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AND STUDIES OF MARSHALL ISLANDS

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ORGANIZATION & MANAGEMENT

UNITED STATES ATOMIC ENERGY COMMISSION

DIVISION OF BIOLOGY & MEDICINE

CONFERENCE ON LONG TERM SURVEYS AND STUDIES OF MARSHALL ISLANDS.

Room 1201,
Temporary 3 Building,
Washington, D. C.
Monday and Tuesday, July 12-13, 1954

The Conference convened at 9:00 o'clock a.m.,

Dr. John Bugher, Division of Biology & Medicine, Chairman.

PRESENT:

DR. JOHN BUGHER
DR. C. L. DUNHAM
DR. G. DUNNING
DR. W. CLAUS
CDR. E. P. CRONKITE
CDR. R. A. CONARD
MR. GEORGE IMIRIE
CDR. H. ETTER
DR. V. P. BOND
MR. H. HECHTER
DR. C. SONDHAUS
LT. R. SHARP
LT. COL. L. E. BROWNING
MR. P. HARRIS
MR. S. H. COHN
MR. J. HARLEY
LT. SHULMAN
CAPT. YARBROUGH, MC, USN
CAPT. ENGLISH, USN
LT. CHAPMAN, USN
LT. LOONEY, USN.
MAJ. HANSEN.

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P R O C E E D I N G S

DR. BUGHER: It falls to me to start things off. First of all, according to our records, everybody is Q cleared except one gentleman who in the short time we could not put through a special clearance. On the other hand, while we have to observe the technicalities of the situation, AEC would not have them communicated restricted data. So we may have to observe a certain silly routine at times. I don't think actually we get into much in the way of restricted data. Among the military we can discuss restricted data anyway. We may have to observe that minor formality since there was not time to arrange a special clearance.

As far as the purpose of the conference, which you all know, I would repeat that our major purpose is to assist in bringing together all the pertinent data and executing the necessary analyses of that data of the study of the persons who were injured by the fallout of the March 1 shot among the Marshall Islanders. That includes also certain task force personnel who were exposed at that time.

The situation of course is a unique one as far as past history is concerned, because we have no similar episode previously in which whole body gamma radiation combined with extensive skin contamination has been observed in a large group of people resulting from mixed fission products. The only other group of people were involved in the same

detonation, the crew of the Japanese fishing craft, which was about 50 miles north and somewhat west of Rongelap Atoll, and was caught in the fallout, apparently being to the north of the main line of concentration.

So that although these people do not come into this particular discussion to any great extent, it appears that the Japanese had somewhat of the same magnitude of whole body exposure as the Rongelap people did, but with somewhat more skin lesion as a result of a longer period of contact with the skin surface, due to poor washing, fundamentally.

The larger group of people are those with whom the special medical team dealt. This report, which is being evolved, will be an extremely important one from the standpoint of the medical information and will be a guide unquestionably in many of the military considerations of the effect of radioactive fallout material.

There is another large element in the picture and that concerns the international relationships which have been thrown into considerable focus by this event. During the last three days of last week, I had to sit with the United States Delegation at the UN because this matter is now a subject of rather violent and acrimonious discussion in the Trusteeship Council. The United States under the trusteeship agreement of 1947 holds the Pacific Islands in trust, among them being the Marshall Islands. That mandate

is administered by a special trusteeship organization, actually under the Department of Interior now. It was formerly under the Navy. As such, it is responsible to the United Nations and under the original terms, the United States held the right to withdraw such lands as might be necessary for strategic and security purposes, but beyond that, to administer the whole area for the benefit of the people concerned.

Bikini, of course, was separated from the islands of free access before the trusteeship agreement was reached. Eniwetok was separated about that time. But in view of the commitments that the United States entered into voluntarily at that time, there was unanimous approval of the trusteeship by the United Nations Trusteeship Council.

Now we find that this is being used as one of the weapons in the war of maneuver. The Marshall Islands petition, which was sent in by a group, particularly at Majuro, is used as a club now to establish a case that the United States has been false to its obligations as a trustee; that it has deliberately destroyed lands belonging to the people governed; that it has injured them in a series of experiments where, quoting various Congressmen and high American officials, we documented that not only did the meteorologists find themselves unable to predict anything, but the scientists were unable to anticipate what would happen,

being greatly astonished by the results of what they had put together.

This is the theme being pushed by the Communist group particularly, largely comprised of the Soviet Union, India and Syria at the present time, in an attempt to get a resolution adopted which is condemnatory of the conduct by the United States of the trusteeship of the Pacific islands.

One of the strong points in this situation is that in fact nobody did die, and all the people have apparently recovered very satisfactorily. Movie films are available in New York showing the relocation of the Rongelap people on Madro Atoll in a very beautiful setting in which the new houses are located, the people obviously happy and healthy. The Uterik people also shun returning to their homes. So far I think that film has not been shown because there was an agenda wrangle immediately which would defer this film showing until later in these hearings.

So that is the atmosphere which exists in the UN in which this whole thing is being used as a diplomatic weapon. We are fortunate, indeed, that the prompt response of the medical groups concerned was so effective in insuring the medical care of these people, and that the whole thing has turned out so happily, as far as the welfare of these people is concerned, apart from the human concern that one does not like to be responsible for injury to anyone.

There is a very significant international issue involved here, with the fundamental argument concerning the rights of the trustees.

So that is somewhat the atmosphere in which we are working, and one of the reasons why it is important to get this report, not only in the very best scholarly form that we can achieve, but also to do so in a minimum of time. Eventually I hope we can also declassify the report, so as to have it published as a piece of medical literature with much medical importance to Civil Defense, to people interested in radiation injury, and a lot of other things. So I think we will realize that everybody in the government concerned with this problem is really very grateful to the group that carried on this investigation so effectively and achieved a very high order of scientific cooperation which existed throughout the program. Everyone who was asked to do something did so with very good will and enthusiasm, and turned in the very best job he could. There was no scrambling for position or notoriety in any way. I think it was one of the most satisfactory efforts that anyone could wish for.

You realize, of course, that the study and the report which you will produce is only the beginning; that the report which is in progress of preparation is only Chapter 1 of a larger volume whose termination cannot yet be foreseen.

In other words, these studies will have to continue for an indefinite number of years. We hope that even after several years, that we will see pretty much the same group of people still interested in this problem, and actively working on it.

We will some time later get to the means by which we hope to carry on the program and to get on with the studies through the succeeding years. Despite the fact that everybody has recovered now, and looks hale and hearty, naturally we have certain reservations about what may happen in the course of 15 to 20 years with skin areas, which have been affected by as much radiation injury as occurred here, and whether or not we will find spermocel carcinoma, one of the long term sequellae of the lesions. I do not know. It is a matter of speculation. But obviously it is one of the things that may give concern.

Captain Yarbrough, have you any additional comments that you care to make at this time before we get everybody to work?

CAPTAIN YARBROUGH: I have nothing particularly, Dr. Bugher, except that this particular incident has brought to light the fact that it is quite difficult to keep together personnel in the form of a unit that can be quickly activated and transported to distant places for studies of this kind. I am sure that all people in the military at this

time realize that there is a critical situation with regard to personnel, more critical perhaps among medical personnel.

This was a subject of considerable discussion at NRDL some two weeks ago, where we have made some effort to at least keep track of personnel so technically trained that with some degree of efficiency in the matter of time that we can pull together some of the units again.

Essentially we have realized that this is a rather mountainous problem. I don't know how far we would get with it, but we intend to continue efforts to at least keep locators on these people where we can requisition them, or request that their services be loaned for solution of such happenings as the recent Marshalese incident.

DR. BUGHER: Thank you very much. We have a plan to split the various people up into study groups, and give them about an hour and a half to get their facts and figures in order, and then return to the meeting. We are just a little bit behind time, but we will try to make it as quick as we can. In other words, if you can cut a little bit under the hour and a half, that is to the good.

Did everybody get these agendas or did nobody get an agenda? I am afraid I am the culprit here. Then one group is to consider the estimate of external dose, with Gordon Dunning as group leader, Sharp and Sondhaus to work with him. You will probably want to get off in some quiet

place if you can find it, and see what you have.

Then for the hemologic findings, the dermatological evidence, and the general clinical studies, Bond as group leader, Conard, Cronkite, Browning, Dunham and Hechter as members of the group. That group will need a little larger room, I take it.

The third group for the nature of the fallout, internal deposition, urinary excretion, body burden of the long term hazard, we had Merrill Eisenbud as group leader, but I have not seen him. He is not here. I understand he is on vacation. Walter, would you act as group leader for that discussion?

DR. CLAUS: All right, I will try.

DR. BUGHER: Harris, Cohn, Harley and Imirie to join in that.

The rest of us -- if there are any "rest" -- I believe there are some more left unattached -- can discuss as informally as they wish these matters.

Now, as to location, I would suggest that perhaps Bond make use of my office for his group.

DR. CLAUS: I think perhaps we can use my office.

DR. BUGHER: Gordon Dunning and that smaller group could use Dr. Dunham's corner. Then we will break up for that individual group work until 10:45.

(Thereupon at 9:45 a.m., a recess was taken until

11:15 a.m.)

DR. BUGHER: Dr. Dunning, are you ready to report on your group on the dosage problem?

DR. DUNNING: Yes, we are.

ESTIMATE OF EXTERNAL DOSE (SURFACE AND WHOLE BODY)

DR. BUGHER: Would you care to come up here where you have a blackboard and chalk?

DR. DUNNING: No, I don't think so. I would like to call on Dr. Sondhaus to present some of his ideas here first, and then I will try to summarize the committee's findings after we get through with all the "ifs" and "buts", and "whereases", and we will try to come up with specific numbers.

DR. BUGHER: That is what we want.

DR. SONHAUS: What we have considered in NRDL were several points concerning the data on the external doses. The first question was the calibration of instruments used. We have quite a bit of conflicting data to some extent. We chose for the most reliable that of the RAD SAF SCOVEY group on the 8th of March. This was done with more adequately calibrated instruments, and we have data on the performance of the T-1-B.

The second question was the energy distribution of the fallout gamma radiation, and its effect on the meter response. Concerning this, we have some spectral

distribution data, which was taken from fallout samples. On this we performed a calculation which takes into account the degradation of the energy due to scattering in the air, due to the distribution of such fallout on an infinite plane.

The next question was the rate of decay of the fallout mixture. Here again there is a certain amount of experimental data which seems to indicate that two exponents should be used over the period of time in which we are interested, namely, a .8 or .9 exponent during the first four hours to four days, and from four days until 25 days, roughly an exponent of about 1.6 seemed to fit the combination of the Neptunean and fission product combination.

The best estimates of the dose rate at the time of evacuation were computed, using these factors, and the time of arrival of the fallout, and the duration of the fallout with the remaining questions considered. Since there is very little accurate data, except in the case of Rongerik for the time of fallout, the best estimates possible were made, and doses were calculated on the basis of either a very short fallout or the longest possible fallout that could be assumed to have taken place consistent with the time of commencement and the dose rates which were read at later times by the survey instruments.

Based on these considerations, we have several sets of doses which are in substantial agreement with Mr. Sharp

and Dr. Dunning, and I think I will leave that for him to summarize.

There are minor differences in the approaches and also the numerical values of the parameters, but these do not seem to lead to any sharply different values. I think that is all I have to say.

DR. DUNNING: As the group can appreciate, there is a great deal of uncertainty in trying to estimate the numbers. Different instruments were used by different people at different times, and different places. Some instruments were calibrated recently before use, some were not. In addition to the actual surveys taken, of course, theoretical computations were made, such as the ratio of formation of Neptunium and fission products for this particular device, being of the order of .8, for example, and then trying to estimate what the relative dose rates would be at different times after detonation and trying to come up with an integrated dose for the times of interest.

In the case of Rongelap natives, the fifth or sixth hour after the fallout to the time of evacuation, there was still some uncertainty as to the exact time of initial fallout, even uncertainty as to Neptunium contribution, uncertainty as to where the people were. We had different dose rat e readings at different parts of the island. Where were the natives? How long did they stay there? Different

dose rate readings inside and outside, but in the case of natives that was not so important, because in the huts the dose rate readings are almost as high as outside.

In the metal shacks for the Air Weather people at Rongerik, this was not so. We have such phenomena as people lying on their mats for their sleeping at night, the fallout material blowing into the huts, thoroughly covering the huts. The uncertainty of the contribution of the soft gamma. As you know, most of these measurements are taken by such instruments as the T-1-B or T-39, where they have essentially a cutoff value of some 70 to 80 KEV. You are missing your soft gammas and your beta.

Then I think there is one phenomenon that was not discussed very much, but which may be important. Unfortunately we cannot evaluate it. We have experienced this phenomenon in the Nevada test, for example, in Shot No. 9, in the Upshot-Knothole series. When you plotted out the dose rate readings with time, you get a definite hump. In that case the area under the curve was not too great or significant. But out in the Pacific, where you certainly had a relatively high concentration of activity in the air, lasting for probably many hours, you might have a significant contribution from sky shine that has not and probably never can be accurately evaluated. This will not show up in any of our dose rate readings.

I told you we are going to have a lot of "ifs" and "ands" but we are still going to come up with numbers in the end.

So taking all these values, the values arrived at theoretically, values taken by the survey team, using the various exponents and so forth, it would appear that the best estimate we can make for the Rongelap natives was about 150 r. This is whole body gamma. This does not include soft gammas, nor the betas. At Elinkani, the data are less firm, but be that as it may, our estimate is about half, or in other words, about 75. Utirik, again, is less firm than Rongelap, but we are not quite so concerned that it is less firm inasmuch as it would appear that the value is about 15 r. In other words, we are not concerned in terms of any biological hazard.

For the Air Weather people on Rongerik, again we have a whole series of survey data, as well as the film badges. After going overall the survey data taken by various instruments at various times and different people, and what have you, it would appear that the firmest data is to go to the film badges. As you know, some of these film badges were in an ice box and some were carried. But for most of the personnel, the film badges were between 40 and 50 r. For one film badge, representing three Army personnel on the north end of the island, their film badge

read 98 r. However, in checking with their actual movement, it would appear that these three Army personnel did not remain there all of the time. In fact, they were back at the other end of the Island and inside the metal buildings for an appreciable amount of time. Therefore, the 98 r probably represents the upper limit of our estimate.

I would like to give you very firm figures, but I think you can appreciate the problem, that this is about as firm as you can get. In fact, maybe we already have stuck our necks out too far.

DR. BUGHER: Do you have any estimate of the range here within which the dose probably falls? In other words, anything that would resemble a standard error?

DR. DUNNING: I was afraid you would ask that. Frankly, I don't. As Dr. Sondhaus has indicated, they came up with a range. I have deliberately not put one in, because I was afraid people would read into that an implication of a standard deviation. I just don't think the data are firm enough.

To make the matter still worse, on this agenda, we give an estimate of surface dose. This is getting into the problem among other things of beta-gamma ratio, which is, of course, exceedingly difficult to evaluate.

If I may just mention, we have some very limited data on the Japanese fishermen, where we have some material

taken from aboard the ship. I repeat it is very limited. But be that as it may, making certain assumptions, it would appear that if the fallout material were to remain in contact with the skin of the Japanese fishermen for one hour, that something of the order of 10,000 REPS would have been delivered to a depth of 7 milligrams per square centimeter. And if it remained longer than one hour,, which it probably did in the case of the fishermen, on up.

In the case of the natives, we don't even have that much data to go on. The fact that the natives were lying down during the evening of March 1, probably contributed to exposing a larger surface of the body to the soft gammas and betas. But to come up with any firm number as these natives received so many REPS, we felt we were unable to do so. The data woulf certainly strongly support the conclusion that these lesions were due to radiation. Of that there seems to be little doubt. But exact doses I just cannot say.

DR. BOND: Can you give us any estimate off the amount of gamma below KV cutoff?

DR. DUNNING: Yes. Dr. Sondhaus, will you tell us that?

DR. SONDHAUS: Yes. I would like to say that the estimates we have do include the contribution of gamma below 100 KV in the initial spectrum which we have. Approximately 8 per cent occurred below 80 KV. When you

translate this into terms of dose, including the effect of scattered soft radiation from the higher energy lines in the spectrum, the percentage of the dose is still in the neighborhood of 16 or 17 per cent. The T-1-B cutoff, I have approximated as best I could with data which were taken in our laboratory, both recently and a couple of years ago. The sensitivity falls down quite strongly at a range of 60-50 KV. But even making a generous estimate of the correction factor, that must be applied to a dose in this energy region, the overall correction factor for the T-1-B seems to be close enough to unity within the limits of the error we can specify here.

The reading of the instrument, I think, could be accepted as being accurate. Since the proportion of the dose in this region is small, I do not think that we need to consider that it departed materially from the total doses estimates we have here. The total dose would appear to have resulted from three general ranges of energy. One in the 100 KV to 200 KV region of about 17 per cent. The majority of the dose in the 600 to 800 KV region of perhaps 50 per cent. And about 15 to 20 per cent in the 1.5-1.6 KV region, with the balance of the dose spread out between these three humps. So that the exposure could probably be treated as a composite of an exposure to each of three separate radiation energies.

DR. DUNNING: Let me ask you again for the benefit

of the group, this material that you used in this experiment was taken from where?

DR. SONDHAUS: The material for this spectrum determination was a cloud sample which was flown back to the laboratory, and the spectrum taken at four days. Naturally this is subject to a great deal of question as to whether the fractionation was the same in this portion of the cloud as it was on Rongelap, for example. Also, there were other samples taken on some of the rafts which were at various distances out from the lagoon. I think the furthest one was not more than 50 miles away. We don't have any direct spectrum determination on Rongelap soil samples as far as I know at this point. There are some, but this was the first spectrum taken at the laboratory.

DR. DUNNING: I wanted to bring that out. We did discuss it in our meeting. I think we must realize that we are talking about cloud sample data, and not the actual fallout. What the difference would be, I am not prepared to say. I think this should be remembered.

Also, the lower value of exponent of .8 to .9 was from material close in at the Bikini Lagoon. This may not be the same found at 150 miles away. This is a throwup.

DR. BOND: What was the calculated value of dose for the Americans? What was the dose calculated in the same manner as for Rongelap?

DR. DUNNING: As I recall it ran between 60 and 76 from the survey team readings. As I say, after considering so many factors, one, that most of these readings are taken nine days later, and when you start extrapolating ack, if your exponent is off, you can be off quite a bit. Other things, how long the people stayed there, et cetera. It seemed that the film badges worn by personnel, and there were three, might be as close as one might hope to come.

DR. BUGHER: Do you think, Gordon, that the relationship between the film badge figure, which we are accepting for the Air Weather Service people, and the calculated dose which was mentioned here would also hold for the actual dose that would be shown by film badges, and that calculated for the Rongelap people?

DR. DUNNING: I thought of that, Dr. Bugher. I don't have the firm answer. I would like to point out this, however, that the Air Weather personnel had metal barracks, and they were indoors an appreciable amount of time before evacuation. The attenuation of these metal barracks -- if you will just be patient here for a moment -- here is one with a factor of two, and so forth. So one might expect that the film badges would show less than the calculated.

DR. BUGHER: So the calculated figure here of 60 and 75 did not include a factor for the buildings?

DR. DUNNING: No, it does not. I think that is

about the best we can do.

CDR. CONARD: Do you have any data at all on the beta spectrum?

DR. DUNNING: I don't. Do you?

DR. SONDHAUS: Not on the beta spectrum. I think we might be able to make some estimates.

CDR. CONARD: That would be nice to know from the point of view of skin lesions.

DR. SONDHAUS: About 80 per cent of the fallout was Neptunium-uranium; in four days we could make some estimates on that basis even though we do not have a complete spectrum.

CDR. CONARD: Anything you could give us on that would be helpful.

DR. SONDHAUS: Surely.

DR. BUGHER: It might be pertinent also, since you mentioned the figure of 10 REP to skin from an hour's contact, to give the assumptions on which that was based with respect to the amount of material on the skin, that is, the thickness, and so forth. It is probably true that the estimate there was a much heavier deposition than actually occurred.

DR. DUNNING: If you recall, I tried to be very cautious and indicate that the estimates were based on very limited data, and on some very shakey assumptions, you

might say. One assumption is that the fallout occurred in the third hour after detonation. The second, and probably one of the most important, that this material was spread out to provide a thickness of .01 of a gram per square centimeter about 40 micron thickness. Here is one that is wide open. The rest of the assumptions, I think, are not too far off.

DR. BUGHER: That was also based on the activity from John Harley's figure.

DR. DUNNING: Yes, sir, there is another very important assumption that would have to be looked at closely. That is the specific activity was 3.1 times 10 to the 8th disintegrations per minute per gram on April 7.

Then it was just a matter of extrapolating -- I say just a matter -- back again, assuming 1.2, and then you come up with a specific activity on the third hour after detonation. Then with the assumption of so much material deposited, and using Rossi's calculations as to self absorption in the skin, we come up with the calculations as to the dose that 7 milli grams per square centimeter below the surface.

DR. BUGHER: What was the activity per gram on that basis?

DR. DUNNING: 3.5 times to the 11th. I believe this is the figure we want. If you accept 1.2, that is about the answer. That is in terms of disintegrations per minute per gram.

DR. BUGHER: That helps. Then we have from the Japanese biophysicists bychance from Osaki who got on that ship on the 16th, 16 days afterwards, and his estimate was a half curie per gram from the materials he scraped up. He had first access to it, and he scraped some up and took it home. That information never appeared in Tokyo. He included it in a memorandum, you will recall, in an addendum to a letter his wife wrote to President Eisenhower. That is where we got our information. That seemed to check in pretty well considering the uncertainty of exponent, and the variation of instrument, and everything in a factor of two.

MR. IMIRIE: What would be the calculated hard gamma dose? In other words, of this 10,000 REP, how much would be equivalent to a T-1-B reading?

DR. DUNNING: You tell me, and I will pin a medal on you. That is a $64 question, and I am sure you know it is a question. Beta-gamma, you just can't come up with a number as you well know. Is it beta-gamma ratio in terms of function of time after detonation, and what you are interested in is the dose delivered to the 7 milligrams per square centimeter below the surface, and how can you make this relationship? As you know in the Greenhouse work, they found a beta-gamma of 157 to 1.

This does not represent the dose of roentgens. Mathematical calculations would indicate something like 130

to 1, but again we don't know. I suspect it is lower than that. How much lower, I am not prepared to say.

DR. BUGHER: Does that represent what you have been able to squeeze out?

DR. DUNNING: That is all we could get out of that lemon.

DR. BUGHER: That is very helpful. It is always startling to find out what gaps in the physical measurements seem always to exist, even after maximal efforts have been expended. No matter how much we have, we always want more, and wish we had something additional to what we do have.

I don't know whether anybody did a complete beta-gamma ratio curve for any standard instrument through this period.

DR. DUNNING: No one did that I know of.

DR. BUGHER: We will have to recruit the Marshallese to do some of these things. Thank you very much, Dr. Dunning. It is good to see some figures here together with a discussion with the universities which are involved in them so that we all remain aware of the inherent area of what I call disagreement, which would not represent disagreement at all, but simply a range of estimate from the data that we do have.

Are there any further questions or comments on this side of the story?

(No response.)

DR. BUGHER: I think we all seem to be disposed to accept the estimate of 150 roentgens for the average dose for the Rongelap people, with some uncertainty as to that. The uncertainty is not specified, but I should think it might very well be of the order of 25 roentgens up or down.

DR. SONDHAUS: I should like to add one thing, Dr. Bugher. In conjunction with considering a figure for the dose under these field conditions, we should bear in mind that this quite possibly needs to be interpreted in terms of the geometry of the exposure. That is to say, that when a laboratory experiment is performed on an animal, perhaps with the dose being divided between both sides of the animals with a bilateral exposure or something of this sort, the depth dose characteristic is quite different than in a uniform 360 degree exposure, such as an individual would receive while standing on an infinite plane. We have begun some preliminary estimates of this at the laboratory by trying to simulate a 360 degree exposure with a cylindical phantom. The first figures are rough, but one might be led to the conclusion that perhaps a 40 per cent increase in the dose in terms of a laboratory exposure would result from the same skin dose, but with the radiation from all directions. That is, the exit dose would certainly be the same as the entry dose in this case, and the volume dose

might be put in terms of a higher figure, differing by about this much. I am not prepared to say anything more than this right now. I do believe the point should be made that the figure, say, of 150 does not necessarily imply the same biological effect as 150 r divided equally between both sides of an experimental animal. This should be taken into consideration.

DR. BUGHER: Are we not talking about roentgens at least 5 centimeters in here when you speak of the whole body dose of 150 rather than superficial area?

