorter lived material, P-32 making up this is less than one percent of the activity, but when we go over into the animal forms, these short-lived materials have tended to disappear or are not picked up.

P-32, very essentially, physiologically, we know, makes up the bulk of the activity -- even a greater percentage of P-32. This is on the basis of 100 percent of the activity. It does not represent the relationships that we were looking at before.

If we extended the heights of these things so that they would be in relationship to one another, we would have to use a log type of scale in order to get them on the paper. As we saw before, the animal forms, something on the order of three orders of magnitude higher than that of water itself.

The concentration of activity phosphorous mainly, here (indicating), of course, is primarily a function of the percentage composition of phosphorous in the various tissues. As Dr. Krumhol indicated with strontium concentration in the bone and concentration of phosphorous ends up so that the phosphorous rich structures are the ones most radioactive, and the fatter ones are the least radioactive, and this is to our advantage in terms of people who are eating these fish where the moderate radioactive materials are generally discarded and the least radioactive materials in terms of the fish muscle, having the lower radioactivity density.

In terms of seasonal variations which have been brought up as before, of course, the flow of the Columbia River represents here, as mentioned by Dr. Pearce, which changes by a factor of five, approximately, during the spring fresh season. This originates mainly from the snow and ice fields up in the mountains and this time of the year the weather warms up and we get a few rains up there, and these snows melt very rapidly, flush down through the river and the flow goes up. Of course, the radioactive materials in the river water, if the piles operate on a level scale, the dilution effect here is the one which is important, so you have activity density of the water being the reverse of the flow pattern. Again, we find a different seasonal pattern in terms of the river organisms which are low during the cold winter months. They are high during the late fall months and we have superimposed here a temperature curve, and you can see the general similarity of the temperature curve and the activity density curve. This brings out the physiological importance of how these forms get the radioactive materials.

We know that the Plankton farms which are in close association with the water have a close exchange, a good deal of adsorption and absorption, following much more closely the activity density pattern in the river, but those animal forms which are tetting their activity mainly by their food have the most activity at the times of the year when they are feeding most actively.

During the cold periods of the year, near hibernation, little food being consumed, the activity is low, and during the periods of high physiological activity, high food consumption, it goes up.

We notice a similar pattern, variations in this, with insect larvae who may go through a few stages of arresting, even during the warm period of the year when normally the activities would be high, and they go into arresting stage when they are not feeding and the activity will drop off. I think Louis was referring to this.

Even in some of these fish in warm temperature, if they stop taking in the material, it will go off in spite of the temperature picture.

How do these things vary with distance downstream? Of course, in through this (indicating) zone here, we have several reactors and the activity density of the organisms increases as we add more effluent to the stream. We find the highest radioactivity in the old town side of Hanford along the shore with a rapidly diminishing activity downstream.

I hope you appreciate what this relative activity density is on all of these slides. You merely take the highest value on the chart as 100 percent and express these other values in terms of percentages of the most radioactive material.

This particular curve (indicating) is for Plankton which has a fair short-lived component so that the activity drops off as we go downstream due to two or three major factors. One of these is time for physical decay of the shorter-lived materials. Another one is a peculiarity of crops of our sampling, and most of this represents in that collection along shore or in areas where the effluent has not been completely dispersed -- you will recall in the first slide we had a band of effluent which went down the river before it became dispersed throughout the river as a whole. Of course, some of this fast drop-off is a result of better distribution of the effluent throughout the river as a whole. We have those two factors and we also have incorporation of these radioactive materials into the biological form which draws them into a biological pool and which makes them available for further radioactive decay for this whole mass of radioactive material is finally moved down the river.

This curve (indicating) as I said, is typical of Plankton. We can draw different ones for each organism and get slightly different slopes. If we draw this (indicating) for the fish, it would not drop off as rapidly because we do not have as much short-lived material present. Actually, if we compare this far enough downstream, in spite of the fact that the Plankton will be most radioactive, the most radioactive form close to the plant, if we go downstream in terms of McNary or Bonneville Dam, we are apt to find that the fish. . . at P-32 are more radioactive than the Plankton form.

The turnover or general accumulation of radioactive materials on a long-term basis has been of particular interest to us in terms of the McNary Dam Reservoir here. We changed a stream condition, flushed the material out into the ocean, into one of a stagnant environment, so we were interested in knowing whether this material might attend to accumulating in the reservoir behind the dam in terms of silt deposition. We followed this over a period of a year since the dam was formed. (Indicating) These are activities in mid-larvae starting in in January where it is typical, and then we have a drop in February, March, April -- during the flood season -- a major build-up during the warm months of the year. These are typical seasonal patterns that we showed before.

Again, last winter a drop down during the cold months and, of course, we were glad to see that these levels and the continuing ones beyond this slide (indicating) here, virtually are the same as they were during the previous years, which certainly, for the first year at least, indicates that there is no major accumulation of radioactive materials in the bottom material which is being picked up by the individual organisms.

Sampling for quantitative determinations -- we have the laboratory program going on in terms of toxicity studies, but we are also aware that the salmon fry may not be the most sensitive organism that we have to deal with in the river, so we are interested in knowing whether any of the population in the river are being affected. This gets back to some quantitative type of samplings of river organisms.

We started out taking quantitative samples in a number of different stations which, as I mentioned earlier, were selected because we thought they were as near the hydrographic conditions as we could find them. This gets to what Dr. Patrick pointed out yesterday. Comparing one station to another, it requires a terifically large number of samples in order to try to make quantitative differences. These stations we looked at in terms of both numbers of species and also the quantity of individual species which were involved. The species number compare quite favorably up and down, but we still get wide fluctuations in numbers of organisms between different stations. This probably points out the kinds of differences that occur even in stations which are selected for similarity initially.

We have arranged these things in terms of the activity density which we find in the river water which is associated with those, so we find that the stations close to the Hanford area are most radioactive, and those further down the river, with the exception of control, are plotted above and, obviously, in terms of numbers, we have picked out three widely different more biological forms. We have no relationship here so far as the activity is concerned.

(Slide) This next slide shows the sort of thing we can get if we compare these with some of our other hydrographic conditions. We have selected the caddis fly larvae here. If we plot the abundance of the caddis fly larvae versus the water velocity of that particular station, we get generally quite a nice correlation in terms of abundance. We get one unusual situation here.

In this case (indicating) this represents an area where the rocks were especially large and had more irregular surfaces than the others which had better attachments at points. Again, this brings out the fact that in comparing numbers of organisms from one station to another in the river, it is a pretty risky sort of thing unless you have a very large number of samples and know what really to expect.

We are continuing, but we are continuing on only one station plus the control which is located in the most radioactive area immediately downstream from the reactors, Station No. 5, which you saw on the slide earlier.