MR. HARRIS: It is air dose.

DR. SONDHAUS: We have taken the readings of an instrument in air and integrated with them. So what we are specifying here is an air dose to which an individual was exposed.

DR. BUGHER: You are not calling it as a whole body exposure.

DR. SONDHAUS: That is right.

DR. BOND: It is usually expressed as a whole body dose. It is a dose in the air given to the whole body.

DR. BUGHER: Has the committee any estimate of dose at 5 centimeters or 10 centimeters or any figure?

DR. BOND: It is under investigation, but it has not been completed. It is being worked on at two laboratories.

DR. SONDHAUS: The 5 centimeter dose will certainly

differ between these two cases if you normalize to the same air dose between the laboratory exposure and the field exposure.

DR. BUGHER: Do you think with your gamma spectrometry you will come out with some sort of estimate here?

DR. SONDHAUS: That is quite possible, I think.

DR. BUGHER: That is the essential thing. We are not only uncertain as to 150 r; we do not say that the individual's bone marrow or organs or spleens got such radiation; is that right? How long do you think it is going to be before we do come out with a pretty firm estimate?

DR. BOND: I think before the final report, Dr. Bugher, we are working with it on our x-ray machines, and the cobalt source that is ideal for solving this problem, and it will probably be solved before the final report is in.

DR. BUGHER: Obviously it is a very important figure to have, and as precise as may be possible. That is a very helpful comment.

Are there any other comments or questions to ask of this committee? If not, we pass to the second group report by Dr. Bond on the clinical aspects which include the hematologic things, as well.

HEMATOLOGIC, SKIN, AND GENERAL CLINICAL STUDY

DR. BOND: I think perhaps we had less uncertainties in our material than the dose group. Obviously the dosage

problem is of tremendous importance to us in an effort to correlate what we saw with physical estimates of dose. I think we will simply enumerate the major conclusions that we wish to draw from this study. If there are comments, I would like to have them at the time, so please interrupt.

I think as far as systemic effects are concerned, the only symptoms that could be ascribed unequivocally to radiation was the early appearance of mild subjective symptoms. This was nausea that appeared to a large degree in the Rongelap people, and with considerably less degree in Ilinkela, and not at all in the Uterik or American groups.

These people were treated identically. They did not know, so to speak, the correct answers to the questions that were put to them. Different interrogators obtained the same results, so we feel that this is a real thing, and probably ascribable to radiation.

Aside from this, there were no other clearcut constitutional symptoms ascribeable to radiation. There was no diarrhea or other classical symptoms of whole body radiation damage. The instance of cold diarrhea and so forth was equal in the different exposure groups.

There is one possible exception to this statement. Abnormal menses were observed in two women in the Rongelap group. Whether this can be ascribed to radiation is a considerable question.

An additional point is that it was impossible for the observers to distinguish among the various groups with regard to the activities, that is, the spontaneous activities they carried on. That is, apparently they played and engaged in the same amount of activity throughout the period of observation.

So much for the constitutional symptoms. As far as the skin lesions are concerned in these people, there was early appearance of itching of the skin, itching and burning, and here again a very large instance in the higher exposure group, less in the lower exposure groups. There were no further symptoms until the development of the lesions which I will go into in a moment.

The question has been brought up as to whether these are beta lesions or chemical burns. I don't think we need to dwell on that except to state that it is the very definite feeling that there is no possibility that they are chemical burns, and they were due to exposure to ionizing radiation.

The second large point we would like to make is that these were contact burns and were not derived from a field of radiation. It was only in areas where there was actual contact of the fallout material with the skins that the lesions developed or in areas where there was a chance for the fallout material to be directly in contact with the skin.

This is borne out by the fact that in general where clothing covered the body, even a light dress in the case of the women, burns did not appear. It is also borne out histologically by the fact that there are islands of norman tissue in between the several affected tissue, indicating the particular nature of the deposited material, and the fact that it was deposited material that was responsible for the burn.

One thing that is not clearly worked out, and I don't know how to explain this. Apparently there is some discrepancy or difficulty explaining the order of appearance of lesions. Generally speaking, the lower the dose, of beta radiation, the later the lesions would appear.

This in general was not entirely the case with these individuals. The feet, for instance, showed very severe beta lesions. The surface of the anacubicle foci, the anterior surface of the neck, showed beta lesions. The scalp and the feet where the skin presumably is thicker sometimes lesions appeared later, and were more severe than in the case of the areas with thinner skin.

Also the order of appearance of these lesions in general was different from some reports in the literature. Also this was apparently a monophasic response as we got a single appearance on approximately the 14th day. There was no evidence of erythema or other skin damage. This

differs from previous reports in the literature, but may be at variance. This may be explicable on the basis that these people did have dark skins, and the darkness of the skin obscrued the early response.

With regard to the severity of the lesions in terms of incapacity to the individuals, the lesions in some of these individuals were painful and of sufficient severity that under all ordinary circumstances, these individuals would be admitted probably to the sick list.

It was the clinical impression in general that these lesions were quite superficial in nature. We are unable to determine whether the explanation for this is on the basis of the total dose received or whether it may be due to the energy of radiation, that is, with lower energy betas one might expect more superficial leasions.

In general the severity of the lesions observed correlated well with the amount of fallout presumably encountered by the individuals. That is the Rongelap people had the most severe lesions, the Ilinkila with less fallout and less total dose had the same type of lesions, but less severe, and generally later in appearance, and healed more rapidly.

With regard to the loss of hair, again this apparently occurred in areas where material was actually deposited on the scalp. It was spotty in nature, and

presumably was not the result of a field of gamma radiation. As I say, it was associated with actual material deposited at the site. It is difficult to arrive at a dose biologically, that might have caused this. However, the figure has been set at the upper limit occurs and recovery is possible, is approximately 700 r since the hair did grow in on the individuals later. We can presume that the upper limit might have been of the order of 700 r.

I think it is worthy of note to state that in general the lesions required no special treatment of any kind. No so-called specifics were used. Healing in essentially all cases was entirely satisfactory. Also, even in the most severe cases of skin damage, there was no systemic manifestations that could be attributed to the skin damage.

With regard to the prognosis of the skin lesions, here again it is essentially almost anybody's guess. There are a number of opinions on this. I think it is fair to say that clinically with fairly large doses of radiation that recovery has been apparently complete, and that we can be optimistic probably about the ultimate fate of these skin lesions. However, because of data in the literature indicating later breakdown in these lesions, and carcinogenic changes, it is necessary that we retain a guarded prognosis and an attitude of watchful waiting.

With regard to the skin lesions, I would like to

mention the nail pigmentation. I think most of you are familiar with that. That appeared at the base of the nail, a bluish discoloration. Apparently this was an aberration of pigmentation. I think it has been reported only once in literature in a single individual.

With regard to the hematological findings in these individuals, here we see no justification throughout in treating the individuals other than as roots exposed to the same dose of radiation. There is no physical basis upon which to segregate them. Even in the case of the Army boys, it appears that their activities are not too different from the remainder of the Air Force boys, so these were treated as a group as with the other exposure groups on the various islands.

A word as to the controls that were used for the hematological studies. They are, I think, at least as good and probably considerably better than most clinical studies of this nature. That is, the control groups were matched with respect to age and sex to the actual exposure groups. So that while we must recognize that strict comparisons are not valid, as we cannot state definitely that they are homogeneous samples of the same population, still we feel it is an excellent control group, and will serve very adequately as a guide.

For the control for the native groups, we went to

Majuro and obtained a very large control group, as I say, comparable in age and sex to the exposure group. For the Americans we obtained a group of Americans that had been in the mid-Pacific for a period of at least two months, and at least to that extent were comparable to the Rongerik Americans who were out there approximately two m onths when exposed.

The control populations -- the controls for the Natives were broken down as regards to age and sex. They did show a difference in response as a function of age. In general as far as the leukocyte-lymphocyte count is concerned, the children below five were different than those above five. With respect to platelets, individuals below 15 were significantly different than those above 15. So they have been broken down into these age groups, and I will speak only of the adults unless I specify children.

In the large exposure group in the Rongelap, there is no question as to the definite change in the hematologic picture. They did fall and remained at a fairly low level for a period of weeks, and there are indications that they were returning towards normal when the group studies were completed.

The change in total white count was reflected in both the leukocyte counts and lymphocyte counts. The lymphocyte counts fell immediately to a level of 2,000 cells and remained throughout the duration of study, and no evidence

of return to normal when the study was completed. The neutrophils fell initially. They fluctuated considerably. Apparently they were returning to normal toward the end of the study.

The platelet counts in the Rongelap group showed a very definite decrease. I do not have slides of this, but I think it is quite evident from the graph, and you can see the general trend. Certainly there is a marked fall from the normal values. They reached a low on approximately the 28th day, and returned to a value of roughly half way between that low and normal, and perhaps were returning to normal at the completion of the study.

I might sayin passing that in general the platelet count at least showed a more regular response than did the leukocyte count. The curve is very smooth. It shows a definite low and return to normal, while the white count was prone to fluctuate as a function of time.

The hematologic findings in the Ilingula group parallelled almost exactly those in the Rongelap group. However, the severity of the changes was not so severe. The time trends, however, were the same.

With regard to the Americans, looking at the white count, the lymphocyte count or the neutrophil count, one would be very hard put to say that they had beenexposed to radiation. The counts are lower than normal, but here again

we get into the control business, and it is difficult to evaluate the extent of fall. However, if we look at the platelet count in that group, here again we get what appears to be a very definite fall with a low reached at the same time as in the exposed native population, and a return towards normal towards the completion of the study.

A word with regard to children versus adults. In the Rongelap people, I think with all end points -- all hematological end points, -- the children seemed to show a more marked response than did the adults. That is on the basis of absolute count. That is, we take the counts per cubic millimeter, and the children's counts were lower than adults. However, if we take these on the basis of per cent of control, this makes a considerable difference, particularly in terms of the lymphocyte count. If we take it in terms of per cent of control, the children were markedly more affected than the adults, which in terms of absolute counts they were more affected, but not nearly to the same degree.

With respect to the time trends in hematology in general, they were markedly different than is observed in the laboratory with large animals. The fall to the lowest point for both the myeloid elements and the platelets was later than seemed with animals, and its return towards normal was later than has seemed with animals.

On looking over carefully previous data from the Japanese and accidents that have occurred at Los Alamos and Argonne, it appears that perhaps this has been observed before, although not emphasized. Actually the data previous to this has in general been composed of very small exposure groups even in the Japanes, while the total exposure group followed may have been in the hundreds. Actually the counts at a given time were done on a very few individuals, as low as two out of the group. So it is difficult to make an accurate comparison between the two exposure groups.

In the case of the Argonne accidents, we have to compare our data with one or two individuals from a comparable dose range.

We attempted to make some correlations between skin lesions, depilation, and various parameters derived from the blood counts. All efforts in this respect were not fruitful. We were unable to evolve any correlation at all between hematological changes or skin or depilation changes.

One final note on the hematology. The time trends, as has been pointed out, are essentially identical to that of an individual exposed at Argonne National Laboratory, who received an estimated 190 REP of radiation. The prognosis for this individual to date has been excellent, and we hope that this may be an indication that with these individuals also the prognosis will be excellent.

I think those are the main points, Dr. Bugher.

DR. BUGHER: Thank you, Dr. Bond. Are there further comments?

MAJ. HANSEN: Did any Americans show they had any beta burns?

DR. BOND: This depends on who looked at them. I think the consensus would be to the effect that the lesions seen are not inconsistent with beta lesions.

DR. DUNNING: Did I understand you to say that these were superficial on the natives?

DR. BOND: This is the impression clinically, that they were superficial. It was like a sunburn with superficial layers peeled off. There is no evidence of deep involvement, and they healed rapidly.

DR. DUNNING: Even those on the feet?

DR. BOND: No, I should not say that. This was most of the lesions. There were occasional lesions that were deep. This occurred on the feet and occurred on the back of the ear of one individual, but as a whole, the lesions were superficial with these few exceptions. This is not clear as to whether it is a function of total dose or energy that is responsible for this. A very thick skin at the heel would probably give you quite a different result than a very thin skin at areas where the skin is more superficial. We have no way of knowing how much material was deposited on

the feet as opposed to a greater surface.

MR. HARRIS: I don't know whether you looked at the pathology, but did anybody make a note of possibly at what level there was fibroplastic proliferation beneath the surface on these individuals? It appears to me that a good index of the energy of the situation, and we assume here this is a 100 kilowatt average energy beta ray which is doing most of the burning, from what I have seen of burns using strontium and various high energy beta rays, the level of fibroplastic proliferation is sharply cut off with strontium. That cutoff point agrees in general with the range of the electron from strontium. So that this might be something to look at if it was in these specimens.

CDR. CRONKITE: Essentially there is no detecting a histologic change between three German octavos, depending on what part of the body you are at. Greater than 50 MU is very little detectable.

MR. HARRIS: I am not thinking of the depth down to which, but thecloseness to which you come. Essentially with strontium what you find is the burn area, and below the burn area is a very thin area of disturbed reparative action that you will get fibroplastic proliferation up to the depths from this point, but below that point which is slightly below the burn itself, you will not have it. The total depth below the surface of the skin appeared to be

roughly dependent upon the range of the beta ray. This could be checked experimentally if somebody wanted to know something else.

CDR. CONARD: Most of the damage histologically occurred in the papillary area.

MR. HARRIS: But there was no reparative action coming from the bottom.

CDR. CRONKITE: There was no fibroplastic proliferation in any that I looked at.

DR. DUNNING: I think the work at Western Reserve pretty well shows that. In case of sulfur 35, if my memory serves me right, he had to deliver something like 100,000 REP surface dose in order to get a lesion, and then they were superficial and healed after a few weeks with no persistent dermatitis.

DR. BUGHER: I saw in some of Gene's intermediate biopsies, not the latest one, the amount of histological change in the basal layers was of remarkable content. The amount of response below the base of the membrane was almost negligible. So I presume we are talking about a very large beta dose to the basal layers of the epithelium and the soft gamma dose.

MR. HARRIS: What was the depth?

CDR. CONARD: Somewhere between 1,000 and 2,000 microns.

CDR. CRONKITE: It was located on the scalp, where they lost their hair.

MR. HARRIS: I was interested in this 700 r. That is how much of what energy at what?

CDR. CONARD: That is medium hard x-ray.

DR. BOND: That is another thing; what is the depilation dose?

MR. HARRIS: How about the children that they depilate all the time for fungus infection. How much is that? You ordinarily use a soft x-ray.

DR. BOND: Yes, with divided doses.

CDR. CONARD: Two to three hundred r usually.

MR. HARRIS: That is measured in hair. You could have a relatively small soft x-ray or gamma dose in the hair follicles, whereas you could have a terrific high skin dose so far as REPs of beta rays are concerned.

DR. BOND: We had a conference with radiologists and this question was asked: What is the depilation dose, and the estimate computed ranged from 300 to 1200 r.

CDR. CRONKITE: There is one point which I think Dr. Bond deliberately left out, and all of us wanted to, but has to be considered to a certain extent, and that is, how serious are the hematologic changes as observed here? I think it is my personal opinion that these people were on the borderline of getting into serious trouble, particularly as far as platelets are concerned. I don't think we can

make a really good estimate of the severity and the potential danger, but I am confident in my own mind with the counts to have fallen lower, there would have been a serious question.

DR. BUGHER: Yes. It is undoubtedly fortunate they were not cocoannut hunting in the northern islands on that date, too. I notice here one of the earlier things mentioned was itching and burning of the skin. The Japanese fishermen complained of that, and also very maked burning of the eyes, actually beginning while the fallout was still visibly coming down. How did you interpret that sort of symptology here?

DR. BOND: You mean in terms of whether it was chemical?

DR. BUGHER: Yes.

DR. BOND: We discussed that. Do you want to comment on it?

CDR. CONARD: I think certainly the fact that the chemical irritating material was on the skin might have played some part in the initial symptology. But as far as production of lesions are concerned, I think it is pretty definite they are radiation lesions, and not chemical in nature. There were quite a few that did report the burning of the eyes, but not nearly so large a number that reported general itching of the skin.

DR. BUGHER: These people did bathe rather promptly

CDR. CONARD: Some of them did, but the majority of them did not bathe until they were/contaminated on the destroyers on the way back to Rongelap.

DR. BUGHER: The Japanese mostly bathe in teacups or rice bowls, and their immediate symptoms tended to be rather severe. They continued. So that those who went to sleep had some trouble getting their eyelids open. They were pretty well stuck together.

CDR. CONARD: We had no reports of that kind.

DR. BUGHER: It would bear on how much of the calcium oxide had been passed through a hydrated phase to carbonate. The carbonate in itself should not be irritating in the slightest.

DR. DUNNING: Wouldn't the time between the exposure and the onset of the burns be so great to speak against them being chemical burns?

CDR. CONARD: Yes.

DR. BUGHER: The lesions appearing two two weeks later are purely beta rays. I am thinking of the immediate events. The Japanese fishermen were considerably closer to the detonation site than the Rongelap people. At least, if they were where they said they were, they were decidedly closer.

DR. BOND: Dr. Bugher, when Dr. Zsuzuki was in

California, he reported that two of those patients had bleeding, and two had microscopic hematuria. Do you have any data on that?

DR. BUGHER: We know nothing more than he gave us. He got that information mostly by radiotelephone after he reached Washington. That was the last he had.

DR. DUNHAM: I had a point on the chemical burns to the eyes and the Japanese fishermen did not develop late burning lesions whereas they did develop very early burns. So I think that points pretty definitely to the eyes in the Japanese probably being on a chemical basis, whereas the native group had a mild transient itching or burning.

DR. BUGHER: One about the sac being continuously bathed tends to clean itself of material that falls in. I think there are some other differences, too, between the Japanese experience, as we know it at least, which is subject to considerable uncertainty, and the subsequent experience of these people, probably relating to the different environment.

If there are no further questions or comments on the clinical side, we will turn to the further information on the problem of the internal deposition of materials, urinary excretion, and these matters that Dr. Claus and his contingent have considered.

NATURE OF FALLOUT, INTERNAL DEPOSITION, URINARY EXCRETION, BODY BURDEN, AND LONG TERM HAZARD.

DR. CLAUS: I approach this report with some trepidation, because as you know, I was shanghaied into this group this morning. It is not often, though, that the shanghaied member of the crew gets to be skipper right away. I was impressed by the apparently large amount of data available on this subject, and as the discussion flew back and forth, I attempted to make a few notes of things which were perhaps appropriate for this report.

The conferees came so well prepared so that the data can apparently go directly into a report that I don't believe it is quite either practical or worthwhile to attempt to summarize the whole business at this time.

What I have tried to say here, if it happens to be in error, I hope my conferees will correct me immediately.

DR. BUGHER: They will.

DR. CLAUS: As to the nature of the fallout, it is pretty well agreed that the fallout on the islands consisted of large particles with radioactivity plated out on calcium carbonate. In the islands, there, at least there appeared to be no real evidence of lime burns. It was interesting that some people observed in the dark room a transient type of phosphorescence, but rather than ascribe that to any extraordinarily high degree of radioactivity, it

is believed that this probably arose from microorganisms which happened to be in the seawater at the time.

There was an extremely high content of Neptunium in the activity. Two thirds was mentioned, and a few minutes ago as much as 80 per cent of the activity at the beginning being in the form of Neptunium. Consequently, most of the burns were probably due to soft betas, most likely from the Neptunium.

NRDL observed that most of the activity was concentrated on the smaller particles with approximately an equal distribution of hard and medium gamma components, a small amount of soft components.

At New York, the laboratory observed as much as 18 per cent of ruthinium in this activity. That would be 18 per cent of the fission products, I would judge, because between Neptunium and 18 per cent of ruthinium, there would not be much of anything left.

NRDL has observed activity in the fish caught in the Rongelap lagoon, and at 116 days there was 35 per cent of total beta activity in the fish as ruthinium.

I might put a few of these figures on the board. There were 2.7 microcuries of beta activity to fish of two to three pounts, and 6.4 microcuries of gamma in the same fish. This was approximately equally distributed among the viscera, the skeleton, the flesh.

Strontium 89 was found to be quite high in cocoanuts or in the juice or sap or whatever they call it. My first thought was the strontium 89 was absorbed through the fronds, but as it was pointed out, that the material appeared in the sap, it seems there was an extremely high uptake of strontium through the roots.

NRDL has also made observations in chickens, pigs, fish and so on, and this material is available for the published report. As of this moment, I don't have any notes here on that.

For the content of humans, urine samples were taken in March of the Rongelap natives, Americans in March and April, Japanese in April, and these samples were pooled and alpha, beta and gamma breakdowns were observed at Los Alamos on the pooled samples.

The alpha activities, it is rather interesting to note, that there was no uranium or polonium observable, and of the plutonium, the body content is interpreted to be 1.6 times 10 to the minus 2 micrograms, or .7 d per m per 24 hour sample which in the business is taken to be insignificant, as far as body content is concerned.

Gamma studies were made with the counter. This is putting the urine samples in the counter. This Orme counter is a counter prepared as a liquid scintillation counter in which the total arm can be placed. In other words, it is a

small version of the counter in use at Los Alamos now for studying the content of a whole man. With the 100 channel analyzer, peaks were found at about 50 KV, 100 KV, 1945, 210. Most of these, I understand, are not identified.

MR. HARRIS: This is just gamma spectrum stuff, and this is not extremely good sensitivity in high energy gamma rays. There were essential peaks that we could not identify. This was 145 kilovolt peak and a 210 kilovolt peak. The rest fell in line pretty well.

DR. CLAUS: The one at 360 kilovolts, a strong one which is iodine 131, one of about 500 as ruthinium, and there are others presumably related to the barium-lithium units. In addition to the fact bey were able to observe iodbe as a gamma activity. It was a volatile component which could be distilled off and observed directly.

I think I might now make a table. These were the natives, and these were the Americans.

Iodine 131 worked out to .56 microcuries and 17.5 microcuries for the Americans. In this equivalent -- would you explain that?

MR. HARRIS: The I-131 equivalent at the time of fallout is meant the total amount of all the iodine isotopes plus the small contributions from mthe tellurium mothers weighted for the beta activities, and put in terms of equivalent I-131. Those are millicuries, Walter.

DR. CLAUS: That is right.

I should not have drawn my line back so soon, because these calculate back to a dose of 150 REP to the thyroid and 50 REP for the Americans. These can be calculated back in terms of fissions. I will not put these figures on the board at the moment. In terms of the fissions to which the natives must have been exposed, in order to come up with these particular burdens, Strontium 89, 2.2 microcuries for the natives, and 0.4 for the Americans. Barium 140, 0.34 microcuries for the natives, and 0.27 for the Americans.

Calcium 45, 0.19 for the natives, and 0.04 for the Americans.

Ruthinium 103, the short lived one, 0.028 for the natives, and 0.015 for the Americans.

I mentioned the fact that these could be interpreted in terms of fissions to which the people were exposed. If you average them out, it amounts to three times 10 to the 13th for the natives and 9 times 10 to the 12th for the Americans. It is Payne's idea that the best way to interpret these doses is to go back to fissions as a common denominator from which you can make a lot of other calculations. I think one might take a look at this, though.

When you add all these up, you find you have even less than 3 microcuries, and interpreted as the very worst, strontium 89, still you have very slightly over a permissible

body burden as presently interpreted in terms of very conservative terms. So that from this point of view, at least, it certainly does not look like this is anything very serious in any of the natives.

These are all short lived materials, of course, and I don't know how one might immediately interpret in terms of strontium 90 which they might have picked up. The dose to the thyroid is sizeable, but still relatively small compared to what we usually think of as the dangerous dose to the thyroid.

So that from the information that we have presently available, I would not be inclined to believe that the present body burden is one we need bother much about. The external doses to which they have already been exposed are much more serious in terms of our usual concepts of radiation hazard than the body burdens which they now carry.

DR. BUGHER: Those are very nice figures.

DR. DUNNING: I think there is another point that bears repeating here, that not only is this saying the equivalent of 3 microcuries of strontium 89, but that, too, is based on the assumptions that you have equilibrium conditions. In this you have a one shot affair and wit h an expected half life of 53 days, this makes an even more conservative picture.

DR. BUGHER: On a one shot basis, this is probably

not more than one per cent.

DR. CLAUS: Do you want to talk about your concept?

MR. COHN: Yes. Our findings are a little different perhaps due to the different approach we took. While these figures on the board represent amounts of body burdens calculated on the basis of I-131, assuming various assumptions, our approach was a little different, in that we derived our estimated body burden extrapolating from animal data. What we did briefly was to sacrifice two pigs from the island after getting a very accurate control of their urinary excretion for 24 hours at 81 days. Then we did a complete radiochemical analysis on all the separate tissues of this pig, and also on the urine.