By comparing the species compositions and the numbers of organisms at this one station from year to year -- there are variations, so we have cut this down to selecting the fall period when we find the greatest numbers of things available.

This station is serving as sort of its own control. Instead of sampling many different locations, we are sampling one station intensively about once a year in order to follow any population change which may occur here on the premise, but if a population should occur in the Columbia River, this is the spot where it ought to be observed first.

We would, of course, like to be able to have some quantitative type of measure on the fish, but we do not know of any practical sampling means whereby we can go out in the Columbia River and go out and get anything that means anything where you have population swimming up and down the area.

The salmon is the thing with which we are most concerned, again, perhaps, because of the economic value. I had better tell you what you are looking at here.

(Slide) This is an aerial photograph of the Columbia River, looking through the water down to a gravel bar. We get some 300 or 400 of these nests in our local area each year. The salmon, as you are all well aware, have their homes very precisely. It comes back very nearly to the particular spot where it was born, so we feel that perhaps one way we can get at a population analysis -- lacking a better means of collecting fish -- is to compare the numbers of fish that keep coming back and spawning in the Columbia River and in this particular area.

If the Hanford Works was responsible for eliminating the salmon, we would expect this particular local population, which is highly specialized for this particular area, perhaps, would disappear. If the numbers of fish which keep returning here remain about the same, well, things just probably can't be too doggone bad.

We have counted these numbers of nests each year.

(Slide) This shows an analysis of the numbers of nests which we have seen. If we assume that the year the parent fish spawn, 1947, 1948, 1949, 1950 -- is normal, and we use this as a zero line (indicating) we can compare how many nests were available when these offspring came back, and we can determine here (indicating) if our assumptions are right. There are, of course, lots of if's here. Then, we should know if this population is remaining the same for going downhill. These fish are four years old when they spawn, so each four years there is a different population that has to be considered independently of those which spawn the year before or the year after. This is what happened. The fish which spawned in 1947 produced offspring which came back. There were a few more nests in 1951 than there were in the parent year of 1947. Every year after that, there have been a fewer nests than in the parent year. Again, this does not really mean anything unless we compare this with what is happening to the Columbia River population as a whole. They are going down over a series of dams and going out into the open. Many have been disappearing there. So, we have to compare this with what is happening to the Columbia River salmon as a whole.

These darker bars represent how the salmon population has declined from the same normal years. We see that with the possible exception of 1953, we find that the decline in the Chinook salmon in the Columbia River has actually been more severe than those which are spawning in our area.

So, if all of these assumptions are right, we do not think the salmon have been hurt.

Now, in closing, how much does this mean in terms of radioactive materials which these fish are getting? We have plotted some colored pictures here which show 100 percent of activity during particular times of the year and tissues which are radioactive relatively speaking. If we go back to this station immediately below the reactors, where we find the highest concentration of radioactive materials in the organisms, and if we pick the late summer period when the radioactivity is greatest and we pick a small fish which is more radioactive than the adult form of the species and compute the dose of radiation which this particular small fish gets, and this is the worst condition that we can visualize for these fish in the river to complete the dosage rate, and this comes out to about .1 of a rad per day. This .1 rad per day you can probably compare -- to put this in terms of a week -- .7 rad per week which compares with the human permissible limit which is, I believe, at the present time, three-tenths of a rad per week, based on a human being who has a life span which is many times that of the fish and based upon genetic change, so we would be very surprised if we saw any radiation damage in the organisms of the Columbia River.

In terms of edibility of these particular fish, we point out that they do accumulate some radioactive materials, but we see no reason to close the fishing because it would be utterly impossible for anybody to sit down day after day and eat the quantity of fish that would be required to give that individual the body burden that would be permissible in terms of the standards that we work with in atomic energy plants.

DR. PEARSON: We have time for just a few questions on this paper.

DR. KETCHUM: Your lowest form in the food chain order was a herbivor, and the amount of accumulation decreased between that and your fish. Does that mean that you would have two orders of magaitude more than the herbivor?

DR. FOSTER: This depends on several things. The percentage of phosphorous composition of the various forms here is of prime importance. In other words, I think this will be out a little later. Plants have a lower phosphorous composition than the animals. If you get into the phosphorous rich animal and you have the possibility of this animal eating a good many plants, concentrating the phosphorous itself and thus ending up with a higher activity density than perhaps the plant did. We also have the time lag which is involved here in terms of what you might call specific activity -- the activity of the P-32 as compared to the amount of stable phosphorous per se that is there. Obviously, this specific activity has to decrease as you go down through any food chain because you have the time element with respect to radioactive decay.

DR. KETCHUM: I know that in the marine situation on a straight chemical basis, phosphorous is close to ten to the seventh.

DR. FOSTER: You are talking about the plant versus the water. We have an aquarium type of experiment where we know the quantities there a little nicer than we do in the window. We have concentration factors on the order of ten to the sixth from the water into the Planktonic algae in very low phosphorous concentrations in terms of the water.

DR. KETCHUM: But you do not have any plant observations on the river?

DR. FOSTER: Yes, we can write down a concentration factor here in terms of the phosphorous in the river versus the plant, and it is on the order of 550,000 times. The fish, in terms of phosphorous -- it is on the order of 150,000 times. Of course, we do have differences -- (indicating) this in terms of radioactivity is not necessarily precise in terms of the phosphorous per se because of decay factors which are involved here. We are dealing in terms of concentration factors of 150,000, 350,000 times, which is something to deal with in terms of perhaps drinking water tolerances.

If you compute the amount of phosphorous which is permissible in terms of drinking water and assume that an individual is drinking a couple of liters of this per day, then, therefore, he gets this many microcuries of water (indicating), but if you take the same volume of water and put a fish in it and he concentrates it 150,000 times and he has a lot of water to draw this out of, you can compute this (indicating) as a rather dangerous level to have if somebody is eating fish.

DR. PEARSON: Is it used at Richland and Pasco and Kennewick?

DR. FOSTER: It is at Pasco and Kennewick. They have a standard water filtration type of plant at this type (indicating), and I think Dr. Pearce gave some of those figures yesterday.

(Indicating) This is an indication of how actually biological forms can help you out a great deal. If you start out with an even desirable or a near desirable tolerance in terms of the amount of radio phosphorous in the water, in the river, and you add this to a river system where you have a large biological community with a high competition for the phosphorous, the phosphorous leaves the water very rapidly and goes into the forms; even if it ends up in the Plankton, this is to your advantage, because if you run the water through a filtration plant, the real phosphorous stays on the side.

DR. HERDE: When you are asked about the concentration factor for plants, you immediately went to the Planktonic algae and you indicated 350,000 multiplication factor.

I know that you would like to emphasize that that sort of concentration factor does not request for terrestrial or semi-aquatic seed types that are growing in the water. They are relatively low in radioactivity. I thought the ecologists might be interested in knowing that we do not have such a concentration in seed plants that have their roots in the river.

DR. FOSTER: That is true. The margin of vegetation is much lower.

MISS PATRICK: Have you ever done any studies on the attached algae?

DR. FOSTER: Yes, we have studies on the attached algae, but the variability in this material is so great that we have almost stopped sampling the thing. We have several factors which are involved in this attached algae. One is that we do get a rather violent fluctuation in the Columbia River. The flood stage is the worst since this goes up from here (indicating) to about 15 feet higher and back down again during the flood stage.

There are weekly variations in terms of a foot to a foot and a half in this particular area (indicating) because of the power dams that are upstream, and during the Sunday close of of the power dams -- they don't need to make so much electricity, so we have a fluctuation in the zone which is not too good for the attached algae.

We have a mat of algae on the bottom which becomes crudded up with all kinds of material floating down the river and what we are measuring there in terms of algae -- it is difficult to separate what is activity in the algae and the other organic materials which have gotten into the film, so we find in similar Plankton, the algae, and we have done this for many years, but we change species very rapidly in the river and the amount of debris which is collected there is a function of what happened to the river the week before, so, in terms of consistent types of information, the algae in the bottom is about the worst sort of thing we can measure.

FROM THE FLOOR: Is it the adsorption phenomenon or what?

DR. FOSTER: This is one which we think will probably -- straight adsorption in Plankton may act for a very high percentage of the total amount that is there.

FROM THE FLOOR: We did some of that, and we did it with cat-tails. We got some cat-tails and a good bit of it was absorption and a good deal of it was adsorption.

DR. FOSTER: Some of these are margin plants growing above the water. The activity was density and above the water, but when the material died and you picked it out, you had a higher activity density than you had to begin with.

DR. KRUMHOLZ: It is on page 19 of your summary.

DR. FOSTER: I would like to toss that on to Dr. Pearce in the biophysics section.

Is it correct in recalling that the amount of natural phosphorous in the Columbia River is extremely low? Is it lower than most natural water?

DR. PEARSON: It is low, but not particularly. I think it is apparent that we do not have all of the fish problems solved, and Dr. Foster mentioned the difficulty of sampling in the Columbia River, and it looks to me like it may be desirable for a number of you to go out there so that you can help him out on this sampling of salmon at the appropriate season.

To finish up our Hanford program, Dr. Hungate will discuss the uptake of fish products and plants and animals in the environs of the Hanford plant.

DR. LINDBERG: I wanted to ask if you have done any toxicity studies on the caddis fly.

DR. FOSTER: We are setting up equipment for that. We realize the salmon might not be the most extensive form, and we want to do a shotgun experiment with a large variety of invertebrates to see if we can find something that may be a more critical organism.

MISS PATRICK: If you are working with caddis flies, be sure you are working with them specifically, and know what you are working with, because there is one matisichi that looks like the ordinary caddis fly, but they contain hugh concentrations of zinc. The ordinary fly won't stand any. Similarly, we have found a similar amount on caddis flies and there is great variability, so please know your species.

DR. FOSTER: Jerry Davis, who is in charge of our program -- it happens to be the caddis fly for the specific Northwest. We are probably in better shape on that one than we are on any others.

DR. HUNGATE: The material I wanted to discuss is a little bit of the material derived from several different teams that have been working on a variety of problems. These are, in general, related to these three problems of the disposal of radioactive materials at the Hanford plant -- the ground disposal, the gaseous disposal and the admission to the river. Any data that you find in any one of these fields is very applicable to any one of them.

The first part I might start with here is the absorption of the various fission products into plants. This is more or less an anticipatory type of program in that we do not really have at the present time any kind of a problem along this line, but you can always anticipate that the information of the uptake of fission products into plants, the rate or quantity taken up will be quite valuable information.

In starting a study of this kind, you do not want to start out and just run through the large number of fission products. I am sorry there are so many numbers on here, but this summarizes all of the fission products which, during the first ten years, occur in the total mixture with a quantity of one percent or greater.

(Slide) This indicates the various fission products in general here.

In the general groups here (indicating) on down, these are the rare earths and these materials, in general, up in here are individual components. I grouped them this way because actually the plant does not distinguish and in general the rare earths are all treated as though they were a common kind of element, and you can group others such as the strontium-calcium-barium as individual entities.

We have been talking about strontium-90. If you start reading across this table here, there is less than one percent of the total fission product of strontium-90 at 10 days, 90 days, and by a year with this, it becomes 1.8.

This strontium-90 does not increase during this time to become 1.8. This merely indicates the general decline of all the rest of this mass of fission products so that the strontium-90 becomes a gradual emergent material as constituting the identifiable radioactivity at that time.

What we did then was to choose certain of these radioactive strontiums, caesium, iodine -- choosing some of these isotopes that promised to be present in quantities which would be of significance and choosing these which would give us a broad spectrum of the absorbable normal products.

The next slide indicates one way we studied this, which was by the Naeubayer experiment with which many of you are familiar. Taking 100 grams of soil contaminating with trees, we obtain an amount of the isotope which we want to study, the strontium-90 or iodine, or whatever it may be, getting this uniformly distributed in a small dish approximately so (illustrating) in diameter and then seeding in here 100 barley seeds which gives a very dense population and gives maximum penetration of this soil by the root system and probably an approach toward a maximum uptake into the aerial parts.

The aerial parts were then harvested at the end of 18 to 20 days, which gave a maximum concentration under the conditions in foliage.

The next slide shows the results of this kind of experiment, and in order to get an emphasis on concentration which we feel is quite important here, and in order to put a basis for comparison of these studies with some nutrient studies, which we will describe later, we are expressing our results as a concentration factor which is simply the concentration in the aerial portion as compared with the concentration in the root environment. In this case, it would be the concentration in the dry harvested barley leaves as compared with the concentration of strontium, or whatever we are studying in the dry soil.

Here we see certain elements, because there is really not much difference in the absorption of strontium-90 or strontium-89 or the different iodines. Whether it is strontium-90 or strontium-89, the plant does not distinguish, so the strontium is here absorbed and it is absorbed in high or higher quantities than any of the others. With barium, it is not quite so much. Iodine is absorbed comparably to strontium. There is a large differentiation which the plant brings about in selecting one isotope against the other. Those over on the right absorb very poorly.

There is a slight difference in soils. Diferrent soils get a differing absorption. This quite varying between what you get in the wide elements. You might say the absorption of soils is the maximum of ten -- the influence of the soil.

The next slide shows the uptake from the nutrient slugs. With a nutrient slug, you can control a single environment substrate factor more readily than you can by modifying a soil. If you modify it by changing PH, or something, then you are going to modify a number of things. The results from here on (indicating) are expressed as concentration factors from nutrient slugs.