At the same time we did a complete analysis of a human sample taken at just about this time, and extrapolating from the pig data to the human, we come out with values of total body burden of beta of .33 microcuries at 81 days. If we extrapolate this back to 30 days after irradiation, this is a considerably difficult thing to do. Most extrapolations are based on animal studies, particularly Hamilton's work in which a constant falloff of activity is assumed. We know that this is not the case.

The only evidence that I know of in human strontium inhalation probably is one case at Brookhaven. We based our data on the rate of excretion of this inhaled strontium

90. While using their figures for biological half life of this period of 81 days extrapolating back to 30, we come out with a body burden of 1.4 microcuries at 30 days.

Barium comes out .7, Lanthanum is .7. The remainder of the activity, the rare earth group, comes out .4. Strontium, barium, and the rare earth group together constitute about 75 per cent of the total beta activity.

We found no evidence of calcium 45.

If you want to extrapolate this back to one day -- and this is a very difficult thing to do -- we decided the best way to do this would be to use the human radium data. Strontium is known to fall off at the same slope at a lower level. We have not calculated it for one day, but it will give a value quite a bit higher.

MR. HARRIS: No, I don't think so. I think if the 30 day level of strontium, taking in per cent of the total amount in the body of the dose given, it may come up about the same as the number which we took back to one day.

MR. COHN: We will have to check this further.

MR. HARRIS: This can be checked. As I recollect, this puts us in much better agreement than we were before.

MR. COHN: The half life of strontium in the first three days is quite tremendous. It has a biological half life of about 4 days. You have to be careful in extrapolating back. Since we have no sample earlier than 16 days, we have

to be quite cautious.

One other point. You mentioned that since this is pretty close to tolerance that we don't think it is too important. I think we have to be very careful. While we may be close to tolerance or a little over, we have a complex situation in which we have not only the internal dose of all these separate emitters added up to close to tolerance, but which have what we think was close to a tolerance external dose. I think the effects are more than just additive. Certainly it does not affect the acute situation. We did individual studies on individuals separately for many days. We tried to correlate our excretion in the urine with various levels of blood picture curves, the platelets, and white cells, and we could not find any correlation. In general, and I think we all agree on this, the internal body burden is roughly proportional to the external dose that was calculated for each group.

The Rongelap are the highest. The Iligina have received half the external, and they are pretty close. Their mean excretion is pretty close to a half of the Rongelap. The American group -- I am not sure what the external dose is now -- the internal dose is pretty close to a fourth of the Rongelap. I think we agree on that now.

MR. HARRIS: This shows a little less than a third and assuming all errors I would assign a sigma of about two to

this one over here, and a sigma of 6 to that one over there.

MR. COHN: I even go further. The individual variations within each group are great. Some show practically none, and some show 3,000 count per 24 hours. Most of this activity is due to adjusting. This is especially borne out in the animal data in which we find the highest amount of activity. GI system and liver and very little activity in the lungs. As pinpointed out, it is not too likely that due to the large size of the particle, 6 to 200 microns, that a great deal of inhalation would have occurred.

DR. BUGHER: It is very clear that these quite different approaches have given results that are not too different, and the results are remarkably close.

MR. HARRIS: One other thing that I did not put on the slip of paper is that so far in our findings in the Japanes we have had some trouble with the strontium method on those. But the activities found in beta activity at these late times indicate that the Japanese were very similar to the Rongelap natives in the amount of internal exposure, and a similar number might be postulated as the native number for exposure to numbers of fish.

DR. BUGHER: As I recall, you put a lot of emphasis on the plutonium excretion measurements, did you not, using that as one of the approaches to the body burden story from the excretion rate of plutonium in the urine?

MR. HARRIS: The values were so low that although we have better human information on excretion at various times, the amounts we found in the urine were so low that there is no significance attached to these numbers. We tried it with small volumes of urine and large volumes of urine, trying to go as high as three liters of a pool sample but this does not work because the residual that you get and the troubles you have with self absorption in counting these, using the larger volume, negates your result.

Especially in this highly concentrated urine -- this is very interesting as a sidelight -- in the standard procedure at Los Alamos in these urines that they use daily on all personnel, at the end of the system there is practical ignition of the residue takes place, and a great flame shoots out and pieces of glass break up and fall in.

On the natives this was really something to see because of the concentration they had. This plutonium number you cannot depend on. If we take what is known about the amount of plutonium made in this particular device, the university is still too great to use plutonium to come out with the number of fissions.

DR. BUGHER: I take it you did not ascribe those pyrotechnical displays to radioactivity. The Japanese did.

MR. COHN: There were a couple of other interesting items that I might bring up. One concerns the internal

decontamination study we did out in the field. We took a small group of the Rongelap natives with the high internal gamma contamination, and treated these with calcium EDTA, given orally one gram per 25 pounds of body weight. I think it is significant to note that we did get some effect, approximately a doubling of the daily 24 hour excretion. This, while not very significant in terms of the total body burden because that means increasing from .1 per cent to .2 per cent for 24 hours is fairly interesting. It might point the way to further experiments along this line of preferably giving the material other than orally, which is the poorest way to administer.

Another observation on the animals, which I think is interesting, a number of the hens started laying eggs about two weeks after we brought them back to NRDL. Most interesting, the shell of the egg ran as high as 60,000 d per m per shell, which was roughly four times the urinary excretion at this time. The albumin also contained a large amount of activity, and the yolk the least amount of activity.

Also we were interested in fertility studies of these animals that we brought back. So far we have mated the roosters and the hens from Rongelap, and my latest report tells me that 50 per cent of the eggs are fertilized. We have gone into the business, and they are being incubated right now. It will be most interesting to study what the

transfer from the mother to the eggs is, and what the biological effects of such material would be.

DR. BUGHER: Yes, indeed. I hope you continue those operations.

DR. DUNHAM: What fission products were increased as far as excretion rate is concerned from the EDTA? Obviously it was not all.

MR. COHN: We don't know. We know from previous animal experiments it was not strontium.

CAPT. ENGLISH: How many days after exposure was this study?

MR. COHN: This was the 21st of April, 51 days after; a considerably long time to expect much result.

DR. BUGHER: Were you getting strontium in the egg shells?

MR. COHN: Yes, we have a chemical analysis of the whole egg, as well as the tissues of all the animals.

DR. CLAUS: The shells were formed how long after the exposure?

MR. COHN: These are animals in our laboratory. 45 days after.

DR. CLAUS: They expected everything to be pretty well out of the soft tissues by that time.

MR. COHN: Yes, approximately 80 to 90 per cent of the activity is in the skeleton system.

DR. CLAUS: Indicating a very high turnover of the calcium like substances in the skeleton in the production of egg shells.

MR. COHN: Yes. The hen has the unusual ability to concentrate the alkali earth that no other animal has in this particular physiological situation. We hope the animals would continue at this high level and perhaps decontaminate themselves. But they reach a peak and fall off at a very low level. The eggs are quite normal in every respect in weight, size, and so forth.

DR. BUGHER: This is a new slant on the means of decontaminating strontium. Are there other comments or questions here?

We have run a little bit overtime. We have come out on the schedule. Let us see if we can reassemble at two.

(Thereupon at 12:50 p.m., a recess was taken until 2:00 p.m., the same day.)

AFTERNOON SESSION 2:00 P.M.

DR. BUGHER: Now we go on to a general discussion of the material which was presented before lunch, and I think it might be perhaps wise to take some of the things up in more or less the recorded order in which they were presented, that is, considerations of dose, and so on, were touched on somewhat.

Gene, did you have any suggestions in mind here as to the general direction of our discussion along these lines?

CDR. CRONKITE: There is one general thing that I don't think was realistically approached this morning, and that is, would anyone venture to make an estimate of the prognosis as far as the individuals are concerned, both from the external exposures that they received, and the internal exposures separately, and then the probable effect of the combination. It was alluded to, but just what is the situation as far as these individuals are concerned? I certainly do not know.

DR. BUGHER: That is a short discussion. Our desire is to get somewhat more extended comment from other members of the group.

MR. COHN: One source of infirmaton on this point may perhaps come out of the animal studies. We brought back something like 66 animals, and the animals in general have about ten times the internal body burden of the radio

isotopes, and they have about 50 per cent, or 100 per cent more external dose due to the longer time they stayed on the animal. If anything does show up, it will be more likely to show up sooner in the animals, and would perhaps give us some idea of the prognosis for the humans over a longer period of time.

DR. BUGHER: What are the animals that you do have surviving ether than the chickens?

MR. COHN: 40 chickens, 6 pigs left, 3 from Rongelap and 3 from Uterik. The pigs had practically all internally and only about 6 r internall.

We also have a cat, three ducks, and I think that is the substance.

DR. BUGHER: Have any of them shown signs of illness?

MR. COHN: Six of the hens have died so far spontaneously, cause of death unknown. On autopsy, we can find nothing. There was a slight hemorrhage in the lung of one of the chickens. Nothing that we can ascribe to radiation per se.

The three pigs that we have left are growing quite tremendously on good feed that they are getting. There are no symptoms that we can observe as far as temperature, weight gain and general appearance.

DR. BUGHER: Did they show blood changes comparable to humans?

MR. COHN: That is hard to determine. Their white cell and platelets fell. Chicken hematology is bizarre, to put it mildly. They have practically no platelets, for example.

We have done pathology on the chickeds that died or were sacrificed. We sacrificed four or five pigs so far, and we were not able to say anything likely about any pathological changes. We do have radio-audiographs on the animals which might be particularly interesting here. This is a radio-audiograph of the tibia of one of the young pigs. The bone morphologically does not appear to be normal. There is parveculi extending through the shaft which is abnormal in a mammalian bone. There is a thickening here which probably indicates a failure of an osteoabsorption and periosteo-aberration. The particular thing about this bone is that there are two areas od dense concentration of the trabecula which corresponds on the radio-audiograph to two separate regions of high concentrations of radioactive material. It was the opinion of Dr. Norris, who did this particular audiograph, that these indicate two separate and distinct exposures to fallout material.

There is a lot of discussion on this, and it is pretty hard to come to a definite conclusion because we, one, don't have controls on these animals, and second, there were quite severe dietary changes in the animals from the

time we collected them and brought them back. There may also be a finding of some disease which we don't know.

DR. BUGHER: Doesn't your bone section show two zones of more compact trabeculae?

MR. COHN: Yes. I don't know whether you can see it, but the arrows indicate that.

DR. BUGHER: Aren't those the two regions of strontium concentration, too?

MR. COHN: I don't know whether it is strontium. It is mostly like strontium and barium.

DR. BUGHER: And the alteration of the bone construction itself would not be a finding of exposure.

MR. COHN: Functionally they appear quite normal. There were two independent pathologists that came to this conclusion.

In another pig, which is presumably a simile of this one, we don't find this double layer again. Mostly you have concentration here, and then you can see a light area which corresponds to the growth after the animals were removed from the island, and then the deposit in the still growing animal.

In the mother of these animals, the sow, we get the deposition here, and what looks like an indication of perhaps a second deposition in here. You can see this better in another audiograph.

This is a baby chick that was just born maybe a week or so after the detonation, and here you have an abnormal morphological picture. In the audiograph you get a concentration of radioactive material in the diaphysis here and practically nothing on the ends where the bone has grown subsequent to the removal of the animal from the island.

Here you have the bone marrow which would presume to be abnormal and perhaps due to the deposition of active material here, due to radiation -- and this is anotherchicken bone here again, looking abnormally morphological.

You have the same thing in a chicken bone. We have trabecular tissue extending down through the bone quite a way, which you do not find normally. This would normally be reabsorbed here. These are two separate animals.

You note there is not the same concentration in each animal, presumably depending on the dietary pattern. We have similar audiographs for a number of the other animals also. The picture of iodine in the thyroid is typical of the iodine diffusion.

DR. BUGHER: Could you make an estimate of the iodine dose?

MR. COHN: Yes. About 15 microcuries was the estimate calculated back to exposure time. It is considerably more than the humans.

MR. HARRIS: 15 microcuries where?

MR. COHN: This is in the bone and not the body.

MR. HARRIS: 50 microcuries in the thyroid?

MR. COHN: Yes.

MR. HARRIS: This refers to in humans in reverse to total body of 56 microcuries, of I-131.

DR. BUGHER: Swallowed and absorbed. Is that assuming 100 per cent absorption?

MR. HARRIS: It assumes 100 per cent of the ingested material.

LT. LOONEY: There are a few interesting comments. We have been making studies in Bethesda , who have had thorium, and it is very interesting to see that this thickening of the shaft is something that we have noticed with other minor changes in these people. Over 50 per cent of some 17 that we have studied had that. In some radium patients -- would you like to go into that at this time?

DR. BUGHER: Yes.

LT. LOONEY: I have recently gone over all the clinical data on the luminous dial workers, some 80 patients, 30 of which were studied at Boston, and 50 at the Argonne National Laboratory in Chicago. In relation to this, some 10 per cent of these people developed bone tumors 20 or 30 years after the administration of radium, and this luminous dial material. Some patients which had less than one microgram of radium had severe bone changes or tumor formation,

or others with 10 or 15 micrograms had relatively little changes. This is a factor of 10 or 15, which must be taken into consideration as far as permissible levels are concerned.

The other thing is that the excretion studies done by Norris in Chicago, there was a finding of 2/1000ths to 16/1000ths of one per cent of the radium excreted per day. This is a factor of four to eight when you estimate permissible body burdens of radium compared to the more accurate estimates by measurements.

So when we talk in terms of permissible levels, I think it is very important as far as the humans are concerned to keep these factors in consideration, that there is a marked biological variation, and also a marked biological elimination of these elements. Since these elements are in the near permissible range, I think these people are extremely important from a long term study, because we know away above this we are going to get tumors and away below we are not going to worry. This is the range which we are interested in.

I think these people are extremely important from a long term study in that respect. I am not familiar with all the radio elements there that are of biological and physical half lives. I think Tomorrow I will take the information and refer it to the studies of the people with the late effects, if that is agreeable.

DR. BUGHER: If one divides the prognostic problem into the immediate and ultimate prognosis, I think the immediate problem has already given its own answer. The fact that except for two people who are still in the hospital -- old people, -- and one woman ready to deliver, all of the Rongelap people went happily off to their new location. There are no ill effects so far as the immediate situation is concerned. So that the immediate prognosis time has already given the answer to that.

The ultimate long term prognosis problem I think would concern itself with two broad aspects. One of the internal emitter question, particularly in the skeleton, which bears on what you werespeaking of, and the other is the possibly very much delayed skin neoplasia which would not be expected, I should think, to appear in less than 15 year or maybe more, if it appears at all.

Have you any opinions on those? What do you think in your own mind is likely to occur to these people in the next 25 years or so?

LT. LOONEY: The thing that we have noted as far as the relationship of radio element deposition to the formation of neoplasia from radium patients was this, that in most all of these characteristic of the histological findings was the formation of an atypicalosseous tissue, which was a bone formation, and this was not usual in the areas

of greatest radio element deposition. In fact, we could make very real correlation between radio element deposition and histopathological.

A few patients that we were able to do extensive pathological studies on, we could find that there are many areas -- not many, but in some areas -- in which the transition from this formation from atypicalosseous tissue and the fiber sarcomas was awfully hard to differentiate. Dr. Lipscott at Chicago has reviewed this and some of his work with plutonium in rats. The feeling is that the most likely place for the formation of these tumors around these areas of atypicalosseous formation, most of the tumors have developed, on the ends of the long bones in thecancerous bones. All of this is supportive evidence that these conclusions are probably correct.

I think this ultimately gets down to what causes cancer. Is it one cell from the irradiation in these small areas in which you have the proliferation of this tiesue become malignant? There is a smoldering of this for years, and then suddenly there is a turnover, and then it seems to spread throughout the skeleton or even multiple tumor formation throughout the skeleton.

As far as permissible levels are concerned, we have to interpret that in terms of this pathological finding in these people. It is to set a level with this marked

biological variation, and I think you have to watch these individual people, and to see what percentage of these people develop tumors as compared to a controlled group.

DR. BUGHER: Yes, that will be done. Do you have any feeling yourself for the probabilities involved here in this group? Would you expect any bone sarcoma to be encountered inthis Rongelap group? We have 84 people, is it not?

DR. BOND: 82.

LT. LOONEY: I would certainly like to have a chance to go over this data much more than I have at the present time before I would stick my neck out so to speak on this. But I do think that certainly with this close permissible level, that these people are extremely important to see if there is an increase in incident of neoplasia in this people. I don't know too much about the distribution of these various elements. Most of it is bone. Chances are your bone tumors are the most likely thing to occur. I would not want to hazard a guess.

MR. COHN: There is one point that has not been brought out, and that I think should be stressed, is that strontium 89 has a considerably shorter half life than radium, but there are a number of short life fission products that would have had to be present in high concentrations for an early interval which we don't pick up after 30 days. The

problem then is what is the effect of a large dose over a short period of time to the bone or bone marrow in this early interval, that is, in terms of possible carcinogenic effects later. This is a problem that has not been explored to any extent at all. Animals are certainly not humans, but certainly that we should consider.

DR. CLAUS: You would not expect these other substances to be present in the bone?

MR. COHN: Yes, there are a number of other bone seekers, I have a list of 15, that are present in high enough fission yield, but due to the fact that the half life is so short, after 30 days we do not pick them up. But they were radiating this bone during the interval.

DR. CLAUS: Would you hazard any guess how much there is of that, compared to this?

MR. COHN: It could be calculated.

DR. CLAUS: If there is any likely tumor formation from this stuff, and an equivalent amount of small quantity in the bone for the short periods of time that these are there, we are in a bad way as far as our permissible levels are concerned, because this is just a little bit over what we claim to be a permissible level of strontium 89 for a lifetime.

MR. COHN: Yes.

DR. CLAUS: This is for a relatively short time, and

we are going all out by saying you can give very sizeable overdoses for a relatively short time without doing any more damage than if you spread it out. So if any of our concepts to date on which we are basing our permissible dose levels are correct, then there is absolutely no chance, or at least an awfully small chance of anything developing from these levels.

MR. COHN: I don't think there has been any study made of high intensities and short exposures.

DR. CLAUS: These are not high intensities.

MR. COHN: Relatively high intensities. There are 12 other products that are bone seekers, in high fission yield, that would have to be present based on the present strontium and barium, for example.

DR. BUGHER: What are some that you would have in mind?

MR. COHN: Telurium, lithium 103, 106, cerium 101 and 144, zirconium 195, which we find in fish, praseodymium 143, yttrium 141, barium 140, and lanthanum 141, and iodinium 147.

DR. BUGHER: The 106 ruthinium has a year half life.

MR. COHN: No, that is only 19 days, I believe. Oh, one year. I am sorry. All of these are fairly long lived, anywhere from 10 to 60 days, and they are all present in a fission abundance of from 3 to 6 per cent.

DR. BUGHER: Are most of those excreted rather rapidly?

MR. COHN: Zirconium, cerium, praseodymium are concentrated in the bone to some extent. I think in this case their half life is probably much shorter than their excretion rates. That would be the determining factor.

DR. BUGHER: It is obviously unlikely that we will ever know more quantitatively what is in these people than we know now; in other words, we cannot really acquire any more knowledge by deferring consideration of anything because we really have in our hands now all the evidence that there is.

CDR. CRONKITE: Does the exposure to 150 to 200 r in relativdy a short time change tolerance concepts? Does this influence the tolerance concept? Has anybody done any animal experimentation where you crack them with a couple of hundred r and see if your same tolerance levels will hold up?

DR. BUGHER: The main point there, I think, is that our tolerance levels are below that for which experimental results can be demonstrated. In other words, one has to go much higher levels of the material. There have been experiments on such things. At the moment I can't recall the results except that they tend to be additive, rather than otherwise.

Walter, do you happen to know of any?

DR. CLAUS: No, I don't know what you are referring to there. All of the permissible levels with the exception of those that are tied in to radium, radium, plutonium, strontium, are based on a calculated dose to the critical organ of .3 of a REP per week. So I think that is probably small enough in comparison with a couple of hundred r dosage you might give so they could be neglected. If you are dealing with radium plutonium, you may have a little different problem on your hand, because those are based on actual experience, rather than calculated levels. But even there I think the doses do not exceed the .3 REP per week. They differ with that level very much.

DR. BUGHER: Dr. Dunham, I will explain what the question is here. In view of the rather small level of individual isotopes in bone and so on, whether the whole body exposure of 150 r which has been brought out this morning is not quite saying What that is to, and whether that would affect the concept of permissible limit, essentially, in regard to the expectations of biological accidents and unfavorable results in the bone from the concomitant gamma ray exposure.

I said as far as the experimental side is concerned, since the permissible limits are themselves levels at which one would expect such things anyway, that it is

rather hopeless experimenting in that level. One would have to go many, many fold up in the concentrations of isotopes, and there I thought the effects were essentially additive.

Do you happen to remember any of the experimental data?

DR. DUNHAM: I know of no experimental data that is comparable to this situation.

DR. BUGHER: No, nothing comparable to this. Exposure levels of 1,000 microcuries of strontium with animals also given x-ray.

DR. DUNHAM: I know of no such class data. The closest would be Fridell's work, where you were concentrating the effects in different organ systems. He has also given some whole body radiation. That matter might be re-looked at in this light.

DR. BUGHER: It is pertinent here.

DR. CLAUS: I think you could extrapolate from Fridell's work if he has not actually done it, and say if you give an animal enough internal emitter so that he is pretty close to the borderline, and then you add x-ray on top of that, you will get more than an additive effect, because the effect due to one isotope is partially compensated by other organs in the body. Whereas if you inactivate these other organs, then you will meet more than

an additive effect. Those are for very high levels. I don't think they are applicable to this situation at all.

DR. DUNHAM: Those are at levels sufficient to knock the bone marrow out.

DR. BOND: Those are very high levels. They are approaching total body lethal doses.

DR. DUNHAM: I was just searching my mind for some data.

DR. BUGHER: I think the answer is negative with regard to the question of whether or not this amount of material in the bone has any effect whatever on the general radiological manifestations of the dose that these people got.

LT. SHULMAN: How about the other way around. The total body radiation, and then the local deposits. Perhaps the 200 r received at that spot plus the local deposits may give different local changes than those calculations based on local radiation.

CDR. CRONKITE: In the children where the bone is growing it gets concentrated in a relatively small area, so as they continue to grow, if this were an adult, it would have been distributed relatively throughout the bone.

MR. COHN: It still concentrates in a factor of ten in the adult.

CDR. CRONKITE: But it stays concentrated in one

area in the child. Does this give a significantly higher dose to those areas so that you may have a different set of standards or think of it as different for a child than an adult?

DR. BUGHER: If you are asking personally, I don't think so. I think in considering permissible dose, we have oftentimes thought of the bone marrow being much more static than it is, rather than regarding it as essentially fluid tissue of a slow flow rate. The probability is that the regularities of concentration are not as important as we have assumed in computation. Usually a factor of five gets in the picture for irregularities, and nonuniform distribution of the material with respect to bone marrow. But certainly bone marrow cell structure is a highly mobile one in terms of comparative bone cells, for example; so it is quite possible that we over-emphasized the fact of non-uniformity, and such experiments as we have had in regard to skin activities would seem to indicate that the non-uniform situation is actually less of a problem than the uniform distribution of the same amount of material.

LT. SHULMAN: Is there sufficient data to know whether the local bone dose in children could be suspected of giving abnormal growth? Do the levels they probably have come close to the levels that do give abnormal development, such as in the chickens? That is abnormal

development, and they are only ten times lower. Over the longer period of time that the development takes place, children may be the individuals to observe in order to find that out.

DR. BUGHER: There may be a corollary to the question, and that would be whether or not the blood changes in the children were not related to the perhaps greater quantitative bone marrow dose. Does anybody have a comment on the question of growth disturbance in children, resulting from these exposures?

MR. COHN: We broke down each one of the groups into various age groups. Below five years old, from six to 15 and 15 and over. We find in the children we have a lower mean emitter excreted. What this means is hard to say. It is not likely that they took up less contamination than the adults did. They ate as much, and so forth. It would therefore leave you with the idea that the material they do take up is fixed more firmly in the bone so that a lower ratio perhaps is found in the urine. In general, more actively growing and proliferating tissue is more radio sensitive. You would expect to find more change in the children than in the adults.

DR. DUNHAM: A youngster two or three years old does not eat as much as an adult. A teen-ager will eat more.