As the question a moment ago proposed, this concentration factor is taken as the concentration in the plant as compared with the immediate root environment, not the immediate nutrient in general. If you add these, in general, they add almost in total over to the matrix which surrounds the root, and we attempt to relate the concentration factor here to that immediate root environment. We have a rough method of estimating this, but we feel it gives a much greater answer than using the nutrient in general.

Observing this data, we see the differences in uptake between, perhaps, representative organisms here. They are very comparable. Strontium is pretty uniformly distributed. Caesium has perhaps a ten-fold variation. In general, with this admittedly very brief sample, perhaps a difference of magnitude is on the order of ten. One order of magnitude exists between different species. I am sure if you look far enough, you can find some that exceeded this, but this seems to be the general limit of the effect of different species.

Likewise, different organisms, different parts of the plant show differing uptakes, and you will see in general the leaves are the highest part of the plant. This is perhaps fortunate because leaves are a nice thing for sampling. It is always interesting to know that in general the seed portion, the seed has the lowest concentration. This is true because in so many instances we do use the seed part for human consumption.

The next slide shows the effect of PH as the slugs become more acid or is adjusted to become more acid, and you have a more rapid uptake. Whether this is a physiological response of the plant or a solubility phenomenon, we have not spent time to absolutely determine.

The next slide shows the effect of concentration. We were rather surprised. Kermit Larson mentioned this same phenomenon yesterday, which he has observed -- namely, although with the various isotopes it has generally been considered the desirable thing to prevent or decrease the absorption of these various radio isotopes by co-treatment with the non-radioactive carrier element. By this co-treatment, it has always been felt that you dilute the specific activity and, thus, a plant or animal is not going to take up as much. You all know this is true in the case of iodine in the thyroid. If you give iodine -- 127 along with a dose of iodine-131, the organism is not going to concentrate in the thyroid gland as much of the I-131 as if you had not given this carrier along with it. This does not hold true for the plants. This is a log scale here, so it spans over quite a diversity of concentrations. As you increase the concentration, there is a tendency for an increased absorption of the radio element.

This simply says that the plant over in here (indicating) has -- well, compared to this, it has five orders of magnitude more element in it than over here. If you are studying iodine, there is five orders of magnitude iodine in a plant over in here (indicating) because we are using the I-131 simply as a tracer.

The I-131 or strontium, or whatever you are using, is kept constant. This is merely the percentage uptake. The percent of the uptake is increasing generally. This is perhaps most notable in the case of iodine.

With the others, there is practically, you might say, roughly a constant. But even this constancy is somewhat surprising. This probably would not hold over such a wide range if these fission products were essential elements, but you must remember these are not essential elements and they are not needed by the plant.

The next slide shows a test of the possible explanation of this uptake and here (indicating), as you remember, is an increa ed absorption of iodine, and in this broken line here we have another experiment in which the amount of iodine was varied and I-131 was tested; and there was an uptake of iodine by the plant.

This particular test was made to determine whether this might be due to toxicity of the root cell membrane and allowing the material to pour through it more readily. If this were true, it should be possible to create the toxicity by one element and cause another one to pour in, and we tested then the toxicity with iodine and the toxicity factor of strontium, and, under these circumstances, there seems to be a marked decrease in the effect of strontium by the toxicity of iodine. Apparently this is not due to a membrane toxicity or toxicity to the plant itself. Our best guess -- and it is simply a guess -- is that you will have varying absorption in the soil.

In summary, I might just say that these plant studies indicate that you have approximately a thousand between the absorption of different elements. All of the other factors -- soil, PH, even concentration here, the species, part of the plant, and so forth -- in general, these fluctuate within a factor of about ten.

(Slide) This is an extension of the studies to outdoor plots where we were trying to determine the absorption. In this case, we are attempting to make a determination of radio strontium from the outside. It merely illustrates some of the techniques that you have to follow in order to control the element.

(Slide) Here we have effectively mulcted the ground because we have contaminated the ground with strontium-90. If there were wind erosion, the particles would have broken away. Under these circumstances, we actually found that the values from a single contamination where the material was not turned under was a value of uptake on the order of one one-thousandths of the value that is obtained in the Naeubuyer study.

As I say, this was all on the surface, so compared with Naeubuyer, this was on the surface and not done on the feeder roots. This is being continued this year to determine whether the distribution which you would have in normal agricultural products will increase this value out in the field.

(Slide) This indicates a study which is of quite a different thing. At the plant, due to production of the different elements, we have a problem from the emission from the processing of iodine through vented gases. Those are vented through very, very tall stacks. You get them up into the higher atmospheres where you have maximum possibility for dissipation of the activity. This experiment was set up to study the effects of chronic application of iodine to a large animal. We have quite a large number of studies on small animals such as frogs, mice, et cetera. The sheep represent a rather significant population in the environs of the Hanford area, and so the sheep were used here to determine the exposure limits which will produce damage and to evaluate and define the pathology, the histological representation of the damage when you do find it.

When you do study this, you have to start with extreme ranges so that you will be sure to have a level which will show damage and, likewise, levels which will not show damage. So, we started out with levels down on the order of .005 microcuries of I-131 fed to sheep per day and the maximum value of 1800 microcuries of I-131 fed to sheep per day.

To give you an idea of what this represents, the lowest value here is .005, which is the lowest value that you would give to the thyroid; the MPC radiation is of .3 rads per week, so the lowest value was the MPC value for man.

As you can see, it simply was not practical to continue it, because using the measure of the thyroid deficiency, the uptake of iodine -- in other words, the thyroid will normally pick up on the order of 20 percent of the iodine which is fed into that glass using this measure of uptake as a measure of the normalcy of the gland. You will see the 1800 simply burned out the thyroid gland. Actually, it returned to normal within 36. I should not really say normal because it returned to zero uptake. Likewise, 240 microcuries within a very short time -- the thyroid was simply burned out by this cauterizing effect of the I-131 dosage.

To get right on with it here, briefly, we had a rather large peak initially in all of the curves at the Hanford environment. With respect to the foliage, there is an iodine deficiency. It was not initially recognized, so, on a goitrogenic diet, these animals absorbed more iodine than if there had been a sufficient supply of iodine in the field. From this point on, there was a supplementation which brought them up, as you will see here. (Slide) This pink one here -- this 5 micricurie per day level looked pretty reasonable, but then this time the uptake fell off very markedly. This seasonal peak which is characteristic -- the thyroid becomes active during parts of the season and then becomes relatively inactive, so you go through an annual fluctuation period. This fluctuation did not appear after the winter of 1951-1952. The next year there was no subsequent rise. On the other hand, the level at 1.5 microcuries is going along still even after the five years here with its normal cycle recurrence; thus, there is apparent damage here at the 5 microcurie level and no damage at the .15 microcurie level.

This 5 microcurie level is 10,000 times the MPC for humans -- just to give you an order of what things we are talking about.

(Slide) The next slide shows the weights of these thyroids at the end of this current. These have just been sacrificed and the data has just been worked up. As you can see, the controls were kept on these projects so that there would be no possibility of contaminating these controls, running about 15 to 20 grams of weight per thyroid.

On the other hand, the 5 microcurie level is markedly decreased. There has been a burning out due to this iodine of the gland. The 5 microcurie per day -- actually it is larger but it is not statistically so from the controls. So, you see where we have a level here which produces damage and one which does not. The one which does is something on the order of 10,000 times MPC in humans.

We have talked here about the uptake into the glands. There have been all kinds of expressions examined.

(Slide) The next slide here shows one of these, a histological kind of damage. It was actually taken from this five microcurie per day level, and you will see over here in the controls that you have the follicles. There is where your colloidistoid is found. The interfascicular area here is nice and clean, with a minimum of fibrosae and nice large follicles.

Over here you have an extensive fibrosis in between, and this fibrosis in between the follicles seems to be one of the things that is most charasteric of this damage to the thyroid gland. Accompanying this usually you will have a decrease in the follicle size, but the one that is most characteristically and the one most readily used diagnostically is the fibrotic tissue in the follicles.

As I say, there were a lot of other things which I simply do not have time to cover. I do not have with me the data which covers these, either, so this is merely an indication of the kind of things which are identified.

I might just mention that in the studies it appears that approximately 20 percent of the daily dose is put out in the milk, so you have then an elimination of the iodine here, which can be through any of the secreting tissues.

This study here, of course, is applicable to environment like Hanford which has a chronic elimination of iodine. This would be true only if you gave this five microcuries per day, day after day, to maintain a radiation level in the thyroid. If this were given a single dose over 100 doses, it would be absolutely ineffectual.

The next slide shows the kind of uptake which follows a single feeding. In this case, the hay was contaminated at the start of the experiment. Of the sheep fed on this single batch of hay, we see the increase of thyroid activity, increase of iodine activity in the thyroid gland, and this reaches a maximum right at about 12 days. This is very reproduceable.

Following this maximum, there is a return, and this return to the base line simply expresses the decay of the iodine in that hay. This is the kind of thing which one would observe, say, following fall-out where the material is on the ground. It decays out, your animal reaches a peak and drops after this peak. It is only in the Hanford area where we have a body burden which is maintained.

I might just say that this peak here is something on the order of 500 to a 1000 times on a concentration basis, that of the hay at its initial highest level at the time of contaminating the hay. The peak in the thyroid, the concentration lies only on the order of 500 to a 1000 times this (indicating).

This establishes, then, in a rather definite the levels of damage which are required for thyroid in the case of iodine.

To determine what this is doing out in the population in the environs of Hanford, we do not have sheep running around the reservation, so we use the next best thing, the jack rabbit. They sometimes look like sheep, when you consider the size that they reach. Only in the jack rabbit would you be able to take off such a good population and still have it come back and not become depleted. There does not seem to be any evidence that this is affecting the population.

(Slide.) The next slide shows a comparison of the concentration in the thyroid gland with the emission rate from the stacks. This is a ratio, and you can see from the fact that it says in here that at any time this stays within a factor of about 10 -- the observations seem to indicate here that there is a correlation that is reasonably close to the general kind of situation that you have.

Furthermore, this shows the same kind of cyclical phenomenon that we observed in the sheep. You have a period of low activity, and then a period of high activity in the glands.

I might say that the maximum level which has ever been attained by this sampling method in the rabbit thyroid approached 5 of the level, which is noted to be damaging in the sheep. You might say, "Well, this is approaching then a damaging level," but we must remember that this was a week. This rabbit undoubtedly went to a peak and the peak dropped off. Peaks of this order are of no consequence, really, because I should have mentioned, for instance, that in the sheep data, it needs a cumulative dosage on the order of 10,000-R because these thyroids begin to show damage. If you give it a low rate, it will take a long time, and when they get to this level of 10,000-R, there will be an apparent damage. Even though we do get up to a region which is modestly approaching at a certain time the damaging level of sheep, it surely is not of significance.

Furthermore, these tissues are treated historilogically, examined to determine whether there is any apparent damage.

Probably the biggest factor in this curve, in addition to the cyclic activity, is the fact that during the summer we have reasonably good winds. We have a hot climate there. The peak boils up and tends to dissipate the activity, and it spreads out and dissipates the activity. It spreads out and you do not have any concentrations that will show in this kind of a monitoring.

In general, the activities which we notice -- the stations are set from 1 kilometer to 30 kilometers and, in general, the activities we find correspond quite closely to the inverse square of the distance. As you go out, if the sight of origin increased activity, then the activity drops off.

Age is certainly a factor here. The two-months rabbit is perhaps three times as concentrated as an adult. Comparing these with other animals, the carnivores are on the order of five or less. The rabbit, of course, is a herbivore. The carnivores drop down to a fifth of this value.

We sample as much of the population as we can find. We go through insects, birds, reptiles, all of these different forms, and these other forms are all markedly below the rabbit. The herbivores, in general, come up and approach the rabbit level more closely and the carnivores are on down.

To mention some of the results that have been obtained from the non-aquatic organisms around the river, returning this effluent to the river, which causes possible damage, you can envisage a potential damage just as Doctor Foster did from this activity, and so envisaging this, we set up the experiment to to test for this.

In the case of the plants, we tested for it by setting up a series of experimental plots in which we took the reactor effluent much as Doctor Foster did, and we took it and put it onto a miliacre outside plot. We watered them. We had three series of plots, one using controlled well water, which had no activity, one using 5 percent reactor effluent, which is well above any level that would ever appear in the river, and then using 100 percent reactor effluent to determine what maximum potential one would ever have. Only the 100 percent level indicated to us possible significance. Determining the activity in the seed here, the activity in the seed, here is the control, or you might say background level, which would be normal to any uncontaminated crop grown in the area. The 5 percent plot was essentially the same. The 1 percent plot showed a value of 2.8 plus or minus .3 times 10 to minus 5, which is not even twice the normal background level. It is essentially nil. It is very hard to get a difference of this type. You just can't evaluate it, and it is so low.

With respect to seed, yield likewise drops. It is interesting to note here that the seed yield here is rather definite. Certainly when I compare it with the amount of radiation that is present, again, as Doctor Foster found, it seems to be the chromium which is used in the pre-reactor treatment -- simply a standard boiler type process treatment -- and this chromium accounts for this drop because, in laboratory experiments, we had very low concentrations down to .01 part per millionth, and we get observable effects on the plant. This seems to be, interestingly enough, an effect on the carbohydrate level. You get an increased carbohydrate and, as you get high toxic level, this drops off. Just what this is, we do not know. It does not affect any nitrogen. These are the only ones that showed any difference. We test a wide variety of other things. We tested very critically the mutation rate in these (indicating) by planting these in the greenhouse and testing the seedling mutations, and there just isn't any.