MR. COHN: We analyzed all the food and most of

the contamination was in the water and in the sap of the cocoanut tree. There were very high levels of contamination. The fish was the other high source. Whether it is because they drink less water, it is hard to think that they took in less contaminating material than the adults.

DR. DUNHAM: I think it is hard to believe that they took more.

MR. COHN: Judging by my own children, they eat more than I do, although probably less at a time.

DR. BUGHER: I think one can say something about the effect of the general whole body exposure, and the expected growth rates. In Hiroshima the exposed children to the bomb within 1500 meters did show some apparent retardation of growth for a few years, but then in the last two years they picked up and apparently equal to the controls. Those were levels which were a mixed grill. Some had levels sufficient to give radiation sickness and some aplasia, and others in the same areas had no recognizeable radiation symptoms at all. So it is a statistical comparison. The difference has been lost now between those two groups.

MR. COHN: That is from external radiation.

CDR. CONARD: No internal there.

MR. COHN: That is right.

LT. LOONEY: Sir, in regard to the changes as far as children are concerned, I remember one of our radium

patients who was working as a luminous dial worker at 15 had areas of increased intensity around epiphyseal areas of the long bones. This, with other data, would suggest that we were not too sure whether these developed years later or developed with deposition.

Gross audio-radiographs were made, and they did have a concentration of radium in the epiphyseal area. The big trouble with the radium data is that we don't have a roentgen grosser so that we can tell these developments of the roentgenographic lesions, we see in the people 20 or 30 years from now. It would seem that these lesions develop years later.

As I say, in this one case which ingested radioactive materials at 15 died at 40, and she had these areas of concentration in the epiphyseal areas. From other studies, it would seem that radium is eliminated from the more accessible parts of the skeleton.

DR. BUGHER: Does anybody else have a different thought in regard to the skeletal prognosis here with this amount of material?

I think the skin prognosis is one which has a considerable uncertainty as well, and also equally perhaps important. I have to skip over to a Commission meeting, so I would like to ask Dr. Dunham to serve as Chairman for a while until I get back. You can go on with this prognostic side from the skin. I think also there should be some

further consideration of not the skeleton, as such, but the hematopoietic system with regard to the long term prognosis, and what we think the problems of leukemia may be in these people, and particularly with skin carcinoma. Leukemia from our Japanese experience would be something of importance much sooner than skin neoplasia, if the latter occurs at all. If you will excuse me, I will go.

DR. DUNHAM: Who wishes to make a contribution on this point? Vic, do you have any further comments you wish to make on the prognostications as far as the skin goes, and the late development of malignant change?

DR. BOND: I would like to ask a question. It was the opinion of David Wood, and others, that looked at the slide, that in addition to late carcinogenic changes, we might expect later breakdown of the skin in a period of months, rather than years.

DR. DUNHAM: In other words, you feel that it is too early to prognosticate?

DR. BOND: To really prognosticate. I don't wish to imply that there is definite evidence for this.

DR. DUNHAM: No.

DR. BOND: It is a foregone conclusion that it will occur. However, they saw changes in the manner of the regeneration of the epithelium which led them to believe that there are possibilities that this may occur. This is

strictly from animal data, and not from human data. Do you have any further on that?

CDR. CONARD: The lesions as they were when we last saw them showed no signs of development of true chronic radio dermatitis. That is, the usual signs of atrophy and so forth that you normally observe in chronic radio dermatitis were not apparent. There were some other changes. Hyperkeratosis was developing, and some overgrowth of the epithelium forming papule structures which Dr. Wood seemed to think might account for the large peel like appearance of the skins which we noted as a later development, along with hyperpigmentation of the skin. Whether these changes have any significance to prognosis, I really don't know. I think that when Dr. Wood comes through with his final report on histopathology, he may give us some more evidence of his ideas in that direction.

DR. DUNHAM: It is really a little early, because we don't know how normal this skin will get. If it gets more normal than it is now, then I think there are grave doubts as to optimistic prognosis. If it gets more normal in the next six months or so, then I think one can get very optimistic again. I believe radiation damage that leads eventually to malignant degeneration, the tissues themselves do not pass through a thoroughly normal appearing stage microscopically ever.

MR. HARRIS: I would like to prognosticate that for at least six years there would be nothing because in none of the cases of Los Alamos beta ray burns, there has been nothing in six years.

DR. BOND: What do the skins look like now?

MR. HARRIS: They are in much different shape than what the natives are. They are still breaking down. A couple of fellows still have to go back to St. Louis once in a while for a little more plastic repair. Other than that, they are in fair shape. You don't have the usual skin. It has the appearance of a cigarette paper type of thing. All of the fat beneath the skin is gone, and will never come back apparently.

DR. BOND: When you say nothing will happen, what do you mean?

MR. HARRIS: You cannot say that on this basis these people will not have any carcinogenic indications.

DR. DUNHAM: You have to give them at least 20 years.

MR. HARRIS: I think probably you might be able to give them 20 years on the basis of the fact that the Los Alamos people who have had this exposure, who have had superimposed exposures on the same skin for the last six years at least as high as tolerance, and probably higher than tolerance in some cases.

CDR. CONARD: They had 4 to 17 thousand REPs or

something like that.

MR. HARRIS: They had maybe 20,000 REP of high energy beta.

DR. DUNHAM: It was full thickness damage.

CDR. CONARD: A fission product material?

MR. HARRIS: Yes, sir. It was filter papers that they picked up with their hands. There was a small amount of Neptunium. I would guess it was only a small proportion of the total. You can get this number very easily by comparing it on the Nevada explosions, and if you compare with any sort of a standard Nevada explosion, you will come out with the number of Neptunium which would be the same that they had.

CDR. CONARD: As near as I can understand, practically all skin malignancies develop on top of a well recognized breakdown of the skin, chronic dermatitis or the usual chronic changes that occur after radiation. A vast majority of these people have shown no signs of developing chronic dermatitis or atrophic changes. There are a few, one or two. One or two of the foot lesions show some evidence of scarring and atrophy, and the persistent ear lesions.

DR. DUNHAM: You would give them all at least 20 years?

DR. BOND: That is for malignancy.

CDR. CONARD: If they don't develop any chronic radio dermatitis within the next five or six years, I would say the prognosis is excellent that they won't have any.

DR. DUNHAM: At least some of their lesions are going to be exposed very heavily to an added insult in the way of ultraviolet.

CDR. CONARD: Trauma, too.

MR. HARRIS: Isn't the humidity a little high out there for having as high an ultraviolet exposure as where you do get ultraviolet carcinoma? In the west, for example, where there is low humidity. If the humidity is so high in the climate that the percentage of ultraviolet is high--

DR. DUNHAM: It is awfully common to sea folk in general. It certainly cuts down what it would be being at the equator or close to it. Still plenty gets through.

CDR. CONARD: There is a great deal more of it, too, due to the long days.

DR. DUNHAM: All year around. It is not just seasonal.

CDR. CRONKITE: The thing that bothers me is what Dr. Bugher had to say this morning. The establishment so far as is known of what could be expected from the standpoint of prognosis with time intervals becomes acutely important because there are a group of individuals who are going to have to go out from time to time. It is going to be

one big chore to go out at regular intervals. If there is no hazard to expect under 20 years, I think we ought to seriously start thinking about these followup trips. Is it even necessary to go back four times in this coming year?

Being one of the individuals involved, perhaps I have a personal bias in it. But just as we are talking here from the standpoint of prognosis, nobody yet has come up with anything that means anything to me, except that somebody ought to watch them.

DR. DUNHAM: I think what we have been talking about in these last two discussions are something that would only happen in 15 or 20 years. I have not been as close to the planning as you have for the immediate followups. But it is my understanding, or at least I would think that the philosophy behind the coming followups was to follow the blood count back to normal, to find out whether it is back to normal, say, next August or fall. In other words, complete the study of the acute phase, and then after that, if you can develop a reasonable rapport with the trust territory physician, there should not have to be an awful lot of expeditions.

CDR. CRONKITE: I was thinking not only in terms of the long term prognosis, but the prognosis for the immediate future, because someone is committed to making a trip in August, and again a few months thereafter, in order

to collect more information in order to be in a better position to plan. But to get any information now that anyone has collected for planning purposes would be of tremendous importance to the people that have to go out there.

DR. DUNHAM: The other thing is the matter of leukemia which John asked to have some discussion on.

DR. BOND: May I ask something before we get into that aspect of it? The general question as to the findings seen in general are commensurate with the dose estimates we found this morning. 150 r is a ticklish problem.

DR. DUNHAM: It may be 175, if it was 150 r measured in the air with a different kind of field, is that right?

DR. BOND: This may or may not be.

DR. DUNHAM: I believe that. Do you doubt that? Noting the figure of 150 r, but the fact that there would be a difference in comparable effect.

DR. BOND: I want to get into that in a minute. This is the problem. Say 150 r and from considerations of dose rates, say this was given over a period of at least hours and probably days, as opposed to the usual single exposure which you would predict would give less effect for the given total dose. Also, there is a possibility that some of these individuals were inside structures and did not get the same total dose that was calculated for the island. These things would tend to make you suspect that the effect

should be less than what you would anticipate for 150 r.

I would like to know if this perturbs anyone? I have talked to a numberof people that it has perturbed. In other words, the findings observed are not commensurate with the calculated dose. Are they or aren't they? Is it necessary to go to something like this depth dose business to explain this? Do we have to go to something like combined effects to explain it. Or is everybody happy with the findings of 150 or 175 r?

DR. DUNHAM: Gene, you studied this longer than anybody in the room. Is there any change in the blood picture as you saw in those animals as you would have guestimated on the 28th of February last, as coming from roughly 150 or 175 r total body exposure?

CDR. CRONKITE: One has to make the assumption on the basis of this data that was collected that either man behaves differently from what we thought he ought to behave on the basis of large animal experience in the laboratory and with fission spectrums from atomic bombs based on Greenhouse work, or there is some weird combination of radiation effects that we are not at all aware of to make this difference.

It is a very real difference in the time sequence in the platelets in these human beings that occur as has been observed in any laboratory animal.

DR. DUNHAM: Is there any comparable data on large animals?

CDR. CRONKITE: Yes, we have 150 r dogs in a large group. They hit the minimum around 12 to 13 days, and are beginning to recover at about the time these people are getting the minimum values. It is an entirely different picture. I don't know how to interpret these things. It may be that the life span of human cells is entirely different from laboratory animals that we are dealing with, or there is some other weird radiological factor that comes in of additivity that prolongs the effect of radiation. I am confused on it.

DR. DUNHAM: It seemed to me in our little discussion this morning we talked about somebody reviewing the Japanese data, and if it looked as though in those that survived there was the prolonged effect. In the two Argonne cases there was prolonged effect.

CDR. CRONKITE: The bad effect on Japanese data is that those who survived the first blood count were taken in the third and fourth week after exposure. We don't know what they were doing before that. The Japanese dying probably received super-lethal doses of radiation, the behavior is the same as that of a dog receiving super-lethal dose of radiation. Then there is this hiatus of three or four weeks where you have to guess what that looks like.

From Argonne there is this one case of a man that was exposed to platelets and leukocytes who hit the low values at about the same time as the men of this large group did. I rather expected that people would be concerned about these differences in the time. Is that what a single dose of radiation would do to a man or isn't it?

DR. DUNHAM: We don't have any evidence to the contrary, do we, in man?

DR. BOND: May I ask Col. Browning and perhaps Commander Etter on this point: Was this actually observed, and does this surprise them? Is it 150 r anticipated or are there difficulties?

COL. BROWNING: I have those records, and I will get them up tonight,where people have been given 150 r in one dose. But if my memory is not wrong on this, it did go down earlier. This was with one MEV stuff.

DR. BOND: How about the clinical picture?

COL. BROWNING: These people were not well, of course, when they received it, which was the reason that we were justified in using those amounts. But they showed very little in the way of radiation sickness, just about comparable to the natives. But the blood did go down earlier with the whites and platelets, as I recall it. I willbring that stuff in tomorrow.

DR. DUNHAM: Did they follow through for six and

eight week periods?

COL. BROWNING: Yes, some of them for six months and a year.

CDR. CRONKITE: This data, if it is available, we ought to get hold of it for comparative purposes. I have never seen it.

COL. BROWNING: I will bring it in for you.

CDR. ETTER: What is it?

COL. BROWNING: It is the therapy units, one at Baylor and one at Sloan Kettering. This was a little bit of everything, including leukemia.

DR. DUNHAM: Commander Etter, have you anything to add to that at this point?

CDR. ETTER: No. I think what is bothering Vic a bit is that in setting up figures for so-called operational implications in atomic warfare, we in the past have pretty much considered it to be 100 r probably at the very low limit of any signs or symptoms appearing with probably a thought that a group of troops receiving 100 r could go on with their normal duties. This makes you wonder if the 150 r is going to result in this type of thing, whether or not our figures for operational purposes might not be a bit on the high side, rather than the low side, and we would have to come down a bit on this thing. I think that is what Vic is getting at.

COL. BROWNING: Harry, if we can get any information here from the physicists about the energy, it does not make us feel too bad. We are quite happy with our 150 r if this is correct. But if this is the very low energy stuff then we have not gained a bit of assurance from it.

CDR. ETTER: If 150 will do this over a period of a matter of 36 hours, doesn't this mean 150 delivered in a matter of a minute or so should not have given a much more acute picture, which means that 150 may be much too high for practical purposes in our operational structure?

COL. BROWNING: Yes.

DR. DUNHAM: How much did these people get the first 12 hours, Gordon?

DR. DUNNING: I don't know whether I can quickly answer that.

DR. DUNHAM: I think this is important in this consideration. What they got in the first 12 hours is not going to affect much more difference than three or four minutes.

CDR. ETTER: I picked the 36 hour figure because of the evacuation.

DR. BOND: They got only 30 f.

CDR. CONARD: I thought the curve was so steep.

DR. BOND: They didn't start their exposure until the plus 6 hours, at which time you are not on the step

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portion of the curve any more, but pretty much on the flat portion. So the dose rate was not changing as rapidly as you might suspect.

DR. DUNHAM: You would guess they got about one fifth of their dose in the first 12 hours.

DR. BOND: That is right.

DR. SONDHAUS: This depends very strongly on whether you assume the fallout was along one or short one. If you assume that the fallout was quick, that the dose built up to a high rate very quickly, then the first 12 hour dose would certainly be appreciably more than if the fallout was slow and only reached its peak after 12 hours.

In either case, I don't think even in the maximum case you can allow for more than about 30 per cent of the total dose in the first 12 hours for the 51 hour exposure. However, in the case of the Rongerik exposure, where the evacuation was at 28-1/2 hours, I think the first 12 hours would probably give as much as half the total dose. These are guesses. They are more than a guess out of thin air, but they are still not a great deal more than that.

DR. DUNHAM: Harry, I stand completely corrected on that point.

CDR. ETTER: How long was the fallout actually observed by the natives?

CDR. CONARD: About 10 o'clock at night.

DR. DUNHAM: About several hours.

DR. DUNNING: But that doesn't mean you have equal quantities of radioactivity coming down.

CDR. CONARD: No.

DR. BOND: I think the important thing is that they didn't receive 90 per cent of their dose over a few hours. It was a relatively slow dosage.

COL. BROWNING: Harry's point is well taken. If they became nauseated at 50 or 60 r, perhaps, and they may well have done so from these figures, then we are going to do some thinking about this. But the clinical data we have doesn't seem to indicate that this happened.

DR. SONDHAUS: I think it might be added that Dr. Dunning's point about the sky shine is important here. We may not be taking that into consideration at all. All these calculations are entirely on the basis of fallout. If there was sky shine in addition this would have to be handled separately.

CDR. CONARD: Can you give us any idea as to what the additive dose would be from sky shine?

DR. DUNNING: I am sorry. By looking up the date, I can give you some estimate from the Nevada test, but how you would extrapolate this to out in the Pacific, I don't know.

DR. DUNHAM: What sort of orders of magnitude are

we talking about?

DR. DUNNING: We don't know.

MR. HARRIS: Were you talking about a cloud of radiation? Is that what you mean? Or do you mean reflected radiation from the atmosphere?

DR. DUNNING: The actual cloud.

MR. HARRIS: This has been simply covered by Parker. This is the opposite of the Clark Gable problem of big ears on both sides.

DR. DUNNING: You can figure out that r per hour is 10 times to the third, times the energy of your emission, if you want to do it mathematically. That still doesn't give you the answer what happened out there.

MR. HARRIS: You could take some numbers if they are any good, which said so many fissions to which they were exposed.

DR. DUNHAM: I think the thing that baffles we poor medicos is the lack of certainty on this whole matter of dose, and the time during which the dose was given, in order to try to give any intelligent interpretation. I think you really brought it up when you said that.

CDR. CRONKITE: My point is as far as writing the report is to completely leave out all concepts of dose. Say people were exposed to radiation and this is what happened. As soon as you put a dose down there, people are going to use it

DR. DUNHAM: With things as indefinite as they appear at this point, it will be more misleading than useful.

DR. BOND: It will never be more definite.

DR. DUNHAM: I am afraid not with this particular group of exposures.

DR. BOND: I didn't ask that question to get at the operational aspect.

DR. DUNHAM: No, but it is an important one to get out in the open and get it over.

DR. DUNNING: Dr. Dunham, I don't think we should be too awfully pessimistic. Take the Rongerik people. They had film badges out there, and they actually read between 40 and 50 r. If you make the same kind of assumptions and the same kind of calculations for Rongerik as we did for Rongelap I say this morning it was 60 to 75 from memory, and looking at it this noon, I find it is 75. Our calculations using the same kind of assumptions at Rongerik, says 75, and film badges said 40 to 50. We know that they were indoors a good share of the time. To me this gives us a pretty good notion that we are not at somebody else's ball park.

DR. DUNHAM: This is fine from what happens from fallout. The thing that is bothering some of the people who are trying to use this material are in terms of other types of situations where exposure might occur in a matter of minutes from an external single source as opposed to

this sort of situation. I think it is important that we find out right now that you are not going to be able to jump from much of this data to this other situation that Harry has to work about, and Gene, too. It does not seem comparable to acute exposure.

DR. BOND: I thought it was necessary to postulate the combination. In other words, can we explain everything we saw on the effects of giving dosages as we know them now, or are these inconsistent with present knowledge.

DR. DUNHAM: Who has in hand the burros situation? That is the only thing that is comparable. Do you recall what course the blood picture took?

CDR. CRONKITE: Higher doses. It was similar to single dose given to dogs.

DR. DUNHAM: I was wondering whether they showed the prdonged curve at the lower doses.

LT. LOONEY: There is one thing in going over all the available information on the use of radium and radon. I can look this up tonight and go over the hematological responses, but I remember there is a wide variation in some of the German literature following the internal use of radium and radon. This might throw some light or more confusion. I will give you areport on this tomorrow. I do remember one case that they said they got an increase in the red cont of something like one million, and I remember

trying to make some sense out of the hematological response that we did find many bizarre responses to the internal use. This might throw some light on the question.

DR. DUNHAM: Any further comments on that point or shall we move on to leukemia? The reason I mentioned leukemia before is because we were talking about operational problems, and if that is going to occur, it will occur in four or five years.

On the other hand, and I think there is general agreement, these people got no more than 200 r total body exposure, whether it was given over a period of 24 to 36 hours, it is most unlikely perhaps that there is going to be a problem. Who wishes to comment on that?

CDR. CRONKITE: These numbers up here are very nice, but is anybody willing to say what that would mean to the bone marrow over a period of 20 to 30 years in REP?

MR. HARRIS: Almost nothing.

CDR. CRONKITE: I would agree with you that one would not anticipate any leukemia at all in so far as the adults are concerned. What will happen to the children is highly questionable.

DR. DUNHAM: How often should there be blood counts done on these people after everybody is satisfied the initial phase is over? I gather we are all agreed that it was not over at the time Project 4.1 came home, is that right?

CDR. CRONKITE: That is correct.

DR. DUNHAM: They will be predicated on what turns up the next time or two out of course. Say two years after the initial exposure, from then on, how often would be useful?

CDR. CRONKITE: I don't know. Once a year perhaps.

DR. DUNHAM: Does anybody believe that leukemia will be an important sequalla among these people?

(No response.)

DR. DUNHAM: A bunch of optimists.

LT. SHULMAN: Is there any experimental animal that more closely approximates humans, like some of the primates, about which radiation exposure data is known?

DR. DUNHAM: No data on leukemia in the primates yet.

LT. SHULMAN: As far as dose and blood count change I don't know whether it has been studied in a chimpanzee.

CDR. CRONKITE: The chimpanzee has been studied, and the rhesus monkey, and the monkey behaves exactly like the dog.

LT. SHULMAN: That only proves dogs are closer to man than we think.

DR. DUNHAM: Are there any other sequallae that might be anticipated, or should be at least looked for? Cataracts have not been mentioned.

MR. HARRIS: We have done some work in the last few years on mice given single doses of radiation extending from 12-1/2 roentgens on up to 5 and 6 hundred, and those mice were kept and examined periodically for a period of two years. We have done this with x-rays as a control for neutron experimentation. We are in the process of pulling it all together at the present time. In my recollection of the last time I looked at this data to try to pull something out of it, the formation of not true cataracts, but detectable lens aberrations in the region of the posterior pull of the lens probably in most cases is non-vision disturbing if we knew what was vision disturbing in mice.

There is a definite correlation with the formation of these opacities, and the dose that the animals had. To the best of my recollection this correlation with opacity is better, and the incidence of opacity formation is higher than is the incidence of leukemia in animals. Therefore, it might be reasonable to continue some studies on these people for the detection of these lens aberrations and even before you would think of looking for leukemia.

As far as leukemia in the animals was concerned, it is going to be awfully difficult to analyze statistically because of a fair leukemia incidence in the normal population of this mouse. The same way with other types of carcinoma. I do remember that there is a shift in incidence of leukemia

and carcinoma as you go to higher doses. For instance, at the higher doses you will find much more leukemia than carcinoma, because of the time of onset, and the animals die before they are old enough to get carcinoma.

At the lower doses I would guess -- this may not be the right number -- I would find there is a positive index for leukemia production in doses above 100 roentgens or so many. I am not sure that this will be forthcoming when this is written up.

CDR. CONARD: What was the threshold dose for opacities?

MR. HARRIS: At half the year -- these are not non-vision disturbing capacities -- is in the neighborhood of 50 r for x-ray.

CDR. ETTER: Were those mice carefully screened beforehand for any lens aberrations?

MR. HARRIS: Yes, they were. The way they were run, this was an experiment in which we attempted to eliminate all bias. All items were coded singly. The observers over the period of two years never knew what animal they had hold of. We think it is pretty good statistically on that basis. There is a fair percentage of visible aberrations in control animals as you put the data back together again off the card files.

The threshold, if you wish to call it that, or the

50 per cent incident level of these opacities was at 50 r at the half year. This continues to go down in total dose at a year, a year and a half and two years.

DR. DUNHAM: It was my understanding that the macaques at Austin, Texas, have failed to show any opacities, many of whom must have gotten more than 100 r a year after I heard about it.

MR. HARRIS: I talked to the monkey man, and they are now getting opacities that were exposed to 14 NEV neutrons.

DR. BOND: Isn't the threshold for cataracts in mice usually lower than for other animals? Wouldn't more suitable data be the Japanese data?

MR. HARRIS: This is a definitive cataract you are talking about. What I am talking about is a smallest detectable opacity. When you take mice, then for a true vision disturbing situation in mice, which is really to squeeze the mouse behind the neck and his eyes pop out, and he looks sideways, and you can see it. This is certainly a vision disturbing situation, and the threshold there is similar to that found in Japan, or the incidence level is similar to what has been found. It was stated about 500 REM equivalent and it runs that way for mature cataracts in mice.

DR. DUNHAM: These are presumably going to be gamma ray cataracts, if due to anything.

MR. HARRIS: I will have x-ray cataracts and gamma

cataracts and thermoneutron cataracts. The opacity incidence in x-ray animals and the opacity incidence in 4 MEV gamma animals is about the same level, 50 per cent incidence. It is almost exactly the same level, although the lethality of MEV is much less for the 4 MEV animals. This would be out if it would do you all any good in figuring out what you are going to look at these people later.

DR. DUNHAM: What was the gamma threshold in rabbits?

MR. HARRIS: X-ray threshold in rabbits for what is called a mature cataract, not threshold but 50 per cent level, was at around 500 roentgens.

DR. DUNHAM: What was the threshold for opacities?

MR. HARRIS: They were not looking for those.

DR. DUNHAM: Certainly that leaves it such that we are going to have to look, probably the sooner the better that somebody can get out there and look, and establish a base line on these people, the better.

CDR. CRONKITE: Somebody has looked. We got a long involved report a few days ago.

DR. DUNHAM: This I didn't know.

CDR. CRONKITE: We just received it. I had not known it had been done either. It is something you initiated.

MAJ. HANSEN: I think it actually started with the return of the Air Weather personnel, and the other American personnel to Tripler at which time we asked Col. Lowry, who

is the chief at Tripler to evaluate all these people to establish a back line. Dr. Bugher came through at that time and went over such records as we had, and then went over with Col. Brennan and talked to Col. Lowry, and I believe he had a trip set up to go to Japan to look at some of the Hiroshima and Nagasaki people, and was asked to stop by and look at these folks at that time. I am sure that is where it began.

DR. DUNHAM: Fine. What does the report indicate?

CDR. CRONKITE: I will bring it down tomorrow. It is about an inch thick.

DR. DUNHAM: Did he find that much?

CDR. CRONKITE: There were three macular degenerations. There were a fair number of things observed, but whether they have any connection to radiation is somewhat questionable.

DR. DUNHAM: Not much in the lens itself?

CDR. CRONKITE: No, not more than would be consistent with some of the older individuals.

DR. DUNHAM: And he examined the entire group?

CDR. CRONKITE: Yes.

DR. DUNHAM: This is fine.

CDR. CRONKITE: With a very nice clinical record on each one of them.

MAJ. HANSEN: I might point out that in each and

every American, he found some anticular opacities, but which he felt were congenital or at least non-significant from a radiation point of view.

DR. DUNHAM: It would be awfully soon to be getting significance.

MAJ. HANSEN: Yes. One of the Air Force personnel did have an anticular opacity. This is down, and points out the value of having this. Dr. Lowry was perfectly willing to state that he didn't feel that it could be due to the radiation, but at the same time it may bring up a problem in the future.

CDR. ETTER: In that connection, Lesinsky has screened all the affected crew members, and he has found an 18 per cent incidence in what he calls the normal opacities, which he contends cannot be distingusihed from those which might be due to radiation. Out of his group he discarded two individuals who showed marked lens changes, both of whom had received heavy radiation in adolescence for acne. That is something that must be considered in their past history.

MR. HARRIS: The incidence of congenital opacity in mice happens to be in the neighborhood of 18 per cent also.

DR. DUNHAM: Are there any other possible ultimate effects? How about aging? Is this population group large enough to even begin to think about it?

CDR. CRONKITE: I don't think it is conceivable to

do any kind of population study on that because in the first place, you don't know how old anybody is, except the ones born since the American occupation. It would be impossible to set up a population study.

DR. DUNHAM: Should there be any special looking for cardiovascular disease, and setting up comparable groups between the Ureriksand Rongelaps?

CDR. CRONKITE: I don't know anything about it myself.

DR. DUNHAM: Does anybody wish to make a statement because this sort of thing has got to be ironed out because there are always kibitzers on the side as to why you didn't run a lipogenic index on this thing.

DR. DUNNING: Dr. Dunham, I seem to be alone here, but the thought came to mind of trying to jibe up the doses and present condition. As I recall, the events out there, the fallout was heavy enough so that it made their drinking water very visibly murky. They continued to drink this until finally stopped by their local leader. These are questions, not statements. Could there be anything there that would cause them to be nauseated as they claimed they were after the first day? I am trying to jibe up the idea of their physical condition and the estimated dosage.

Secondly, remember when Dr. Zsuzuki was here, he made a strong point that he felt the poor condition of the

fishermen was due to so greatly their extensive beta burns. Did those natives have enough burns, or would that be sufficient to have any effect?

In other words, did we have something here in addition to whole body gamma that might account for some of these physical conditions? I don't know. These are both questions. I would like to repeat also that the estimates were without any estimates of contribution of soft gamma to beta. I saidthat this morning, and I say it again, as well as the sky shine.

DR. DUNHAM: As far as the Japanese fishermen, I don't think the natives have lesions comparable to one or two of the fishermen whose scalp was a mass of exudation, from the photograph.

CDR. CONARD: I think it was more extensive than anything in the natives.

DR. DUNHAM: This man's scalp was just a mass of gunk. Certainly I think Ray would go along that there was nothing unusual in the way of systemic symptoms that could be related to the second effect of skin burns.

LT. SHULMAN: I think you might look at it as a local sunburn.

DR. HARRIS: Could you possibly conclude this apparently self limiting nausea that these individuals had, and they had some, did they not very early, could be due to

a local radiation effect? After all, these people got a fair number of REP to skin. There is no reason why they didn't get a fair number of REP to the lining of the gut in the same situation.

DR. BOND: Why should they get the same amount of REP to the gut lining?

MR. HARRIS: I mean just from drinking this water they had.

DR. DUNHAM: They were not great water drinkers. We know that.

MR. HARRIS: I know. I doubt whether it is correlatable, but it is an excuse.

LT. SHULMAN: Do you get nausea if you drink radioactive tracer doses?

MR. HARRIS: I don't know. It would seem to me that even if they didn't absorb too much, they might have gotten a fair number of REP to the lining of the gut which is rather sensitive to radiation.

CDR. ETTER: But isn't this very comparatively mild nausea, which they got earlier, consistent with the marked amount you got later on from a statistical standpoint? If they got this much blood depression, should you not expect them to have some nausea to start with?

MR. HARRIS: I don't know.

COL. BROWNING: These cases I was mentioning had

the same thing. They got down to 10,000 platelets, and 1,000 white count, and they don't have any nausea.

DR. DUNHAM: I think there is more the chance that nausea was chemical rather than radiological.

CDR. CONARD: There is a big psychic element in evaluating nausea and vomiting. The water was turned yellow and they were told it was poisoned, and they got immediately sick if they were told the water was poisoned.

DR. DUNHAM: Why were they told that they were poisoned?

CDR. CONARD: Because the water turned yellow.

CDR. CRONKITE: It was because they were getting sick.

CDR. CONARD: Either way, or both.

LT. SHULMAN: I have some observations on fertility. Are those to be included?

DR. DUNHAM: Dr. Shulman raises the question of observations on fertility. Does anybody wish to make a comment on that?

CDR. CRONKITE: My feeling toward it is very simple. We should not attempt to do any studies for fertility for obvious psychological reasons for natives themselves. It becomes a fairly personal thing for getting specimens of semen and prying into these things. It is difficult enough to get a specimen of urine, and feces, let alone inducing

masturbation on a large scale of Marshallese.

DR. DUNHAM: If properly induced. You don't know who the fathers are. You are dealing with a group where there is no control. You would have to use the Uterikans as control.

MR. IMIRIE: You could only use the control years since 1945, because before that it was Japanese.

DR. DUNHAM: And up to 1950 there was no penicillin to clear up the gonorrhea and keep the tubes open.

CDR. CRONKITE: In terms of birth per unit of time or anything like that, I think it would be meaningful, because the Navy cured the gonorrhea, and all the women are now fertile, as amply demonstrated.

DR. DUNHAM: Furthermore, the data in Japan suggests that as far as live births and so on are concerned, there are pretty good data on that. A lot of it where large numbers of people studied both control and irradiated population, and there is apparently no difference. There has been a general drop in birth rate in Hiroshima and Nagasaki, but it is the same in the control population as in the radiated. This group which has undoubtedly received all of them, something less than 200 r, I don't think you would expect to find a thing on overall birth rate. As Gene pointed out, it is not practical to do sperm counts and that sort of thing.

CDR. CRONKITE: I would like to ask Capt. English whether levels of radiation such as these children received possibly were a little increased to the dental germinal layer due to soft component, would there be any reason to examine these people by, say, yourself, or somebody else familiar with the radiation effects on growing teeth.

CAPT. ENGLISH: Judging from work we have done with swine and rodents, I don't believe this is a sufficient dose that you can expect to find enough dhange in the developing teeth, after they reach maturity, that you would find any changes. With swine, we were usually up in doses of 400 r before we found anything that was very pertinent, and with rodents on their continually developing incisor teeth, you get up in the nature of 1,000 r, actually we used 1500 r, in order to get the stoppage of enamel incidence. With the rodents it is a very marked chang,e, and you would not have to go that far for record purposes as a minimum change.

I would strongly suspect that 150 r would not show you anything. Particularly would you have the trouble of having a group whose nutritional conditions and health conditions in general are so varied that even if there were some minimal changes, you could not pinpoint it down to radiation changes, because hyperplasia can occur from numerous things.

I would not want to spend the time to do it myself in this range. However, I would like to have a selection of extracted teeth, not to point as far as the health of the population is concerned, but to see if there were enough deposited materials to make radio audiographs.

CDR. CRONKITE: If somebody were to collect teeth that had fallen, you would like to have them?

CAPT. ENGLISH: Yes, we would like to make sections of those.

MAJ. HANSEN: May I interject here that I was going to bring this up. Among the Americans we were fortunate to secure a few teeth that were extracted and save them. These were sectioned and are on nuclear plates. Dr. Reed at National Cancer Institute is doing this. His report should be through in another week or so. We have also been running control teeth. I had felt that this was a good move and remembered that these natives do have quite a few extractable teeth. If there are any taken out, I know the people at National Cancer Institute would be very glad to run the radio audiograph. Whether you would like to use control teeth from natives from other atolls or control teeth from around here, I think that could be arranged. He feels very sure that he can detect any level of activity at all. He has found some in the control already that is within normal limits, of course.

MR. IMIRIE: I made one observation here talking about dose. That is, we have talked about 150 r and yet Gene Cronkite has brought up the fact that the blood picture did not follow what he expected it to follow. You would not expect a large number of people to have nausea at 150 r. So many things are in disagreement with what you would expect from a dose of 150 r. We know on top of this there was a large dose of beta radiation. How much no one knows, and probably will never find out. I doubt if there is actually any laboratory animal experiments that have subjected a person to what would be equivalent of 150 r and superimposed on this a large factor of very high energies.

Isn't it reasonable to assume that if some of the other strange things have happened, such as the blood picture changing late, and so on, that some of the other things, like dental situation, and leukemia and carcinoma, and so on, where based on present experience, we would not think it would happen, that it may turn out to happen? Therefore, I think these people should be watched very closely. This is a unique situation that has never come into being before, and you don't have any animal comparison.

CDR. CONARD: There have been studies of animal changes in the blood of animals from beta radiation. There are no significant changes.

MR. IMIRIE: How about the two together, hard gamma

and superimposed soft gamma and beta on top? This is a unique situation.

LT. SHULMAN: In thinking about getting specimens, they do have an autopsy room at Majuro, and if somebody were to die from other causes, it might be worthwhile at least letting the physician there know what specimens you would be interested in.

DR. DUNHAM: I think this is very important.

LT. LOONEY: There is one suggestion with regard to teeth in autopsy, since we do know there is a marked variation in urinary excretion, we might be able to tie the teeth analysis into the people with autopsies and get a complete termination and indirectly work around to total body burden from urinary excretion. Maybe you could tie this down for future information, although it would not help the Marshall Islands.

DR. DUNHAM: Gene, you seem to be about to say something.

CDR. CRONKITE: I was thinking that when Vic or I or somebody else out there knows ahead of time of the various things that might be done, and everybody is willing and agreed to do them, it is fine, but if suddenly on the spur of the moment, you find yourself out there with dispatches coming in that everybody is putting on their afterthoughts on it, I personally would dispatch it up. You can't do it. I

am hoping that the people will get their thoughts on the record. Something like cremation would be a good idea. How in the world with the people's burial habits, how can you sell them on the idea of getting their bodies embalmed and cremated.

MR. IMIRIE: Don't they bury very quickly, the same day, I believe?

CDR. CRONKITE: Within six hours, usually.

DR. DUNHAM: Is there any further discussion of the data with relation to prognosis?

CDR. CONARD: One thing we might look for is possible premature graying of the hair.

DR. DUNHAM: I notice we are scheduled for a coffee break. If the coffee does not materialize any better than it did this morning, I wonder if it may not be smarter to go on to Item 5. I have no idea when Dr. Bugher will be back I prefer he chair that particular discussion, but the chances of him being back by five I don't think are awfully good.

CDR. CRONKITE: I would like to make a suggestion. I know Capt. Kellem was unable to come today. He is particularly interested in Section 5. I know Dr. Bugher is also. Perhaps it could be postponed until tomorrow.

DR. DUNHAM: It is quite agreeable to me. Is there anything to lift from tomorrow's agenda to shorten that? I suspect not.

CDR. CRONKITE: I would like to move to knock off early today.

DR. DUNHAM: And work late tomorrow?

CDR. CRONKITE: And if one has to work late, to work late tomorrow.

DR. DUNHAM: Let me check when Dr. Bugher is to come back.

(Brief recess.)

DR. BUGHER: I take it that the group thinks that there might very well be certain things resulting over the years with these people, particularly the Rongelap group. I presume that you would expect an actuarial contraction of life span as an expression of the radiation exposure. Whether one can appreciate that at all would depend upon good actuarial statistics of the Marshall Islanders of a sufficiently large population. That may not in fact exist. But it is reasonable that even with numbers that small, some difference may be shown. I don't know whether that was your consensus or not. It is something naturally to watch.

What was the opinion about leukemia?

CDR. CRONKITE: That was unlikely.

DR. BUGHER: On account of the small number of people

CDR. CRONKITE: The small number of people, and at that dose exposure, the incidence would be extremely low, based on the Japanese experience to date.

DR. BUGHER: The chance of getting a case of leukemia in 100, those people must be of the order of one in 100,000, or something of that order. If you had 100 times that probability, the chances of getting it in any one of 100 people would be quite small. So even if the leukemia rate were very much increased, you would not expect more than one case, probably, and if you had one case, you would not know whether to attribute it to radiation or not. I think I would agree with that.

What has been the feeling of the probability of skin carcinoma?

CDR. CRONKITE: Almost unlikely. There would be probably very little due to the absence of the deeper effects, there are no continuing ulcerations and the likelihood of neoplasia is considered to be rather remote. However, the fact of the rather continuous exposure of ultraviolet may increase the incidence somewhat.

DR. BUGHER: You would expect that to be much more likely to be recognized than leukemia, I take it.

CDR. CRONKITE: Yes. It would be more likely, but still probably would not be a major consideration.

DR. BUGHER: I don't know what the frequency of cutaneous carcinoma among these Miconesians is.

CDR. CRONKITE: I asked about that when we were out there, and I was led to believe that of what grossly one

would consider carcinoma of the skin would be infrequent. They did have some lesions like the one I think you saw on a man that was there that is a very indolent type of ulceration, presumably cancerous. They have a lot of internal cancer, but relatively speaking, very little of the skin. They only last year started vital statistics in the Marshall Islands.

DR. BUGHER: The population numbers are so small that it makes it difficult to reach good actuarial conclusions unless the differences are really huge. I should think that the probability of skin carcinoma should be fairly appreciable, and in people living to their fifties or sixties we may see quite a number of cases. That is merely my personal reaction to that. I asked Dr. Zsuzuki when he was here what he thought about that, and he discounted it completely and thought there was nothing at all of any interest there.

I rather had the impression he had enough worries now, and he was not going to cultivate any more. I was rather intrigued at his negative response to that, because that was one thing that seemed to me to be more likely recognized from all the changes which might exist from that level of exposure.

CDR. CONARD: I think a lot depends on how the skins look in the near future, and whether we have any further

breakdowns of the epidermis.

DR. BUGHER: Yes, that will be a component there.

I suppose now we come mainly to the mechanics of getting this material into a single report. Gene, I think inevitably the leadoff on that falls to you.

CDR. CRONKITE: Dr. Bugher, I wonder if it would be permissible to postpone that until tomorrow. Captain Kellem was unable to come down and he mentioned that is one thing he is interested in seeing what conclusions you come to.

DR. BUGHER: Would you like to take that up the first thing in the morning?

CDR. CRONKITE: Yes, if it were feasible.

DR. BUGHER: That is perfectly all right. Obviously we want a report which is complete and lucid, and which has all the pertinent data. If we can eliminate restricted data in this report, I think it would be desirable.

DR. BOND: Can you tell us, Dr. Bugher, what aspects of the report are restricted now?

DR. BUGHER: When we come to matters that are of trouble, if we can declassify it in almost the form it is with possibly the deletion of small sections, it would make it much more convenient, and will cut down the time delay. The things that we would have to hold as classified would be such things as the composition of the material with respect to Neptunium, any question of fission capture ratios of

neutrons, information which would suggest the content of normal uranium of the device, isotope ratios of the short half life ones that particularly give information on the type of device, and anything that might lead to any more clear estimate of yield than has been given.

I am aware that the Congress itself compromised the yield, that is, members of the Joint Committee in statements to the press. So that cannot be regarded as a sensitive piece of information as it was. But that is the sort of t thing that would bear on the questions of design, efficiency, proportion of energy released by fission as against thermonuclear reaction, and that sort of thing.

I don't think that inhibits one from giving the isotope analyses in urine, the estimate of body burden, and the computations of number of fissions ingested, for example, per person, the sort of thing we have on the board here. While it is classified now, I think we can declassify it without too much restraint.

CDR. CRONKITE: I don't thinkm there would be any difficulty in writing the report with no restricted data in it. After listening to the discussion on dosimetry today, I have more or less made up my mind -- I don't know what I will be able to talk the other people into it or not -- to summarize dose in one table as the best estimate and the statement of the uncertainties connected with it, and then

the people that are interested in dose to ask them to write a separate addendum amplifying the uncertainties.

DR. BUGHER: Yes.

CDR. CRONKITE: Then we get away completely from the Neptunium, and the unfortunate statements in the first report of talking about tritum and lithium and one thing and another, and our ignorance of whether they were important or not.

DR. BUGHER: Yes, I think that is a good point. When it comes to the proportions of isotopes in fission products in the fallout material with respect to those that are of medical importance, we have essentially released that information to the Japanese in the following form, that is, we have told them that the pattern of those substances followed the bimodal efficient curve for uranium 235 for fast neutron fission with respect to the modal regions, not with respect to the other. We gave them this information, that while we were not saying that what we were dealing with there was a fission reaction completely, that they could use those parts of the published curves and it would give them approximately the relative amounts of those isotopes, such as strontium, cesium, barium, lanthanum, that appeared to be in the material that fell out.

So that much of the statement is already essentially public knowledge, because if we have given it to the Japanese,

we have given it to everyone. Consequently, it gives us no difficulty in dealing with those quantities. The approach there has been that the longer lived isotopes, the ones that were important after ten days, are no longer capable of giving sensitive information or information in any sensitive area. Since it is accessible to anybody who wants to put out flypaper and do his analyses, it is inherently data that cannot be classified, or at least will give us no trouble in declassifying.

I see, therefore, no real difficulty in dealing with the internal emitters here. In fact, I can see some intelligence advantage to introducing the plutonium business. It would perhaps throw foreign intelligence services off the beam somewhat to introduce the problem of some degree of plutonium ingestion at a very low level. It might or might not. I don't think it presents a difficulty.

The Neptunium matter, if it were brought in, would be much more pertinent. But as you indicated, it is not necessary really to go into a detailed discussion. How the gamma spectrum got that way, the best you can do is to give an estimate of what it must have been.

To some extent we have to make a report to the United Nations, presumably ahead of the general public distribution. I think it will make quite an impressive document in that environment, too. So it may be that when

we come to that sort of thing, we have to produce it in essentially the form that it will be published.

CDR. CRONKITE: Could you say what sort of a time factor you have in mind with respect to the United Nations?

DR. BUGHER: It is the same old story. The faster we do it, the better, consistent with quality. This is something we don't want to do any job on that five years from now you look back and say, why didn't they take a little more time and put a little polish on this or that paragraph. So we want really a first class job. The sooner it is available, the better off I think we are.

CDR. CRONKITE: Apropos of that, I am very pleased that the preliminary report is secret restricted data, but after re-reading it, I wish there were a lot of things that were not said or said better.

DR. BUGHER: It serves its purpose as a draft. Do you want to make it top secret now?

CDR. CRONKITE: Yes, I would go to top secret. Under the conditions, and where it was written, it is not so bad, but it looks bad in Washington now.

DR. BUGHER: As a rush draft, it is a very commendable job, and it is the kickoff point for a more complete version. I have read it, and there are spots that you want to change naturally, and some deletions undoubtedly, but it is a pretty solid story. So those are the essential things.

I think we can get good help in AFSWAP, and all other places, in clearing it with reasonable speed.

CDR. CRONKITE: There is one thing I want to bring up again tomorrow when Captain Kellem is present. When there is urgency on the report, there becomes a real problem of discussing with AFSWAP to have just plain stenographic help. It has become an acute problem. AFSWAP doesn't know whether they can supply anybody. In the present status of the classification of the material, you have to have not only stenographic help, but the cleared stenographic help. The Institute is not in a position to do things on a rush basis. It will take a matter of weeks and months to go through MRI at the present time.

DR. BUGHER: How many people do you need for that?

CDR. CRONKITE: One person out there for a month or six weeks could solve the problem.

DR. BUGHER: Do you think it would be acceptable to Captain Kellem if you people recruited the person at our expense?

CDR. CRONKITE: I think that will probably be acceptable, but I would prefer to defer that until Captain Kellem hears about it.

DR. BUGHER: We don't have enough reserve now. We are short on secretarial help. So it will be a case of temporary employment of somebody who is cleared and who has

all the other attributes that you need for this. An ability to type I suppose is one of them.

CDR. CRONKITE: The ability and not the classificatio

DR. BUGHER: I am sure we can straighten that out. I presume you are in the same situation?

CAPTAIN YARBROUGH: Worse than ever.

DR. BUGHER: Are there any other points you would like to bring up now? If not, I think wecan postpone this item for the morning and adjourn this afternoon. If there any point which it would be wise to bring up now?

If not, we are just about on schedule, and gained three minutes.

(Thereupon at 4:27 p.m., a recess was taken until Tuesday, July 13, 1954,at 9:00 a.m.)

SECOND DAY

TUESDAY, JULY 13, 1954

9:00 A.M.

DR. BUGHER: Let us come to order, please.

We are glad to have Captain Kellum with us this morning, which enables us to take up the topic which was deferred yesterday until he could join us. That is the mechanical problem of getting out a single report, and how to divide up the work to the extent that it has to be divided.

As far as I am concerned, I look to Commander Cronkite, so in that regard I think it is a good idea to ask him to give us what his ideas and suggestions are for getting out the final report.

CDR. CRONKITE: I have been giving this problem considerable thought. I don't know that I have an adequate explanation or solution to the problem. As I understand from your comments yesterday, Dr. Bugher, in addition to the report, we are obligated to make to AFSWAP, to Colonel Browning and the field commanders, a report that would definitely have to be unclassified for the United Nations. Is that correct?

DR. BUGHER: That is what we will come to. I thought the final report preferably should be in a form which is easy to declassify. Perhaps it might then be declassified and either in its full form or with some

deletions to be in good shape to be used as a UN report, as well as for publication.

CDR. CRONKITE: Did I also understand that the report for the United Nations was to take precedence over the report for the task force?

DR. BUGHER: No. At least I don't think so. We are obligated here since this is a part of the task force program, to make the official report through the task force channels. You remember I talked to Dr. Graves, and he is agreeable to making these shortcuts which would speed up this thing, but in the long run he wants to naturally see the report in the regular way, too. I don't think we are asked to make a report to the UN prior to the routing of the formal report through the proper channels.

If we have to make such a report to the UN, and I am sure our delegation at the UN will not be at all bashful about telling us if they think they will profit by it, then we would have to do a quick job on our special report and clear that quickly for that purpose. But as of now, I don't think that we need to do that.

CDR. CRONKITE: Since I have had ample time to go over the preliminary report we wrote out in Quadjalein, and each time I read it I realize what an unwieldy document it is and how difficult it is to go through from chapter to chapter to make any sense. I am not apologizing for it,

because it was written in the field. It is still a pretty good report. It has a lot of inherent defects that should be corrected.

I think the first I would like to see done is to take the section which is now Chapter 2 on the dosimetric considerations, and have that drastically reduced. I would like to have Dr. Sondhaus, Mr. Sharp, and Dr. Dunning try to make some very short statement that would be acceptable to everybody, and preferably of no classified information; then in addition to make a separate addendum that would not be in the same volume that would go into all the material that is inevitably secret restricted data.

I think in this way it would improve the report for the average biological and medical reader so as not to have to wade through all the "ifs" and "ors" and "buts" and come out at the end as to whether or not there was a dose or not from a physical standpoint.