Another series of tests which, again, were carried on as a monitoring program involved the monitoring of some of the water fowl and in this bunch I included the Swallow, even though the Swallow is not a water fowl; nevertheless, it inhabits the general region there that ducks and geese inhabit and the Swallow uses, thus, a similar type of food.

Actually, the Swallow uses these midges which Doctor Foster indicated are the hottest portion of the biota there in the river. The duck and geese monitoring program here has been going on for some time now. The ducks and geese come in and nest in the islands along the river there. It is a modestly used testing ground. Obtaining the activity density in these ducks and geese -- we find in the adults we have a factor of some 75,000 times the water. You must remember that these look like terribly large figures, but remember that water here is way down at a base line which is well, well below any Mary, Paul, Charlie, so even though these may appear as large figures, actually the cumulative effect is not great.

As you would expect here (indicating), the adults coming in and going out, they don't achieve as high a concentration as the young, which are relatively localized. They used the material from this area (indicating).

One of the interesting things we found was the development of the highest activity here in the eggs, which is a million, five hundred thousands times water. Just making a rough kind of evaluation of this, this is on the order of one rad per day. This is a reasonable kind of thing, then, to evaluate for possible effects of this radiation. If it were a couple of tenths of a rad per week, there is not much point in making a survey to see if it is going to make biological effects, because they would be so minute it would not pay us to make such an investigation.

We are getting into a borderline case, you might say, and it is a reasonable type of thing to survey.

For years, we have been evaluating these animals for the percent fertility of these eggs and for the percent hatchability of the eggs, and by abandoning the program for the longevity of the birds -- I don't remember the exact figures here, but the control values derived from California runs something like that -- 90% for hatchability and fertility, and our values figure-wise run about 93. They are higher, but I do not think they are statistically higher. I did not get the statistics on that. I simply got the figures, so it does not look as though, on those counts -- I might say, the banding and the longevity is the same. This is a kind of rough estimate because you are again using the sampling procedure which spreads out, and you get small returns. But on the evidence we have, there does not seem to be any effects of this dose.

This one rad, of course, as I say, is something that appears there and as you can see, the egg has a high concentration of phosphorous. The young, the decay begins to set in, and this is a peak which occurs, and then you have a decline in activity and less irradiation which is received. This is not something which is a body burden which is carried from now on.

The Swallows, even though they seem to have a less good chance of showing effects, nevertheless, the circumstances are excellent for studying the Swallows. I say they have a less good chance for showing effects simply because the activities concentrated in the tissues are lower, so they are not receiving as high irradiation dosage. Since the Swallow nests along the river banks, in great profusion, it gives a possibility for observing these longevity figures, these fertility things, with a good chance for getting a highly significant kind of figure in a native population. These Swallows simply fly along and pick up these midges.

This program, then, is just being initiated, and we do not have data for fertility, reliability, and so forth.

I would just like to say a word or two about this same thing that was raised earlier. I might just put a little diagram on the blackboard here.

(Illustrating.) If you start in with a tissue here, which is on the order of .01 phosphorous, and these are organisms here (indicating), these have a ratio of one to a hundred -- P-32 to non-radioactive P-32 per gram. This would be a means of evaluating the activity density. Your activity density, then, you will see, is related to the amount of phosphorous total, the amount of radio phosphorous and the weight of tissue in which this is contained. If these are eaten by another organism, which has the same concentration of phosphorous in its tissue, these might all be incorporated into one large body, and we would have three P-32's for 300 ordinary phosphorouses per gram, because, of course -- in other words, there has been an inclusion of material here into the -- something looks wrong here -- there has been an inclusion of material into one organism, and you do not have any concentration.

On the other hand, if these same organisms are eaten by an organism which has, say, a one percent phosphorous, we now have these three per three hundred all inclosed in .3 grams, and this activity now has been concentrated tissue-wise into a more densely packed arrangement, and under these conditions we have the situation which derives from a high phosphate activity tissue eating a low phosphate activity tissue, and you would have this concentration of activity density as you go along through the cycle.

I simply put this on here to illustrate the kind of thinking that you have to use in considering these increases of activity denses because, obviously, you must have certain maxima.

In the ducks, you have in the early stage the young animal, and the bone becomes hottest with P-32. This is a tissue which is actively turning over phosphorous in the young animal. It is likewise a tissue that has a high concentration of phosphorous. In the adult, the bone becomes inactive turnover-wise, but the breast muscle becomes active turnover-wise, and this, in the adult, becomes the hotter factor. So, here, you have the total phosphorous, or the amount of phosphorous, and the turnover of phosphorous in those organs.

DR. PEARSON: Are there one or two questions anyone would like to ask?

DR. BILLINGS: That graph you had on the jack rabbits, on the ratio -- was there any activity in the jack rabbits in the rainfall of the preceding months? It looked to me in the dry summer like it dropped, and then went back up in the fall, and then in June there was a high peak, too.

DR. HUNGATE: That high peak in June was a spurious peak due to a particular circumstance.

DR. BILLINGS: You ran it for three years, did you?

DR. HUNGATE: Yes.

DR. BILLINGS: I wonder if there is any correlation there so that it gets in the plants and the jack rabbits eat it and, therefore, they concentrate the iodine.

DR. HUNGATE: Actually, if you had a rainfall there, you would probably have a low value because the rainfall would tend to take it to the ground rather than have it settle on the foliage. This is probably adsorbed on the surfaces because the time rate of going to ground and then going into the plant -- you would have a tendency for this to occur.

DR. BILLINGS: In other words, it is just dust landing on the sagebrush?

DR. HUNGATE: Most probably.

DR. BILLINGS: Do you have a down draft so that in the winter --

DR. HUNGATE: You have a lower insect --

DR. BILLINGS: Inversion -- and in just taking it on out

DR. FRENCH: On the sheep, you are feeding definite amounts of iodine, too. What was your technique for measuring the amount of activity there?

DR. HUNGATE: The sheep were fed individual spiked pellets, and this was so that you knew what they got, and then counting of the thyroid gland was done with a standard geometry 3-probe counter, counting from three directions, centering the animal in a certain direction and getting this 3-probe counting of the thyroid. It was external monitoring.

DR. HERDE: The apparent seasonal variation shown in those sheep could be attributed to the fact that part of that iodine was, or during the spring months, concentrated in the thyroid of the glands that were in utero. These were pregnant sheep each year, I believe, that they were monitoring. Consequently, it is not an entirely breeding thing. Breeding we acknowledge as seasonal, but the thyroid activity would be not quite that pronounced had they not been pregnant sheep.