The next is the section on excretion and body burden which represents a tremendous amount of work. It is always a difficult thing to go through and understand. I would like to give that the same sort of treatment of cutting it down for our final report to a very short version, emphasizing the status or the significance of the excretion of the material, a statement on probably body burden, and leave out for this report all of the extensive animal data;

then to ask the respective individuals from Los Alamos, New York Operations Office, and NRDL, to submit rather complete addenda to the basic report where they can go into all of these imponderables that we discussed yesterday.

Then so far as the clinical and hematologic and estimates of the prognosis, and perhaps a discussion of future handling of this type of incident, I think we should go into more detail, and it would be primarily up to Dr. Bond, Conard and myself, and the others, to get this down in rather intimate detail.

I would like to see, if at all possible, the parts on dosimetry and the part on the excretion and deposition in the individuals, and our part of it, in firm form beforethe 1st of August. I think the addenda will come along as people can do it, perhaps before the first of the year.

Histomatology of the skin I think should be in detail whenever Dr. Wood is able to complete it, and be incorporated with Dr. Conard's section in the report. I think that takes care of my attitude towards the final report itself. It will contain no restricted data. I don't think it will contain any military classified data.

COL. BROWNING: No. When you sent that paper over the first time, there were a very few mentions in there of material that was really classified as far as we were

concerned. We are more concerned at the present time with the timing of the release of this classified or unclassified document for obvious reasons. I think it should be done, but I would like to wait until some of our friends get through insulting us before we give them more information to insult us with.

DR. BUGHER: If we wait until our friends get through insulting us, we will wait a long, long time. I think our point of view on these matters that we meet our obligations and what is or is not said or done by other nations does not cut too much of a figure in that. When we are asked for favors by other nations, we expect to be at least couched in reasonably printable language. I think the time on this report as far as release is concerned should be based on our considerations and advantage in complying with our obligations.

COL. BROWNING: Yes, sir. I would like to say perhaps one or two things about this report. Personally I would like to see it in one huge volume. As I recall from my medical school days, there was nothing that upset me more than to be referred to five other books any time when I tried to find a particular mention. However, I realize that this will be a continuing thing and must be a series of reports, rather than one, for some years to come. There will be no way to cut it off sharp, and say this is the final

report. There will always be something coming in. For example, you mentioned the skin studies.

CDR. CRONKITE: I see no reason why the addenda cannot all be put into one volume. We can give you a final report as I outlined it within a matter of a few weeks. To get a final report, particularly as far as the excretion and the animal stuff is concerned, is a long period of time. I have not had an opportunity to discuss with Dr. Cohn and Dr. Sondheus and the others, and Dr. Harley, what their attitude would be towards what I have just proposed.

DR. BUGHER: We can hear from them now.

MR. COHN: I think I would go along with the suggestion. I think I would perhaps want to include some of the animal studies which are pertinent to our understanding of the human picture, rather than really discuss the human picture, and leave off all this auxiliary information which I think is quite important. I don't think we can in a matter of two weeks get a complete report out on all the work we have been doing. I think I would go along with having the various other laboratories who are connected with this project submit a detailed report of all the work they have been doing at a later time. Perhaps to integrate our conclusions as of now we could do our first report. I would go along with that.

COL. BROWNING: It might be a little more fair to

a lot of these laboratories which have taken on this work to submit the reports as more or less their own work, rather than to throw it into this large one. I thought of this a couple of times in looking through the material that we have gotten from many other sources.

DR. BOND: Do these come out as general reports or reports from the institute? Would the Los Alamos report constitute an addendum report?

COL. BROWNING: I don't know that much about it. WT-90, one of the old ones, has just been released now in the same identical form of the special report of the laboratory which did the work. So there is no reason why this could not be done. But it would certainly be valuable to have all these in such form that they could be added to the rest of the report.

CDR. CRONKITE: One thing I forgot, Dr. Bugher, if it were acceptable to the people concerned, I would like strictly from the standpoint of it being easier for me to have Mr. Sharp get the stuff together on the dose. He is geographically in the same place I am. Dr. Cohn at NRDL is in direct contact with Bond. I think it would be easier for them to take the major responsibility of writing that part of the report. I have not discussed it with them. I don't know whether they are willing to accept the responsibility of doing it. I think it would expedite

completion.

DR. BUGHER: I daresay there is no difficulty of doing the various pieces according to the way that seems to be most effective.

DR. SONDHAUS: I think it would be inevitable that these addenda would include classified material. It is too unwieldy to have an unclassified report with a classified appendix.

DR. BOND: The whole thing would be declassified and you could detatch the classified.

CDR. CRONKITE: You could separate it and say pages 1 through 20, retype and send through for declassification. What is wrong now, every other page we have a little bit of secret and restricted data on it. That is about all I have to say about the report, except for how much pressure is there from the standpoint of time so far as AFSWAP is concerned, and so far as you are concerned? This becomes rather critical.

DR. BUGHER: I think our point of view is as I expressed yesterday, that we would like to have this Part 1, as you call it, of the report soon, but we also want it of top quality. If one could accomplish both of those things by the first part of August, that would be fine. It certainly would be extremely helpful. We don't want to delay. We don't want to sacrifice quality of presentation, either, nor

the quality of the reproductions of the illustrations. In other words, this report is one of the few which will receive a very critical international look, and we want it to be altogether to the credit of the United States, not only in the scientific content, but in its format and actual appearance. Those are the considerations as I see them.

Col. Browning, do you have some additional considerations from your point of view?

COL. BROWNING: No, sir, because in essence we have access to most of the information at any rate, and as far as our headquarters is concerned, we can always use the raw data. However, we are no less anxious than the rest of the group to have the thin put out in such form that it can be made available to the general medical profession. Our stand is not the old hidebound school type We think this should be out so it can be used.

DR. BOND: There is a practical consideration here if we have the colored metal plates in the report.

CDR. CONARD: It will take considerable time on those, I am afraid.

DR. BUGHER: How much?

CDR. CONARD: I don't really know.

CDR. CRONKITE: It will be about six to eight weeks if we can contract locally. If we have to do it through Los Alamos, Lord only knows how long it will take.

DR. BUGHER: Captain Kellum, how does this plan sound to you?

CAPT. KELLUM: This sounds all right, sir. I would like to make two or three remarks in a general way to support one or two suggestions that I have a feeling are acceptable from things that have already been said.

Like all other laboratories we are under the necessity of justifying our existence budgetwise, and one measure of our productivity is the reports of our scientists either in our own format or in the form of reprints. I remember some years ago when this business first started. I was new at the Institute. I found that roughly a quarter of the total effort of the Institute was not recorded anywhere. This went into the Bikini reports, and this was in no sense a criticism of anybody. It was just the way the thing went, and people had not thought about these matters. Since then, there has been more consideration given to what Col. Browning just mentioned, and that is getting the reports in a form that they can appear from the laboratory as well as an AEC report. This I appreciate very much, and I would like to put in a plea for further consideration of that.

The other item that bothers us a little bit, we don't mind dropping everything to get these people under way when there is an emergency to do it, but preparing reports is a bit of a burden for us because we are relatively a

small activity. The clerical force is kept at a minimum because we are anxious to use our civil service and military ceilings to best advantage, and probably invest more in scientific personnel and technicians than we should. The result is that when we come back with a big push to get a large report out in first class shape, we are in a difficult position without a little additional qualified help.

I would illustrate what I mean by the comment that the individual on our staff who is best qualified to carry the burden of getting this report on paper is currently fulfilling the functions of chief clerk for the whole establishment. We obviously can't take her off that job, because there is not anybody else to do it. I would put in a plea for some consideration of assistance in this field.

DR. BUGHER: I am sure that can be arranged. Cdr. Cronkite mentioned yesterday the sheer burden of detailed labor which is involved. There is one place we can assist, I think.

CAPT. KELLUM: I might say we are not looking at the money balance on this. We have been well supported by our own budget, and by funds made available for these occasions. It is the civil service ceilingsthat get us.

DR. BUGHER: Captain Yarbrough, do you have any comments on this?

CAPT. YARBROUGH: I have no comments on this

particular subject. I do want to say some thing later when it is posed on the further study, about the utilization of personnel. We are encumbered with ceilings, as anybody else, and as far as furnishing secretarial or clerical help, we are more or less handcuffed by much maligned ceilings.

DR. BUGHER: Are there other comments or suggestions with regard to this plan of information? This seems to be a ve sensible one. This seems to develop a compact straightforwar hardhitting report which carries all of the solid information and the results of computations and is accompanie by an addenedum which may be more voluminous than the first section which will include in it all of the classified material, data, prolonged discussions, and so on, which then could be made available to those who are interested and feel the need of going more critically and minutely into all of the background data.

The No. 1 section would be the base report from which special reports might be prepared, and that itself should probably be in pretty much a form which would be ready for publication on being finally declassified and made available for that purpose.

I am sure if something occurs to anyone, we would be very pleased to have additional suggestions. But it seems to me that it is a very logical way of going at it.

You, then, Gene, make assignments of division of labor. There has been no objection to any of those suggestions. I take it they are generally agreeable?

MR. HARRIS: I can't say this is an objection, but as far as we are concerned out there, we will go ahead and write a report that will come out as a Los Alamos document of some sort. I personally don't like to write very much, so I would be very glad to give you all the data that I happen to have available, and you might write as much as you wish about it.

As far as the addendum goes, we can offer you an entire report that you can include as an addendum if you wish to use it, and then you can discuss it at length if you wish to do it. But as far as we are concerned, we will discuss it once, and then forget about it, because we haven't got time to go over and over it.

DR. BUGHER: I think your addendum idea incorporates all the special reports.

MR. HARRIS: Would this be satisfactory as far as what little we have done? You can take the whole report and include it as an addendum.

CDR. CRONKITE: It is perfectly adequate as far as I am concerned. My main thing is to get the gist of your data in Dr. Cohn's hands so he can put up something in that section of the report.

MR. HARLEY: One problem on this excretion data and so on is that so far no one has taken all the data and put it together as far as I know. You have looked at your data, and we have looked at ours, and everybody has looked at theirs.

MR. HARRIS: I understand this is Ken's job to do. I think this is fine.

MR. HARLEY: Somewhere he has got to get this before he can do his quick job.

MR. HARRIS: As I say, you are welcome to it. As long as I can get it out through the various machinery of the mailing system at Los Alamos, you can have it.

MR. COHN: You will have that for afterwards or somet ing that we can use right now. If it is a question of two weeks or something, I don't know whether we can wait until we get to the front office or not. We have to have some time to analyze the data before we write it. This is a problem.

DR. BUGHER: I think that is something you can resolve among yourselves.

CDR. CRONKITE: I want to bring a question up that may not be quite appropriate, but I have just been reflecting on the thing. All of us have understood that under the regulations of operating things in connection with the task force, that the first obligation is a report to

the test director, and then afterwards you put out your own reports. If Los Alamos can diverge from that, I think the rest of us have been getting a rather dirty deal. Why a report from Los Alamos and not a report to the test director?

MR. HARRIS: Because we were using a few facilities of the test group but we were only using them in a logistical line. As far as we were concerned, we were not members of ay particular project or any other group at Eniwetok. We were operating out of our own laboratory, not as a part of J Division or any of the test groups.

CDR. CRONKITE: Just because I am a little concerned about it, I have heard many times from Dr. Graves that this is exactly what should never happen.

DR. BUGHER: I think from our point of view that all those concerned in the study were operating as a part of the joint task force. The program was set up under that, and there could be no other participation. I don't think, Payne, as I would see it, that your point of view would hold here.

MR. HARRIS: The thing about it was that we went out to get these samples, and we had no knowledge of this whatsoever. We did not know that there was such a project established until arriving at Quadjalein. We had no knowledge of this even from our own place.

DR. BUGHER: Tom Shipman had knowledge of it from

me over the telephone.

MR. HARRIS: I didn't know it.

DR. BUGHER: There was no uncertainty there.

MR. HARRIS: Whatever way you want to handle it is all right. As far as the Los Alamos report is concerned, it is an ordinary Los Alamos document. There have been many, many reports which have shown up as WT reports from tests, but also came out as Los Alamos documents before or simultaneously with the appearance of a test report. That is what I mean by a Los Alamos report.

DR. BUGHER: This is precisely the same as the chemical samples on cloud samples done at Los Alamos, or anything that has to do with Operation Castle. It is a Castle report.

CDR. CRONKITE: I am sorry for bringing the subject up, Dr. Bugher.

DR. BUGHER: That is purely a matter of channels of transmittal. I think there is no question about the character of the project, and where it belongs in the scheme of the task force, as all of the material is a part of the Castle operation udder a definite title. That was the decision of the commander of the task force and scientific director, Dr. Graves. We concurred in that.

MR. HARRIS: Then this brings up another point of holding up the release of a report. Doesn't this have to go

through the test director, or somebody like that?

DR. BUGHER: That is right.

MR. HARRIS: It goes through there before there can be made any sort of release publicly of this sort of thing?

DR. BUGHER: That is right.

MR. HARRIS: This essentially throws it into the Los Alamos declassification system, is that right?

DR. BUGHER: That is correct. The only thing that has been done which is somewhat irregular, you might say, is to reach an understanding with Dr. Graves that certain short cuts will be made at this end in order to get the material in shape more quickly. But having done that, the document still clears through the established channels. There is an attempt here to gain time, for example, on illustrations. Normally those would be done out of Los Alamos.

MR. HARRIS: Don't you think if you talk to Al about this, he would be very sympathetic towards this procedure and it might be that he would speed up such things as declassification of plates, and so on?

DR. BUGHER: He will try to, yes. Ralph Smith is also quite aware of these things. There is every effort in prospect to accelerate the thing within the framework of the task force. But it doesn't mean cutting corners and missing the intersection here.

DR. BOND: How soon can the data be made available?

MR. HARRIS: I can make it available mainly by just summarizing everything, like I have now. This is the work of a day, and putting it in a letter form and sending it. That is all. My holdup is this: If I don't put it in a letter form ahd send it out that way, then it bypasses the classification people at Los Alamos, and they might come back at me for bypassing them on this sort of information.

DR. BOND: Would it be possible to get that at a very early date? I think the point is that Dr. Cohn is to do the job of primarily integrating the data, and without your part, he cannot proceed.

MR. HARRIS: All I have to do is write it in the form of a letter, and I can do this immediately when I get back.

DR. BUGHER: I think that covers that problem, and undoubtedly from time to time we will have some minor difficulties, but there should be no trouble in resolving them. The general procedures, I think, of the task force are quite clear. Is that enough, then, Gene, on the mechanics of this?

CDR. CRONKITE: I have nothing else. It seems quite satisfactory.

MR. COHN: One more thing, Payne. If you are going to have a summary, you will probably have to have more

than that. We will probably need your raw data.

MR. HARRIS: We can talk about this later. There is no use taking up time here.

DR. BUGHER: Now we will turn to the long term medical care and study problem. I see I am listed for that. I will tell you what we have done.

OUTLINE OF EXISTING PLANS FOR LONG TERM MEDICAL CARE AND STUDY.

The situation which arises, of course, is unique inasmuch as this gets into fields of responsibility and authority where the lines are not automatically sharp and clear, and where we have the problem of groups with authority without capability; other groups with capability and without authority, a situation which involves us inevitably with other countries to some extent.

Then you have the immediate problem of following a relatively small group of people who are not familiar with and do not understand any of these things that I have mentioned previously.

To meet all of these things we have by fairly general agreement made certain arbitrary decisions in the hope that they are based on logic, but they have certain degrees of arbitrariness, as you realize.

In the first place, these people are not United States citizens. The territory on which they reside is not

American territory. The United States under the United Nations Trusteeship Agreement is the governing authority. It does not have sovereignty. That is one aspect.

The governing authority has an administrative organiz ation under this trusteeship agreement. That administrative organization has its home base as far as Washington is concerned in the Department of Interior after the transition from Navy to Interior for that function. The central office of the administrator of the trust territories is presently at Honolulu, and will probably be moved either to Guam or Truk within a few months, the idea being to make it more central. But at least that is the administrative centers.

They have various district centers and administrators and that is the administrative framework which exists. It has seemed to us that any departure from that administrative pattern would run at once into the questions of legality and even more importantly from the practical standpoint it would lead to confusion. Therefore, it seemed very important that the Marshal Islanders themselves, as they look at things, would see only one agency. That is the one they always deal with, namely, the Office of the High Commissioner for the Trust Territories.

There we have the example of authority without the capability. The High Commissioner does not have the scientific

staff, he does not have the logistic resources which would enable that organization to do any of the things that must be done here.

In the area of medical responsibility, the Atomic Energy Commission has accepted the responsibility for continuing studies indefinitely in the same way that we have the responsibility for the studies in Japan. The Commission has resources scientifically of varying character, but not all of them, and does not have in itself the necessary logistic support in the Pacific Area.

So here again agreement first with CincPac that the Navy would undertake to support as necessary the question of transport, supplies and so on; in so far as possible certain of those activities have been charged to, and the costs recovered from, Joint Task Force 7. But this organization, of course, is one that terminates after a time and is succeeded by another one. So that it is clearly recognized that the task force could only be economically responsible for a short period of time, and later on the question of cost might have to be resolved in some other way in regard to logistics. However, that is something that is more of a minor problem.

We should also recognize that when it comes to a question of performance, the capability rests in various places. It has been our thought that as far as is possible,

the same group of people who have done the basic study should continue with interval surveys and detailed studies of these people over the succeeding years.

We realize that faces will change. Some of you will go to other posts, and some may go to other appointments entirely. But as far as possible we would like to see a continuity of interests here, and participation.

The routine, I might say, normal medical care of these people will be assumed by the trust territory. For example, the people at Majuro and the Rongelap people will be looked at and watched over as far as their daily ills are concerned by the medical people there, and would have the services of a hospital. When they go back to their home atoll, it may be necessary to set up some sort of a special station there which would make it possible to carry on a dispensary service on a considerably more elaborate basis than they had before, which was nearly nothing, and also to furnish a base of operations for the teams that would presumably go out at intervals, the intervals perhaps getting a little longer as time goes on.

This latter type of thing could also b e carried on through the trust territory administration. Our job is to see that it is done, and that the facilities are provided. Where the High Commissioner gets his facilities is something between him and the rest of us, actually. But as

far as the people are concerned that is an activity of the Commissioner. I think there will be no difficulty about any of those things.

The project itself has been one of joint participation. The Navy, of course, has contributed very heavily here in the matter of personnel and time and thought, and it is simply a simple testimony to the fact that the people who have been interested and working in these fields have predominantly been in the two Naval Research Institutions.

Those are some of the factors in the pattern of responsibility and organization. Our objective is to maintain a smoothly working situation so that the continuing medical stddies can go on indefinitely, I think.

The discussions yesterday on prognosis emphasized especially the long term end results which can only be appreciated by following these people over many years.

In a letter to Admiral Pugh, which has come to the various persons concerned, I outlined the background of the problem, the way it was handled, the results to date, and itemized the objectives of continuing investigation as I think we all pretty much agree at the present time should be kept in mind. These are as listed here.

I am talking now of these interval examinations. A complete physical examination and interval history. Second, hematological studies, including quantitative

examinations, such as hematocrite, white blood cell count, differential count, platelet enumeration and bone marrow studies.

Three, special investigation employing both color and black and white photograph, as well as skin biopsy if the latter are indicated.

Four, opthamological studies with special reference to the lens. This will obviously come in with a little time to fit in with the studies in Japan.

Five, special growth studies of children, including attention to the development of dentition. I believe that was mentioned yesterday.

Six, the progress of pregnancies, and the status of newborn infants. I don't think much comment is required there except that essentially it be a documentation of nothing happening in all likelihood.

Seven, quantitative studies of internally deposited radioisotopes by means of urinary excretion measurements, external radiological measurement and localization, together with such radiography as may be useful. That is a euphimistic way of saying if people die, we want full autopsies.

Eight, environmental surveys of the affected islands and atolls and appropriate examination of the animals left on the contaminated islands. In other words, the project needs

to be pretty much of a rounded one, and include the continuing environmental study as well as the continued medical study of the people themselves.

Now, our feeling was, too, that various groups of people have special interests and would like to have sample material of various kinds. As far as possible, the groups concerned with the interval study should attempt to provide those samples. If the Department of Agriculture wants a soil sample, we should provide it. There is very little real difficulty about matters of that sort.

It is assumed, then, that the financial responsibility for these studies and investigations is assumed by the Division of Biology and Medicine of the AEC, and we work out the details as we go along as far as how we do that is concerned.

I believe the general objectives and so on from the position of the Surgeon General of the Navy have been agreeable. CincPac and Admiral Persley has given it an unreserved backing for this project. The administration of the trust territory is quite happy at the rather simple and straightforward relations through their organization. I think they feel that it bolsters them and strengthens their standing in their administrative responsibility, rather than diffusing it.

I think as we see it those are the main objectives.

There will undoubtedly be problems arising that will have to be resolved from time to time. The main thing is that we keep a project going in continuity more or less independently of the individual task forces that come and go. Of course, it would be somewhat associated with each and every one. Those are the general thoughts, Captain Kellum, I ha on that part of it. I would like to have your comments, and Captain Yarbrough's, on the general project.

CAPTAIN KELLUM: I think I am not in a position to speak for the Bureau, but from our own point of view, we are, of course, very much appreciative of the opportunity of participation and look forward to continuing our participation and support.

I can't miss this opportunity to comment briefly on the general spirit of good will which has prevailed through all of these successive operations, and which has made possible the smooth cooperation of representatives from many different agencies with What appear to be first rate results.

DR. BUGHER: Thank you. Captain Yarbrough.

CAPT. YARBROUGH: I would like to echo Capt. Kellum's comments on the affability of the relationships in this particular endeavor. In fact, I would like to go a little further and say that I think that it presents an opportunity for our naval participants perhaps to solve some of our problems.

Particularly I refer to the military, where there is the ever-present problem of affecting continuity for our research career people. We are always presented with the requirements of certain of our bureaus to comply with military regulations with regard to rotation of personnel. More specifically I refer to the fact that we get a good scientist, we give him a job while he is in uniform, he gets going on a job, and when he is beginning to be productive, along comes the necessity of transferring him someplace else, probably where he cannot proceed with the same line of work.

Therefore, I feel that this perhaps is an opportunity when we re-visit the scene of this accident, I suppose is the proper term, that perhaps we can effect a change of orders for these people where they will be in a sea duty status. Although the time might not be comparable to the requirements of certain bureaus, I believe merely the change from shore duty status to sea duty status will satisfy the regulations of particularly the Bureau of Naval Personnel.

Secondly, I feel it is an opportunity to further another desire of ours, which is to somewhat follow the thinking expressed by Dr. Bugher in the way of continuity in that we like to keep together people in a unit who are capable of performing such tasks as this. We do not like to get them scattered over the face of the naval concentrations.

Perhaps we could use this so to somewhat keep up with these people and keep them in a group that can be sent out to do -- I don't like to say the word "crash" studies -- but do a rather complete study on a crash basis, which is perhaps better phraseology.

I think those are most of my thoughts at the moment, except I think the most logical next consideration should be the matter of intervals. What is the interval that we should pursue in making this study.

DR. BUGHER: Thank you, Captain.

Col. Browning, the AFSWAP participation falls on your shoulders. Have you comments on this general plan?

COL. BROWNING: The conduct of this further study quite obviously falls within the purfiew of the AEC, not only by fiat, but because you are particularly well set up to do this sort of thing. To carry on a continuing study that none of the services by themselves or as a group could possibly manage, in other words. You are the most appropriate agency also because of your various laboratories to collate this information and make a continuous study of it.

Our interest in this, of course, is, one, that we get the information that comes out of this, such stuff as is operational and material that we can use, and we have another interest at this which is perhaps a bit at variance with what Capt. Kellum and Capt. Yarbrough mentioned, in that

we have responsibility to all three services, to try to continue an interest in all services in this business, and not look merely to one.

With all due respect to the work that Navy has done, and certainly they did everything they possibly could in this study, our feeling would be that we would like to interdigitate one or two officers of the other services, certainly qualified people, and not use this as a training situation, and to interest people in this who perhaps now feel that they have been arbitrarily shoved aside in this.

As I say, this is a continuing problem that we have. We do feel that all three services must have the capability of carrying on their own studies and their own work within their own services.

DR. BUGHER: It would be a helpful point if you would get available or have kept available in the Army, for example, some of the people who have now years of background and special training in this field. It would be a very substantial contribution towards keeping that capability alive.

COL. BROWNING: Yes. I am fully sympathetic with the Captain's problem here, because I realize that the Navy has had this situation for some time, and that they are fighting it. As a consequence, I have kept myself out of their side of the thing, other than to give them a little

moral spport.