DR. HUNGATE: I think this corresponds fairly reasonably for those. The fetus does not begin to take up any activity until about the 48th day.

DR. SHOUT: I think I misled Doctor Pearson when I wrote to him and said I would like to make some comments. I think when I wrote to him I must have left out the word "here" because I meant to say opportunities for university studies in ecology at Oak Ridge.

One thing that I have been more or less struck by in the discussions is that we have in the areas of the Atomic Energy operations offices, different ecological situations with respect to environment. For example, in the case of Hanford and Savannah River and Oak Ridge, as ecological regions, about the only thing the three have in common is that a river forms a boundary line of the reservation. In order for you to appreciate perhaps a little bit more the ecological situation that we would have in Oak Ridge as compared with some of the other places, I would like to make some statements for just one moment about the geographic situation there.

What you have in Oak Ridge is a wooded area and a hilly area in contrast with Hanford and Idaho. A wooded area receiving about forty inches of rainfall. It is composed of a series of parallel ridges running about 200 feet in height. The Smoky Mountains are in one direction and the Cumberland in the other. These ranges along in a general direction -- something like this (indicating) -- from the southwest slightly toward the northeast. The town occupies a segment something like that (illustrating) and out in between these ranges lies the plant areas, and bounding the area of 54,000 acres on the east and south and, to some extent, on the west lies the Clinch River, runs in a number of bends and flows on over into the direction where it enters the Tennessee at the town of Kingston, where we enjoy the location of the world's largest steam-electric plant.

As far as the plant areas are concerned, coming from the town, one would go out on roads that lead through the valleys and ridges in more or less this kind of an arrangement. (illustrating) so that you have valleys containing old farms separated by these ridges.

In an area out here (illustrating) the K-25 area would be located, the gaseous diffusion plant, the monster that is responsible for Colorado Plateau operations to the Belgian Congo and everything else.

The Oak Ridge National Laboratory area is about here (indicating), the Y-12 electromagnetic plant area is about here.

In this section agricultural lands that are operated by the University of Tennessee for the Commission lie in about this area and on in down in here. This is a well fertilized and farm-managed area. On the other hand, there are some segments that could be used for ecological studies. For instance, there are segments up in here (indicating) where there are machine-planted pines coming from -- planted a number of years ago -- likewise, in this area.

There are old fields that are located in here (indicating). There are old fields that are located in the area in here and there are rather flat, open fields located in here that are composed mainly of the decomposed conishauga? cells.

The area in about this region here (indicating) would not be available for any kind of ecological study.

In general the wooded area, for several reasons, would not be available.

Along here (indicating) the white oak lake in Baymont, the dam, the lake itself, the creek that runs into it or the two creeks that run into it are in about that region. We have here a section where at least in part sampling studies and small plots could be set aside for various kinds of population samplings and study.

We also have in our area a number of universities with people who are interested in Taxonomy and Morphology of various groups. The thought occurred to me that it might be possible for a few facilitating regions to encourage some of our colleges and universities to come in and make use from an ecological point of view of some of the available segments of land, depending upon what they are interested in. What I was thinking about was that you have in particular colleges and universities faculty members who are going to be in these institutions on a long-term basis and they may come in and make such studies, using these areas essentially as collecting grounds. We would make no charge for that.

In connection with White Oak Lake, we have no intensive short-term studies going on. I wonder if it would be possible for some of the animal groups and plant groups growing in the lake in which we are not particularly interested or perhaps even on a more intensive basis might include whether advantage might be taken of some of the university people in making studies on their own, and on their own time, for work of this sort. Whether this idea has merit or not, I do not know, and that is one reason I would like to say something about it and let you take pot shots of it.

I have tried to include this in a statement -- a number of sheets which are on the table on the left for you to take a look at.

When I say no intensive studies are going on at White

Oak, I do not mean to overlook the fact that the Oak Ridge National Laboratory, which has control of the lake, is not carrying on work. Doctor Morgan's group, through the monitoring groups, are continuing to do sampling studies of the work. They are continuing to follow up on certain organisms that live in the lake in which they have certain interests and follow up on the water fowl.

Looking back over some of the things that have been accomplished in the White Oak Lake study, it seems to me there are a few things that more or less seem to be outstanding with respect to what little we know about them. For example, we do not know enough about the adult insects. In fact, we know only too little about the larva insects that are in the immediate environment of the lake. We are not too well acquainted with the bottom organism population. It has been my impression that our bottom organism population in White Oak Lake has been rather sparse. We have no guarantee that that bottom organism population is going to be improved, because I certainly would not stand here and say this lake is now going to be free from silting conditions, because we can never tell when large-scale construction and earth-moving operations will take place in the area which will have vast influence on the lake.

However, the rate of sedimentation is rather low, and it is running around 1 percent of the lake volume per year.

In regard to the shore line vegetation of the works, left over from the ecological survey, we have certain sampling plots that are laid out. I trust, Louis, we can find those plots again.

In regard to the fish, there is still question of the small fish -- to what extent we have small fish on the upper portion of the lake where it was impossible to do sampling that was really efficient. We don't know enough about the recovery situation, and the recovery from the silting in the lake. We would like to know a darn sight more about the migration of the fish into the Clinch River. We do not have, as far as organisms are concerned, any indication, really, of the situation in the Clinch River from the mouth of the White Oak Lake on down.

One thing that we have missed completely -- not completely -- is the question of the uptake of the mollusca in the Clinch River. One thing I might refer to in passing -- I have heard it from Thomas, Victor, Albert, people; if you remember the genes eels -- they were characteristic of the rather rapid flowing pre-impoundment Clinch -- as a genus have been wiped out by the fact that you now have the Clinch as essentially an impoundment, the Norris, the midstream section, and the impoundment that comes on all the way back to the mouth of the White Oak Lake.

The vertebrates should be fairly abundant in the areas of the -- you see lots of woodchucks, foxes, and I am sure the small vertebrates are in abundance there.

For twelve years, the area has been, in essence, a game preservation. The study of this area is facilitated now, if someone wants to do some work. It is a controlled area, and no one no longer has to have passes at the gates if he stays on the main road. Through application to us, we would be glad to make an arrangement so that AEC control would be cognizant of anybody who wanted to do biological collecting in the area and would be identified so that he wouldn't be picked up and hauled off to jail or anything like that.

I wanted to mention this because I would be very pleased indeed if you would have some comments to make on it.

DR. PEARSON: Thank you, Sam. I imagine you will have a lot of the colleges down there. We will take a five-minute break now.

(A short recess was taken at twelve o'clock.)

DR. PEARSON: This conference has certainly served a valuable purpose and an objective so far as the people in AEC are concerned. We have learned a great deal about what is going on within our own organization with which we were thoroughly familiar before.