However, on the Army and Air Force side, this has devolved in our fight, and we are having exactly the sam problem with the personnel officers, who insist on training a radiological officer into Lower Slobovia to replace a general medical officer in Ward I with complete disregard to the fact that he is not doing his primary function.

In the three years he is there, he loses all contact and much interest in the field. We have spent a lot of time and taken a lot of the services' time in training these people, only to lose them. We have found in this regard that the AFSWAP training course is a good stepping stone into radiological residencies, and we have lost over 50 per cent of our people in this because they do achieve a certain amount of permanence by doing it.

CAPT. YARBROUGH: I think you answered a question I was about to ask. Are you speaking for the Air Force also?

MAJ. HANSEN: We have lost many people. Almost everybody that has gone into the program has gone into specialty training from this program.

COL. BROWNING: Dr. Cronkite, at the end of the operation, sent a letter through channels concerning his feelings on keeping these teams together. As usually happens this went through several offices, and arrived at our place for some sort of answer. I have been working on this now, and

coordinating it in advance, and it should be coming through shortly. It will be our recommendation to know more on this situation, and will just about cover the things you mentioned, Capt. Yarbrough. It perhaps willhelp you in establishing your position. We do feel, one, that all three services should at least have on paper some sort of list of people who can be made available on short notice in the event any such accident as this occurs.

Again we are not trying to get into the individual services and tell them what to do in this, but to in effect give them the benefit of our experiences recently and indicate some of the ways in which this could be better expedited.

Of course, we also have a little personal interest in this, because these three lists will help us in the event of an inter-service type of thing forming into an interservice team.

CAPT. YARBROUGH: It is perhaps obvious that I am gathering some ammunition for another submission, particularly to the Military Coordinating Committee on Medical Research, where our ultimate aim is to combat the current imposed ratios by getting a recommendation out of Mr. Quarles, and perhaps out of Dr. Berry, that our billets for research people should be exempted from the currently imposed ratios.

DR. DUNHAM: May I make a comment here, although

this is something in my direct interest. Dr. Bugher will be back in a few minutes.

I hope you people in this battle, as it were, for keeping some of these highly trained people more or less together and certainly available, are keeping in mind the importance of the enlisted personnel, and not just officers. Watching Gene's team out there, it was obvious that it was the fact that the enlisted personnel had worked together before -- boys from Hunter's Point and MRI that really made the thing click, with all due respect to Gene and the officers involved. They knew each other and worked well together right from the very beginning. This is something that is very important to bear in mind in this consideration.

CAPT. YARBROUGH: Particularly I think the isotope technicians are getting scattered to the four winds, where people who have spent time and money in training are now in Lower Slobovia or somewhere else.

CAPT. KELLUM: Dr. Dunham, may I make one brief comment in passing? I think we all recognize the fact that this last team that went out that was mostly Navy was largely fortuitous. There is no disposition on the part of our outfit to try to corral leadership in this field.

As a matter of fact, and this is what I really want to say, from our point of view at Bethesda, we would welcome, if there is any point in it, from time to time the assignment there on duty of people from the Army and

Air Force. I think there is good precedent for that, and if there is an occasion for that kind of thing, we would welcome it.

DR. DUNHAM: I am sure when Col. Browning made the statement that some of the other services felt they had been shouldered aside, he is aware, as I am aware, that they were not.

COL. BROWNING: No, I don't mean on this particular incident.

DR. DUNHAM: As I recall, you made great efforts personally.

COL. BROWNING: This goes back to the same situation. On each occasion, it would have necessitated the complete dropping of a particular function. I realize that the Navy also had this problem, and they were willing to do so. Because of the time basis on which this thing came, it was not possible to make all the representations that were necessary to get these people involved in this thing. By doing this in advance, we feel that some provisions can be made to have understudies. I can speak with some sympathy for the Captain's statement here, because I spent two years at NRDL, and I look back on it as a very interesting and very instructive period.

We again would like to see this occur. This is another one of our recommendations. But we can do no more

than recommend and as often or not, it is only from our position that we can make these recommendations.

DR. DUNHAM: Well, good. These are generalizations. I think everybody is agreed as to what must be in the future. There is a matter of the return trip out there coming up fairly soon. I think perhaps this is the point in the discussion to get down to specifics. I understand there is actually an existing plan for Dr. Bond to go out with a group. Do we consider this an accomplished plan?

CDR. CRONKITE: May I interject a comment? Dr. Bond and I made sort of a gentlemen's agreement with each other providing that it was acdeptable to everyone else, that we would share the responsibility in alternating in going out there in the first few trips. I would like to twist Dr. Bond's arm a little bit to take the first one going out. I think from here on he should be the one, if he is to be responsible, that is able to select the people that will go with him, and what of the various things that are proposed they would be able to do. It becomes a rather acute problem of a few people for a long time, or relatively large number of people going out for a short time.

DR. DUNHAM: We would appreciate your comments, both Dr. Bond's and yours, Gene, as to which is the most feasible, and also useful approach to the problem. What do you mean by a short time, and a long time?

CDR. CRONKITE: I frankly don't know. My personal inclination would be to perhaps take more people out with the idea of doing the work in the shortest period of time possible so that there will be less disruption of the ordinary activities at home.

DR. BOND: May I bring up this point here. Dr. Burleys had or included in his list of objectives resurveys of the islands.

DR. DUNHAM: Yes.

DR. BOND: The individuals who are most interested now are located on Majuro. The islands themselves, Uterik, and so forth, are some five or six hundred nautical miles away. I wonder because of the geographic setup and the attendant logistics difficulties, whether it would not be proper to consider these as two essentially separate projects that might be coordinated.

DR. DUNHAM: It seems to me so.

DR. BOND: I would like to know whether we are including that in the discussion or whether we are discussing the return at this time.

DR. DUNHAM: Before we make the decision on that I would like Dr. Dunning to make a comment about what he is aware of in the way of plans for resurveys and any suggestion he might have as to how best to accomplish that.

DR. DUNNING: I am not aware of any definite plans

for resurvey.

DR. DUNHAM: Is Donaldson's group going out there? There is no point in having two or three different outfits picking up soil samples, flowers and cocoanuts.

DR. DUNNING: It would appear to me that the purposes of thisstudy and for the objectives for which the team is going out there that it would be of secondary interest at most to notice what the levels are at the islands now and in the future. In other words, we have all the data right now that is going to be of direct interest for determining dose and so forth.

DR. BOND: I was thinking more of the internal considerations rather than the external.

DR. DUNHAM: When it is safe for them to go back or is it safe?

DR. BOND: Along that line, I would like to state that the individuals at NRDL in the chemical technology division, of course, are interested in the general problem of contamination and decontamination. Here we have a situation in which an area that has vegetation that is used as food stuff has been contaminated, and these individuals at NRDL are very much interested in sampling these materials as a function of time after detonation to determine the distribution of it. How much of it gets into the edible vegetation there, and this sort of thing.

We are interested in the sampling and conducting radiochemical analysis. They are actively interested in making a survey. They are not completely aware, nor am I completely aware, of what has been planned or what is to be done on this score.

I would like to put in at this time that if there are no specific plans, and if it were desirable and acceptable that there is a group there that would be interested in taking the primary responsibility for doing that.

DR. DUNHAM: As this particular resurvey has medical implications, I would appreciate your comments as to when is the best time to make it, and also how often it should be done. I suspect not very often, but there certainly will have to be done at least one survey done well in advance of the first guestimate as to when the Rongelap natives can go back to Rongelap.

DR. BOND: The present plan is that the initial survey can be conducted as the survey of the people out there, and depending on what is found at that time, a date could be set for another resurvey, tentatively six months.

DR. DUNHAM: In other words, there would be two resurveys before they go back to their atoll?

DR. BOND: Yes.

DR. DUNNING: By whom?

DR. BOND: Here is a group that is interested in

doing it. I might elaborate a little bit. They are radiochemists primarily. However, in conducting the survey they would want very much to have the cooperation of such people as Donaldson, perhaps individuals in the trust territories, and individuals who have worked with the uptake of radioactive materials in plans, to assure that proper sampling is conducted, and that they do get the proper vegetation, and so forth, so that their results are meaningful.

As I say, this group would be interested in taking the responsibility for seeing that the resurvey is properly accomplished.

DR. DUNNING: Dr. Dunham, it seems to me that the sensible way to go about this is to find out what plans are in the making. I am sure I don't know all the plans of the Donaldson group and NRDL, and the carryover from Task Force 7. We should find out what is in the wind. Again I am not sure as to where we stand, shall I say, legally. But it would appear to me again that we need a central agency for someone to get this thing coordinated. It is just that. It is not the idea of giving commands, but of coordinating the efforts just the same as this whole medical team going out. Perhaps we need another similar program on the physical side of it.

DR. DUNHAM: Dr. Bugher, we are currently discussing the matter of resurveys of the natives and islands. Dr. Bond has made the suggestion that the two not be considered as

identical efforts necessarily, because the natives are down at Majuro, and have an entirely different logistic setup as opposed to returning to the islands.

The discussion has gone so far as to Dr. Bond offering and urging that the NRDL group be permitted to be the group to resurvey the islands, perhaps in September and again in March, with a thorough survey of the plants, soils and food supplies.

The question immediately comes up, what other plans are in the making or actually under way for resurveys of those islands from a radiological safety standpoint, and the standpoint of the food chain possibly being contaminated.

Is Donaldson's group going to do anything there that would overlap or duplicate such a proposal?

DR. BUGHER: Yes. The existing things, I think, are these. The marine biological side of it is immediately in Donaldson's hands. In that capacity he reports to this Division. However, his work does tie in with some other aspects of the Pacific Science Board and fans out in various ways, even including the University of Hawaii.

The main responsibility there for the marine biological situation is in Donaldson's hands, particularly with reference to the fish.

We have also on Eniwetok the small biological station which we have set up which is available not only

to government organizations, but to university marine biologists who may want to spend time studying some aspects of the coral atoll. Some of these studies, at least, should bear on the environmental thing.

We have in the Division a large program which is purely environmental contamination studies which we carry under the name of Gabriel. The scope of that program is worldwide. The problem of the biological setup of the contaminated islands is clearly likewise germane to that program. That brings us into close cooperation with the Department of Agriculture, because some of the outstanding skills in soil composition, soil analysis, and so on, lie there. We have an extensive cooperating program there.

We also have a very elaborate setup for analysis for longer lived isotopes. It is set up in three places, the New York Health and Safety Laboratory, the Columbia University project, and one in Chicago. So that the environmental aspects here are quite broad. Any group that does the intermittent surveys will have to plan that it will be not only wrking for itself, for its own interests, but also a service group for various other outfits who likewise have very pertinent interests here, and have available skills and resources which perhaps would not be entirely available to any one particular group.

You all had that problem confronting you with

Quadjalein's operation when Cdr. Cronkite began to feel that he was the chief dispenser of urine for the whole Pacific area and even suggested a different code name for the operation, the name of one of the more popular and decorative woods in the Marshall Islands, which I thought was a very nice name. The species known as the Pissonia Jiant. However, we work it for the actual handling for the immediate environmental survey, that group is going to have to do a lot of specimen collecting for other people who are interested.

Our general feeling is that whatever group has a legitimate interest and capability we should get the material for them and expect from them a report of results to go into the hopper here. That is the way it has been working.

DR. DUNHAM: I might interject one remark here that just occurred to me, Dr. Bugher. Are we to consider indefinitely that these surveys have to go through the task force report channels, or is there a cutoff point when they become sort of on their own?

DR. BUGHER: The cutoff point is when Task Force 7 is no longer active. What is that situation, Colonel?

COL. BROWNING: As far as I know, Task Force 7 will go out of existence some time this summer. It should be some time during July, as the last guess on that.

DR. DUNNING: The last I knew, Admiral Bunson was taking over at the end of July.

DR. BUGHER: The real problem that the authority outside Eniwetok and Bikini lies in the trust territories administration. He is the one that in a sense puts on us the requirements to satisfy his needs. So we all in a certain sense become a service facility to him.

In practice actually he recognizes that the capability for planning and all that lies in this general group of agencies and people. If he finds that he can't answer all the questions that are asked him, he may ask us to do some things that we may not have thought of. But generally speaking, that is our line of authority and our general responsibility.

The point you raise is a good one, especially the situation while the people of Rongelap are down on Majuro Atoll, and it may therefore be practical and convenient to submit the thing, particularly during this period. Is that what you had in mind?

DR. DUNHAM: That is what Dr. Bond was suggesting.

DR. DUNNING: Let me ask Dr. Bond in the light of what Dr. Bugher has just said, would you still give the same expression of interest and willingness?

DR. BOND: I believe so. Of course, they are interested in obtaining the samples and doing radiochemical analyses on them, and following the uptake material into the edible plants. They are willing to do this obviously on

samples that someone else collects or they are willing to go out and collect the samples, and have stated they would be quite willing to collect additional samples for other interested agencies.

DR. DUNNING: I am wondering then if one possibility might be that NRDL actually do the shovel work and someone act as coordinating agency to see what the needs of the other people might be?

DR. BOND: This may be. Who would that be likely to be?

CAPT. YARBROUGH: I think NRDL has gone a little further than Dr. Bond is indicating, in that this morning we have a proposal formulated, and there are quite a few specific items in it, where they wish to have it in the form of a project. Inasmuch as I will have to give an answer one way or another to this proposal, it would be very interesting to get the consensus of opinion here.

They are proposing that it be done at the same time as the biomedical portion. They propose that USNRDL carry out these studies in fiscal 1955 at the 2.6 investigation or man year level. The estimated cost of this will be some astronomical figure of $42,000. Since it is envisioned the programoutlined may continue over a period of several years, it is suggested that while the laboratory will probably be supported by BuMed and/or BuShips, it may be

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desirable to seek funds from USAEC to finance collective samples in the field. As indicated above, there is an early schedule to begin followup medical studies with which the proposed project should be coordinated. It is therefore requested that subject proposal be reviewed, and if it is acceptable, that approval be given at an early date so that prosecution of the program can be effectively carried out.

Incidentally, their details are of the opinion that surveys will be required at six months intervals.

DR. BUGHER: Yes. I think the first year it would probably be ordinarily at least in part. I think six months intervals seem reasonable for the first couple of years anyway.

CDR. ETTER: After having read that proposal that Captain Yarbrought has just briefed, I would like to suggest that the action by BuMed be that it be forwarded and readdressed other than it is now to the AEC for their comments since I think certainly your group should be coordinating group for all studies of any type which are going to be done under this long term program. I don't think that any individual laboratory or activity should take too much unilateral planning here, except to get things moving. All these proposals should be coordinated through your office.

DR. BUGHER: I think it would be presumed that in a program of this kind as suggested in NRDL the resources of the various other agencies and services would be also available. I mentioned the marine biological field. That is an area where ONR has a very considerable interest. This marine biological station at Eniwetok has developed, and while it is AEC financed, ONR has a very real interest in it, and has given quite a boost in the way of transport and sponsoring of conferences in this general area, and in various other ways.

So we do have joint interests. We inevitably bring in the Applied Fisheries Laboratory at Seattle, which is an AEC setup. We bring in the Hanford interests in the fresh water biology somewhat -- not so much, of course -- but they are a closely related group of people who work there. We have a program on tuna fish biology and fission product uptake at Cocoanut Island at Hawaii under the University of Hawaii. The fish program ties in likewise with the Fish and Wildlife service, Dr. Setty, stationed at Honolulu, and the Fish and Wildlife Service here in Washington.

Furthermore, we have a program on Atlantic Ocean tuna fish at Beaufort, North Carolina, and cooperative work with the Scripps Institution which likewise begins to tie in with some naval interests. All of these things are

going on and they have to all come together, which means that various persons are interested in coming into the environment of the islands for various reasons and various groups need specimen material.

The particular group immediately responsible for conducting the surveys and maintaining the records has to meet the requirements of all the other associated people, and likewise consult with them on program needs and specimen needs and techniques. I think as mentioned here it does not stand alone as you might say, a free floating program, but rather one that operates closely with a number of other programs and there is no sharp point at which the interests end for one and begin for the other.

I think the NRDL people are quite aware of that and probably would make it then more interesting and of more advantage to them. We would presume that the coordinating side of this would continue here because we already are deeply involved in the whole problem of marine biology in the Pacific area.

I have not mentioned a program of sampling of corals over the entire Pacific area, which is in motion now, with the objective of doing quantitative strontium 90 analysis to see if we can in that way get an integrated sample of ocean currents by static sampling. I don't know whether it will work out or not, but we want to give it a try.

CDR. CRONKITE: Dr. Bugher, I would like to make a general proposal in view of all this information that has come to light here, that there be a clearcut separation of these long term problems into two facets: One a medical and one a biological and physical. It seems from the medical standpoint people that are involved are willing and will continue to carry the ball on that. They would not be the right ones to carry on the sample collection and any planning for the work to be done in the field for the continuing biological-physical work. I think we are in a position to assure that the medical side is done and not in a position to be of much assistance or assure that the biological and physical work will be done.

DR. BUGHER: You think you can keep them sharply separated, particularly after we put the Rongelap people back on their atoll?

CDR. CRONKITE: Perhaps at a later date it might be logistically practical to merge the two. At the present time one concern is transportation and time involved for doing these various things, and that would involve an inordinate period of time for the medical group for the sample collection for the other people.

DR. BOND: It is quite a distance between the two locations and the logistics problem is great. The type of equipment and personnel required are entirely different.

DR. DUNHAM: I think the wishes of CincPac should be considered here, too, because it might be simpler for them to treat such an expedition as a unit, even if one unit went one place, and another another place, and were separate from theirs on down. I don't think they should be completely isolated activities at this point.

CDR. CRONKITE: I didn't express myself well. If the group doing the medical work are also to do the sampling, you run into the problem of air transportation out there, which is very limited. The regulations under which they operate of not being airborne before it is light and to leave the lagoons at a time so they can be back before sundown, which gives you about three to four hours work in an area, so there will have to be repeated trips. I can visualize and I don't think it is unrealistic, say a month at Majuro with all the complications so that the same people would be out there for two months, if they are both doing the same thing. I think it is a terrific slice into trying to carry on work at home, too. Whereas if the group went out to do the sample collection, perhaps at the same time as the ones doing the medical study, everybody could get their work done within a period of three to four weeks.

CDR. ETTER: I don't think there have been any proposals for the same group to do both things. This is not NRDL's proposal at the present time. They propose an

additional group above and beyond to be separate individuals. That is the way it now stands.

(Brief recess.)

DR. BUGHER: We will start again. In view of the recommendations as to separating the environmental studies from the medical studies, we agreed at this end to have considered Item 7 to have been disposed of, and we will make arrangements for environmental surveys and sample collections as a separate program from the medical program, but the actual timing will depend appredably on the convenience of the Commander of Quadjalein. Particularly after the Rongelap people are back home, it might be more convenient for his point to take the whole thing up and dump them for a couple of weeks or a month and come back and pick them up again, and get rid of both groups at the same time.

Would that be agreeable all the way around? We are very favorably disposed towards the NRDL group conducting the actual survey and sample collection. We can work that out here with Captain Yarbrough and the others who are available as to what specific form that would take.

I think perhaps that is enough time, then, for that subject, unless somebody would like to add to it.

MR. HARLEY: Dr. Bugher, I have a question. Will someone like Lyle Alexander be brought into this work for assistance?

DR. BUGHER: Yes, that is where one of our functions come in. Alexander in particular is the soil expert for the Department of Agriculture, and who is extremely familiar with all the problems right from, you might say, a tour out in Nevada, on to the soil collections in Pakistan. So this would be one other facet of his interest. We would certainly want him to come into the picture.

Actually the Commissioner of the Trust Territory has in mind a small agricultural experiment station on one of the nearby atolls, perhaps somewhat different than the usual agricultural experimental station, but we get involved in that, too, in some respects. We are supporting it.

That will be the function of this office, that is, to make sure that all the elements necessary to a complete survey be provided, and that the necessary samples are arranged for, and that they are adequately documented.

We will of course presume that the interests will go considerably beyond what the NRDL might itself regard as its legitimate and proper interests, Such matters as the ionization of soil constitutents is important to us. It might not be important to NRDL. They may be interested in radioactive components only.

We are very much tangled up with the problems of calcium movements in coral atoll and the crystaline forms in which the calcium salts are found. The equilibrium

reactions which enter into the growth of coral by which some of the calcium comes from sea water, and apparently a certain amount is recycled from old coral deposits, As far as I know, we do not have a measure of that. We think we can get it now from some of these environmental studies, that is, a quantitative estimate of the various equilibria that come into the growth of coral, how much new caldum from sea water passes by and how much is previously established calcium.

All things of that sort which are important to marine biologists might not be important to the NRDL program. All of these things have to be put together.

I think that is generally agreeable from your point of view, Capt. Yarbrough?

CAPT. YARBROUGH: Yes, sir.

DR. BUGHER: So at this point, let us go back to item 6 with regard to comments relative to specific studies. I read a list of eight broad items that we have suggested to Admiral Pugh, as the sort of thing to visualize in a continuing investigation. I should not think, for example, that item 1 needs any further comment.

CDR. ETTER: That is right.

DR. BUGHER: That is standard practice in a thorough medical setup.

On No. 2, the hematological studies, have you any further comments to make?

CDR. CRONKITE: I have one comment on that. The regular routine stuff is that you can't do a lot of bone marrow aspirations on your people. You soon have lost your rapport unless there is a good reason to do it. I think they have to be done but I will be frank I do not know when they ought to be done, and when would be the best time to start doing it. I would like very much the advice of somebody who studied the changes in Japan, such as Valentine and Lawrence and Maloney, to get the appropriate time when to first do it.

DR. BUGHER: The Japanes have done bone marrow biopsies on their fishermen. I have seen the sections of some of those, and they are quite interesting. The bone marrows apparently are continuing to show a persistent depression even though the blood counts themselves were moving upward. That was a few weeks back. In that case the bone marrow studies done early are in themselves quite interesting.

CDR. CRONKITE: Could that material be made available?

DR. BUGHER: I can't promise what we can get available from those people. They say yes, with the greatest pleasure. We don't have it, but we will try to get the material. Part of it is an element of barter, too. We trade something that we have for something we want from them.

CDR. CRONKITE: We did not do bone marrow aspirations

out there. We gave that serious thought and decided not to do it in view of considerations. There were so many children present, and it is a rather unpleasant procedure.

DR. BUGHER: Have you thought much of the possibility of tagging procedures to measure red blood cell formation rates?

CDR. CRONKITE: We thought about it.

DR. BOND: We thought about it, and thought it could be done, but wondered what the value of the program would be and whether we were justified in doing it.

CDR. CRONKITE: The problem of a questionable nutritional status and various things that can interfere with the iron uptake that would be unconnected with the exposure to radiation and the difficulty to get a truly unbiased random sample of normals for comparison. I don't know whether it ought to be done or not.

DR. BUGHER: Have you any comment?

DR. DUNHAM: No, I think as far as the nutritional status is concerned, when you left them it was not bad. I don't know what has happened since when they are over on this other island. Are they still going to eat in the style in which they were eatingat Quadjalein?

CDR. CRONKITE: I hope not. They will be so obese that we will never get any blood out of them.

DR. DUNHAM: I think if it is not controlled, it

will not be meaningful. If it is controlled, it is more significant than bone marrow biopsies as far as the red cell cycle is concerned.

DR. BUGHER: Are there any other comments in regard to the hematological studies, and things that should be done? If not, then the dermatological side. The skin studies mentioned included biopsies if the latter are indicated. That is an evasive case.

CDR. CONARD: There are certain cases that have more severe lesions I think should have further biopsies, particularly the ones we previously biopsied. It would be interesting to follow them and see what later biopsies show.

DR. BUGHER: You can probably find the old lesion by the scar of the first biopsy.

CDR. CONARD: We know exactly where it is. We have the color pictures which we intend to take back further, and take further color pictures.

DR. BUGHER: So you think in terms of objective photographic record with time here combined with some histological work. I think probably the histology would be the important thing here, even though there is no grossly visible change.

CDR. CONARD: Yes, sir.

DR. BUGHER: Are there other comments?

If not, what is your feeling about the ocular

studies? They are mentioned here because the lens changes in Japan were not really fully anticipated when the original plan there was set up. It was only after the lens changes from the cyclotron workers became more important that it was realized that there might be something in Japan, and Dr. Cogan conducted such a survey and found in fact that was the case.