This is the first chance we have had to get together with various people working in this general area, so I hope it has been mutually advantageous. We hope that this has provided information on at least the general aspects to some of you folks in the ecological field who have an interest in this area, and we have not left too much time, it seems to me, to take advantage of the words of wisdom of those who are in an eminent position to provide us with such words of wisdom, and for this I apologize.

AEC is certainly cognizant of the ecological implications of the Atomic Energy program. We have attempted to make at least a modest beginning towards the solution of some of these problems, bringing to bear talents in the field of ecology, biophysics, chemistry, microbiology, and people from universities and colleges, as well as from industry.

The importance that we attach to this field of ecology, I would like to announce at this time, is exemplified by the fact that the General Manager and the Assistant General Manager for Research have approved a position for an ecologist, and we are looking next September to having with us one of your own members, Doctor John Wolfe from Ohio State University to sort of pull together these ends and fill up the gaps that we folks who are pretty much unfamiliar with ecology in some respects may have overlooked, and I am sure there are many things which Doctor Wolfe will be able to tie together.

I would also like to mention that the reports from the test activities that go on at Eniwetok and the Nevada Proving Grounds that have ecological implications, as they are completed, we hope we will be able to declassify them and make them available to you. The process of declassification of a test report takes somewhat longer than, say, Doctor Odum's going out to Eniwetok and making a report, ~~and I hope some few others who will~~

be ~~going out and carrying on scientific research work will take~~ less ~~time on your report and make them much less formal~~.

About 95 percent of the work that goes on in the Division of Biology and Medicine is on an unclassified basis. One of our problems is making available to scientists in the field, to the press, and to the lay public, factual information on the whole problem of biological effects of radiation. This is pretty well illustrated by a little incident that happened a few weeks ago in connection with the meetings of the Federated Societies in San Francisco. Following those meetings, and editorial appeared in the San Francisco Chronicle commenting on the outstanding work that had been done by an assistant professor at Western Reserve University. They emphasized the fact that this work had been done under meager support from the university, as contrasted with little ~~help~~ information that comes out of the Atomic Energy program with our restricted ~~budgets~~ policies. What this man referred to as being reported to the ~~Congress~~, to show what little control we exert over it -- he reported his results and the reporters there were not even aware that it was part of our program, and I think this could be multiplied over many times. We certainly hope that all investigators under the Commission have freedom to publish their results. We do solicit your aid in making information available to us, and making available facts to the public and to the press. I think that will allay any of the fears and concerns that have arisen in the minds of the public.

There are certainly more problems to work on in the field of ecology than there are personnel, scientists, and funds available. I think this is true in every scientific field that we have, and biology and medicine and the physical sciences as well.

We hope that this will only be the beginning of a closer liaison between the scientists working in the Commission, in the AEC laboratories, and the members of the Ecological Society and other groups which we have mutual interest in.

We are supporting biological science alone to the extent of some 200 projects in universities and colleges. Most of those are on joint participating basis. They are on problems that are of interest to the colleges and the universities and to the Atomic Energy Commission. We are able to make some funds available toward the support of those programs. Even in training, I think we make an appreciable contribution. It is estimated, not in biology and medicine combined, but just in biological science, we are providing funds to enable 200 men and women to pursue graduate research work under our grants. We hope that this can be continued.

For this, we are indebted to the scientists attending the universities and colleges and our research laboratories over the country.

Those are the few words that I have to say and I hope that you folks who have been so generous in taking time out from your business and academic research activity in colleges and universities, and we hope that we can have some suggestions, criticisms, constructive, and indications of the groups that we have, and I turn it over to you now. Let us have some suggestions from you folks. We have been doing too much talking in our group and we would like to do a little listening and get a few words of wisdom, which I know you have.

DR. PARKS: Mr. Chairman, Madam Patrick, and gentlemen: In September of last year, the Ecological Society of America, meeting at Gainesville, Florida, at the Albert Isaac Beta Sam meetings, appointed a committee on the effect of radioactivity on natural populations, appointed on this committee Doctors Billings, Ketchum, Odum and myself. This committee was invited to attend the conference, and along with a number of our distinguished coworkers, when they got here, they held a short meeting on the night of the 19th and decided that we should not operate in a vacuum -- we should take advantage of our distinguished colleagues' presence, so we asked them during the course of yesterday if they would meet last night, and we all assembled after the banquet and drew up some remarks.

The people present at this meeting were: Doctors Billings, Ewell, Patrick, Ketchum, Odum, Shanks, Pitelka, Curtis, Krumholz and Park.

I shall be quite brief. Operating on the theory that the longer the spoke, the greater the tire, so you need not worry.

In the first place, I should like to thank the Division of Biology and Medicine of the Atomic Energy Commission on behalf of the Ecological Society of America for inviting us here and allowing us to listen to the fine work in progress. I am sure we are all gratified.

We should also like to congratulate the Atomic Energy Commission on acquiring an ecologist on the biology and medicine staff here, and we are very happy that it was one of our members, Doctor Wolfe.

Secondly, we feel that we must have a much more intensive training of radiation ecologists, young people who can take their part in what appears to be a long-term investigation.

Thirdly, we believe that there should be an intensification of the research program on the basic principles of community ecology with reference to radioactivity.

Fourthly, in view of the possibility that public power reactors will be located in various parts of the country, we recommend establishment of research programs on major community types not now under intensive investigation such as grass land, Pacific Northwest Forest, estuaries and American Tropican Rain Forest, and Chapparal.

Number five, we recommend that more attention be paid to joint programs in ecology and genetics. We feel that there is a good deal of gold to be mined in such a program, such as, for example, the microbiological aspects in community metabolism.

Finally, number six, we wish to assure the Division that the Ecological Society of America is willing to help in any way that the AEC suggests.

DR. PEARSON: Thank you very much, Doctor Park. We appreciate these kind words and these recommendations. I feel so strongly about how useful they are; I feel it would be helpful if members who met last night could put these recommendations down in writing.

DR. PARK: These are late night notes, but I will try to work them up.

DR. PEARSON: Doctor Wolfe, I trust that you have taken especially careful note of these suggestions and I trust they will be on hand, on your desk, the first morning you report to duty, and we will be looking for you to implement them within the confines of available funds.

Are there any other comments that might be made here?

(No response.)

Gentlemen, I again want to thank each and every one of you for your participation, and I hope this is only going to be a beginning of closer liaison and closer contact with not only the ecologists we have from outside of the Commission program, but that many others also, and we welcome suggestions and/or comments either individually or from the Society as time goes on regarding areas of work, the gaps that we have and steps which we might take to fill in our knowledge, and we certainly know that we have many of them.

If there is nothing more, we will be adjourned. Thank you all very much.

(Whereupon, at 12:20 o'clock the conference was concluded.)

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