CDR. CRONKITE: Col. Lowry made the initial survey. I do not know him personally, but he presumably is a highly qualified Army opthalmologist. I imagine for the initial survey that would be entirely satisfactory. The problem comes up, when should it be repeated, and who should repeat it. There are no qualified opthalmologists in the group out there originally. At one time Dr. Sinsky was mentioned as the desirable individual to do it. Is he available now?

DR. BUGHER: He can be obtained, I am sure. Col. Lowry did the first survey. One of the problems here is not only a technical one of minute examination, but also the maintenance of a continuing record which somebody else can look at and interpret. Sinsky did it in Japan finally by getting an artist to help him, and he painted pictures of what he saw. Those pictures are over at the Armed Forces, and they make a very fine record. This is probably what we can do in this case.

DR. DUNHAM: We can use cameras. There are cameras available for taking lenses that were developed in

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the National Research Council Cataract Committee. There is one floating around in this country now, and I am sure it could be borrowed any time resurvey is indicated.

CDR. CRONKITE: The original examination was an opthalmatopic examination.

CDR. ETTER: Sinsky contends that it is only through a slit lamp that you can pick up the early changes.

DR. BUGHER: There is no question.

MR. HARRIS: Since this interpretation is so important here, might it be reasonable that on a succeeding examination if Sinsky did it, that Lowy and Sinsky go together and do it, so that he would have Lowry's background from a previous examination? Perhaps this would not mean a necessary resurvey immediately. In other words, you could hold off a year or so.

DR. BUGHER: Yes, that makes very good sense to me.

CDR. CRONKITE: The general consensus is to delay the further opthalmoscopic survey for a few months.

DR. BUGHER: There is the point here that the base examination has been made.

DR. DUNHAM: There is some question as to whether it was made by the slit lamp or not.

CDR. CRONKITE: Lt. Sharp was there at the time and says it was.

DR. BUGHER: Before it was done, we understood it

would be done that way, so that clears that up. If that has been thoroughly done, perhaps another look in six months might be adequate, then. Is that what you had in mind about that time interval? Within the first year?

Mr. Harris. Yes, that is what I was thinking. This would be onee before their return to their home atoll if that year business holds.

DR. DUNHAM: I would urge a photographic record if possible of these observations.

MR. HARRIS: Yes.

DR. BUGHER: That opthalmologic camera is good if it is used properly. It gives a picture of the anterior portion of the lens, at least.

Now, what do you feel about the question of growth studies of children, and there is also mentioned dental development. One of the things that was mentioned that was noticed in Japan was a temporary disturbance of growth rate, but also there have been some interesting dental anomalies which appeared in young people who were irradiated while they had their primary dentition. In other words, that is babies or the first years of life. They have shown some rather interesting dental changes apparently resulting from some degree of damage to the tooth buds of the permanent dentition.

CDR. CONARD: What sort of doses were involved there?

DR. BUGHER: Of the order of prdbably 100 or 200 r, something of that sort.

DR. DUNHAM: Captain English discussed this with us yesterday, I believe. He was not expecting to find much but wished to have any teeth that came out spontaneously or pulled made available, In other words, he didn't believe that it would be worthwhile to take a special trip out there to study all the children. Wasn't that the idea?

CDR. CRONKITE: That is right. He discussed this with Commander Loce, who has had a wealth of experience in dental studies of native populations in the Pacific. He did not consider the matter of dental changes, and those that were in utero at the time. But all that were born at this level of radiation and in view of the extreme difficulty natives have in their dental growth, he did not think that it would be possible to get anything out of it. I do not recall him saying anything about the study of the babies. There are six babies involved, I believe.

DR. DUNHAM: He did wish to make radio audiographs of any material that became available.

CDR. CRONKITE: Yes, and the group at NIH are interested in that also.

CDR. CONARD: Particularly in the insidious teeth in the children.

DR. BUGHER: The administration of the trust territory has more or less a peripatetic dentist who would

be very glad to help in this sort of thing of collecting teeth in the intervals when nobody is around.

Then pregnancies and status of newborn infants. Certainly the chance of picking up much of anything significant is probably small in that. I daresay the occurrence of pregnancy and the inevitably correlated birth dates are not going to have much relationship to the time of visits of the special medical group. So the chances of being able to study these things on the part of the survey teams are not going to be too good. However, the resident physicians can carry out, I think, quite a lot of observations you might want them to make in the interval.

Now, on the quantitative studies of internally deposited radio isotopes by excretion measurements, radiography and localization, we include here autoradiography and autopsy work, I think that might merit some additional discussion. I think, Dr. Looney, some of the things you were mentioning really could be brought out here, if you care to develop them.

LT. LOONEY: Yes, sir. In the studies of radium patients that we made we found that the most consistent and probably the most valuable clinical finding was the small changes roentgenographically, and as mentioned yesterday, those were primarily the result of a formation of atypical-osseous tissue. It was found that similar changes were

present in bone following yttrium, plutonium, strontium, radium. These were similar.

We were able to pick these up from the clinical standpoint by areas of inreased density which usually occurred at the cancellous bones at the end of the long bones. We were able to correlate this wi th the amount of retained radium, and it was a more reliable clinical indicator than any other findings that we found.

The other important thing is that in the tumors that developed in these people, most all of them developed at the ends of the long bones and a roentgenographic study of the long bones gives a base line for any future changes, and also a study for any changes in symptoms which might be pre-cancerous.

All these radium patients follow a rather fixed pattern, namely, that about 15 years after the deposition of the radio element symptoms occurred. Then later symptoms will occur and wre consistent at the point where the chamber develops. This is accompanied by areas of density. This is a very important aspect as far as the long term study, as far as the internal emitters are concerned.

I know you are interested in radiation as far as x-rays are concerned. I looked up the data from Brookhaven on a 40 to 80 KV machine, and found that taking for instance, a survey of atypical fibula, this would give you a .12 roentgens to this part. Taking a book on burns, the tibia,

the exterminators were concerned 13 per cent of the body area. This is figures, but it will give you this idea of .012 roentgens. As far as the roentgenographic changes are concerned, we found with the exception of only 10 or 20 per cent of these people that if changes occurred in the long bones, in the radius of the tibia or fibula, it was almost always bilaterally. So taking one long bone one year and another this would minimize the radiation to this and give us basically clinical data for changes which may occur three, five, ten or fifteen years hence.

I have a chart which I don't know whethe r you can see it or not. I just happen to have a chart. This gives you some idea of the distribution of the tumors that developed in these radium patients. You will notice that they are all almost at the ends of the long bones or near the ends.

The roentgenographic changes occur either at the ends or the middle, so by taking an x-ray of the joint and probably two thirds of the tibia, this would give us a very excellent base line study for our future changes which might develop.

This would be very important for any autopsy material or any biopsy material that we might get when these people were having operations that could correlate the roentgenographic, radiographic and histopathologic changes.

That is another point I would like to bring up of the technique which we used at Argonne of taking audio radiographs, and from this to use as a guid e for the detailed ones. We were able to take whole sections of bone and from this take x-rays and also audio radiographs, and then in the critical areas, take biopsy material from this to study detailed audio radiographs in which we could simultaneously study radio element deposition and histopathological changes.

This was a simple and satisfactory method of study. Since strontium is probably your biggest trouble from long life, I would suspect that this would get a similar distribution, and also a similar change. I think another important thing that this may answer is this: We don't know from the radium study at what time these changes develop. We have an idea it is five or ten years following deposition, but with the shotter half lived material, the calcium that was gotten in here, these changes may develop earlier, and the malignancies might develop earlier. But this would be one step ahead of the game if we had this base line study. I think it is a very important thing to incorporate in a study of this type.

DR. BOND: May I ask how many individuals would you wish to x-ray? Would this be the entire group or children specifically?

LT. LOONEY: I think you might stagger this and maybe take ten or fifteen, depending on the facilities and the situation. That would be my suggestion, to start out with ten or fifteen, and see how we came out, and to try to get a control set up, and maybe the following year get ten or fifteen more. This survey should probably be, one, one, three, five, ten , fifteen and twenty years, with any repeat x-rays if patients should develop symptoms or anything suggestive of malignancy, and take a biopsy as well as an x-ray.

The base line is one, three, five, ten and fifteen, or something like that. I don't think they need to be made as often as the studies if you are going to make them annually. I would say that probably an x-ray of the tibia and fibula, the radius and ulna, and the x-ray of the pelvis. We know from the radium studies thatthese are the places that would most likely have these changes, if there are any changes.

We could omit the pelvis. This gives you one roentgen of irradiation to an area. I don't know what the approximate radiation in terms of whole body radiation this would be. The external radiation is very small.

DR. DUDLEY: I wonder if it has been discussed here what the level dose is from the internal emitters from quantitative terms? I put together the two microcuries of

strontium and that gives an infinitive dose of less than one breadth.

DR. BUGHER: It has been discussed in the sense of extremely small inasmuch as the total activity is probably much less than tolerance when you think of that over a long period of time. That is the catch here, I think, and if you x-ray a bone, you are going to give more of a dose probably in the course of the examination than the internally deposited isotope is going to be able to give, which makes it difficult to arrange a series of examinations to allow for that factor.

LT. LOONEY: Sir, in regard to the x-rays according to the Brookhaven group,60 to 70 per cent of the radiation will be expended in the first three centimeters, and 8 per cent at 8. So the radiation of the bone would be very small.

Another thing I would like to emphasize here is the marked variation that we found in the radium patients when estimating body burden from urinary excretion. We found a factor of eight. This was long range. I would certain expect a much greater range from the estimation of the total body burden from urinary excretion.

One of the things is that we can work out the most practical and sensible means of handling situations of this sort in the future by this method. Going over this wrk with Dr. Cohn, the likelihood of malignancies developing

here are small. But there are many other factors that as a clinician I would not be satisfied with the estimates and the assumptions made here. I think the final proof is the evaluation of these people, and knowing what the situation is after ten or fifteen years.

DR. BUGHER: Gene, do you have any thoughts as to how to carry out this sort of program? For example, is Quadjalein itself equipped with these facilities?

CDR. CONARD: You were speaking of later studies on Rongelap?

DR. BUGHER: I was thinking of that.

CDR. CRONKITE: They have as good equipment at Quadjalein as we have here. It is a first class naval dispensary.

DR. BUGHER: So it would be a matter of bringing a few people over.

CDR. CRONKITE: Yes.

DR. BOND: It could be done right at Majuro.

DR. BUGHER: Down there it is a question of a launch ride. It is only about five miles from Majuro Island. It is a natural excursion.

CDR. CONARD: Is it accessible by air?

DR. BUGHER: No, it is a separate island.

MR. HARRIS: It would be nice to put some of these people under a human counter.

DR. BUGHER: Yes.

LT. LOONEY: We considered taking a scintillation counter out there. I didn't bring this up. I do think your suggestion is a very excellent one. If the logistics permit, it should be done.

MR. HARRIS: If you did such a thing as this, the time to do it would be probably during the time of testing out there when they were hauling out all sorts of heavy equipment. The human counter weighs approximately ten tons. That is the Los Alamos human counter. You can split this up a little bit by draining, but that is also quite a load.

DR. BUGHER: There are some drawbacks to it.

MR. HARRIS: However, the arm counter, something like that, which weighs 3,000 pounds, might be feasible to do in this case.

LT. LOONEY: We have a very sensitive scintillation counter we have been using on thorium patients which would certainly be adaptable to the situation if this is something that should be done. I think that body surface trying to correlate external measurements with any roentgenographic changes or radiochemical findings in the skeleton would be interesting and probably helpful, and give us full information to try to correlate all this as to the best practical means of evaluating people in the future.

DR. BUGHER: You may get an answer there on trying to distinguish between background and the individual counts. Some of the Air Weather Service people who were on Rongerik would be fine. In other words, they have much lower levels so if one could recognize anything on them, then you would know it would pay off to haul the equipment out.

MR. HARRIS: It had been calculated. I think as far as the Air Weather Service personnel, it was calculated on the basis of urinary excretion. You might be able to see this beyond the natural K-40 background. This would be entirely in the nature of an experiment if it was done, and not come into routine medical situations as far as I can see.

MR. HARLEY: Where could you make the measurements that the background from the fallout would not be too much for you ?

CDR. CONARD: I don't think Majuro got any fallout.

MR. HARRIS: Our background at Los Alamos is higher than it is at Quadjalein.

MR. HARLEY: Majuro got a pretty good sock.

MR. HARRIS: You could not carry any such thing farther than Quadjalein. This is designed to take care of high backgrounds. Ten tons, it has to be.

DR. BUGHER: The question of autopsy is again going to be something which will have to be arranged with the

local medical personnel. I do not know how difficult it is to get autopsies of the Marshallese.

CDR. CRONKITE: They have done autopsies at Majuro. They have to be done immediately. Their custom is to bury people within six hours after death.

DR. BUGHER: So almost certainly you are going to have to reach an understanding with the medical officer to do an autopsy and collect the material you wish, and preserve it in the manner which ycu desire tohave it preserved for shipment to you. The chances of special teams ever having an opportunity to do an autopsy are not too good.

CDR. CRONKITE: These were made originally by the commander, and after the decision to move to Majuro was made. It is not clear to me whether anybody talked to Dr. Kirk at Majuro, whether they are aware of the necessity for doing autopsies.

DR. BUGHER: I think they are vaguely aware of it, but as far as specific needs are concerned, I am sure that has not been communicated. That is one of the things that could be done. I think they are willing to do anything that is asked of them, if they are able to do it, and will follow the suggestions quite enthusiastically. I don't know of any specific request having been passed to them, other than that they should give the general medical care to people that under the other plan would have been forthcoming from

the medical officer at Quadjalein. I am sure they will do these other things, too, if you will just outline to them what is desired.

CDR. CRONKITE: Would that be done by the first team that goes out there, or should this be carried down through the trust territory, so that everybody is aware of the need?

DR. BUGHER: I think it probably might be a good thing to prepare that in a set of written instructions of things that are needed, and we transmit that through the trust territory administrator so that in case somebody should die, before the team gets out there, that the opportunity to get materials would not have been lost.

Are there other comments along the lines of these topics?

CDR. CONARD: In addition to that, I think that it would be nice to have good rapport there on the observation of skin and any other changes.

DR. BUGHER: Yes, I think we assume all through here that everything that is done is done by the cooperation with the trust territory people, just as the movement of the people, for example, has been done as far as form is concerned, under the general supervision of the trust territory officer. He is the one that the people themselves look to, not only during times of special studies, but in the interim. In that sense, we are simply backing up their

people in carrying out these various things. So I think you will find to whatever extent is useful trust territory administrative people are available at all times.

Are there any other comments or questions here? If not, we have a subsidiary topic called institutional and individual responsibilities. Have we covered that sufficiently or do you wish more comment on that?

CDR. CRONKITE: There was a statement earlier when you were out, Dr. Bugher, of the desire of the Army and Air Force to have people participate in this followup. If these people could be designated so that they could be split up between the team that Dr. Bond takes and the one that I take, it would be most helpful in our planning.

DR. BUGHER: Yes.

CDR. CRONKITE: I would like to also extent it not only to the matter of officers, but to enlisted technical personnel, so that no one laboratory gets hit too badly at any one time.

DR. BUGHER: I think Col. Browning would undoubtedly keep various groups in the picture. For example, Col. Brennan and other groups interested in these lines. There are various others, some of whom have been scattered a bit, but I think could be recovered on special assignment on things of this sort. So that in a way what we get concerned with are individuals who may have special competence and interest

here, and that involves any institution with which they may be associated with at the time. Fundamentally it is the same old problem of people who are competent and interested, wherever they may be, if they could be made available.

The question on Uterik. The environmental studies will have to be done at all three atolls. These teams naturally should pay some attention to the Urerik people, but I presume the proportion of attention will be pretty much along the line of proportion that they got in the beginning which was not very much. In other words, just an overall surveillance to make sure nothing odd is developing.

DR. DUNHAM: Should they be reviewed this year at all, that is, the Uterik people?

CDR. CRONKITE: Practically speaking, I think not. From an academic standpoint, probably yes. My general thinking along these lines was that since they had perhaps a tenth of the exposure that the Rongelap people did, if nothing is showing up in the Rongelap people, there is relatively little reason for even academic purposes to study the Uterik people. If something does occur in the Rongelaps, then we should take a look for both straight medical care and academic reasons at the Uterik people.

DR. BUGHER: They obviously should be visited by the special team at least as a social call, if nothing else.

DR. DUNNING: What do you wear on an occasion like

that?

CDR. CRONKITE: Mr. Eisenbud expressed an interest before I left Quadjalein in having urine samples from the Uterik people. When I do not know, or what intervals he desires.

MR. HARLEY: Are they back now?

DR. BUGHER: They are back on their own atoll at the present time.

MR. HARLEY: We would like to get a set of samples before this project gets going, if we can. We were thinking of dealing through the trust territory people perhaps to get a sample certainly in August, and then perhaps another sample when you get out there on the study, if that is possible. I think the urine sampling can be done without any of our personnel there.

DR. BUGHER: They are accustomed to it now.

MR. HARLEY: Yes.

DR. BUGHER: At least the Rongelap people are. So I think that is probably the balance of the thing.

Administrative and logistic support we have already discussed. Those are administrative problems which we have to solve among the group here, really. I think that has been already pretty well clarified.

Now, the transportation, air and survace. Air is MATS, I presume, and surface transportation again comes to

Commander, Naval Station, Quadjalein, I believe, for probably all of it, unless this 10 or 11 ton piece of equipment, and things of that sort, have to go out. That would be a surface transportation problem.

MR. HARRIS: I was wondering if you had at all considered the possibility of getting some of your logistic support from the Eniwetok field office, and their prime contractor.

DR. BUGHER: Yes, we have. We may very well wind up with a launch over at Rongelap for environmental surveys and continuing studies of various kinds. But at the present moment, we have not approached the field office with any such request. I think in a way we have to wait until the need is a little more clear than it is now before we make a specific request.

MR. HARRIS: I was particularly thinking from the point of view of the people who are doing the environmental studies if they could take an LCU or something of that sort from Eniwetok to Rongelap, and work off it. They could perhaps cut down their time of stay from maybe a month down to a week or something of this sort.

DR. BUGHER: Particularly between test series there is not too much difficulty in getting such equipment. The question is housing and messing here. That is something I presume the teams have to solve for themselves.

CDR. CONARD: Accomodations on Majuro are very limited. Before we ate with the Air Force weather group there. They were kind enough to feed us. But their facilities are extremely limited. I think we really should have some concrete arrangements made before we go this time.

DR. BOND: On that, NRDL says it will not be too difficult to contract with the trust territory on a setup where we can do this to have them house us and feed us, and we can provide any additional food they might require, and also provide them with additional personnel they might require for the preparation of the food. This could be accomplished very easily on our working level provided the overall situation were taken care of at the higher level.

DR. BUGHER: There will be abundant supplies of fish and cocoanuts.

CDR. CONARD: And we could take some C rations along.

DR. BUGHER: I think those matter we could very well ask the trust territory administrator to see what he can provide, and when they reachthe limit of their resources we can supplement them. It is almost as quick to build a thatched hut as it is a tent, and much more comfortable.

CDR. CONARD: The housing is not so much of a prdblem as feeding. They were unable to take care of our small group in feeding us. The housing was no particular

problem.

DR. BUGHER: That is one of those things to be worked out as you go along, I think.

CDR. CRONKITE: I think what is more of concern to Dr. Bond and me is who is going to take the administrative responsibility of seeing that all these things are arranged sufficiently far in advance so that we don't get into a bind when we arrive. It is going to take quite a lot of doing.

DR. BUGHER: In the first place, you will get into a bind when you arrive almost inevitably, that is, it always works out that way. We will undertake to clear through here a request for what you need as far as you can foresee it, and with AFSWAP, and the Navy to get all the arrangements made ahead of you.

DR. BOND: Is it permissible to correspond with the doctor on Majuro in informal correspondence?

DR. BUGHER: I think it should be with copies in here so we know what has been done. When it comes to something that is specifically asked of the trust territory people, then we should go through a formal route. I mean by trust territory administration. If you want food, services, and things of that sort, then we should put that to the Commissioner himself. It almost always works out in the end that it is the Navy that does it.

CAPT. YARBROUGH: I just wonder in that respect if perhaps some advance information to CincPac could not

be furnished? They are the ones that can effect detailed arrangements for messing. They will ask the Air Force facilities, do you have sufficient rations and personnel to feed so many people. If you don't, Commander, Quadjalein will probably be directed fo furnish the same.

DR. BUGHER: That is right. Our procedure has been all the time that CincPac is always informed of whatever is going on. We have operated right along on that basis.

CAPT. YARBROUGH: But also as usually happens, the quality of such depends entirely on the diligence of the individual who is addressed to get these things dom. I was a little disappointed that Clark is leaving, because the suceeeding man won't be familiar with the visiting group. Admiral Clark may have briefed his successor. I think perhaps it might help if in addition to what CincPac directs that personal correspondence to Commander, Quadjalein might help out a great deal.

DR. BUGHER: Yes. Does that help? We will attempt, then, to make these arrangements through the various channels. In other words, we can act here as a clearing house. That means we will work with AFSWAP , with BuMed, with CincPac, with the Commissioner of the Trust Territory, whatever channel is appropriate for that particular problem.

The AEC field office gets called on, too, for help here. If we have to hade additional housing, for example,

for the venture to Rongelap, we will have it put up. Those things I believe we can take care of as need arises and really should not be any source of worry to the group here.

DR. BOND: Will it be appropriate for us to supply you with a list of requirements, dates, and so forth?

DR. BUGHER: Yes, I think so.

DR. BOND: And what is required you will handle from that point?

DR. BUGHER: Yes, we will put it in the various places where it needs to be.

CAPT. YARBROUGH: The first figure you need is how many people are going, and how long you are going to stay.

DR. BOND: May we discuss that for just a moment? Is there any opinion on the number of people that should go. I think we started that discussion earlier, and got sidetracked into something else. The facilities on the island would accomodate not more than 15 people without considerable difficulty. I personally feel we can probably get along with less than 15. Does anyone have any specific opinion as to the number that should go?

DR. BUGHER: I hope it would be less than 15. The more compact group, the more smoothly operating it would be, and less of a strain on whatever locality it comes to rest in. Every time you add a man, you add a bed, you add a chair and table, and piece of roof, and the capital outlay of course

becomes progressively greater.

DR. BOND: The point I was thinking of is whether to take more and stay a shorter time, or take less and stay a longer time.

DR. BUGHER: My point of view is that the fewer people, the longer time is the more productive sort of thing.

DR. BOND: Then we will compromise on it.

CAPT. YARBROUGH: What will you settle on, say 12, 13

DR. BOND: We can leave it at 10 or 12, and we can supply in the next few days the specific names. As a matter of fact, we can do it pretty well right now.

CAPT. YARBROUGH: I think if you can quote something like not more than 12 people --

DR. BOND: I think thatis a reasonable figure, not more than 12 people.

DR. BUGHER: Is there any other aspect you would like to bring up in that connection?

CDR. CONARD: Do you think these arrangements could be completed by around the middle of August or some where thereabouts?

DR. BUGHER: I don't see why not. We are going to have a double problem a bit. That is, one for the medical people, another one for the environmental people, and the latter may need either a landing craft for a week or ten days, or it might need that plus a temporary camp. We will see where

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we come out there.

That seems to cover the Item C, integration of visits between Marshallese, trust territory, DOD, AEC, and CincPac. All visits which involve Marshall Islands people and area, that is, the territory outside of Eniwetok and Bikini should be cleared through the High Commissioner of the Trust Territory. It will be up to him then to inform his people as to when and who is coming, and what is expected of his people. He will do that with a great deal of enthusiasm, because he feels very strongly that the work of this group has fundamentally greatly benefited the conduct of the administration of the whole trust territory. It has put the United States Government in a firm position of a humanitarian interest in people, and in their welfare, which is worth more than any amount of words. So we can anticipate no reluctance on the part of the trust territory administration to advance these studies in any way that they possibly can.

All visits likewise informed to CincPac. In other words, these plans with the trust territory likewise should be communicated to CincPac. The various other groups here which are concerned, also, that is, AFSWAP, in general, is the channel for the information of the services as to what is going on. Entry to Bikini and Eniwetok will be cleared also from here through Santa Fe Operations Office, and the

Eniwetok field station.

Is there any other thing that you can think of in regard to routine information of channels?

DR. DUNHAM: No.

DR. BUGHER: Are there any other comments on those things? What do you think, Col. Browning, in regard to these information channels? Have we missed anything that we should do or have we suggested doing anything we should not do?

COL. BROWNING: No, sir, I don't know of anything. I would like you to put in a plug here for a very firm commitment through trust as to the housing and messing because it takes a long time to get extra food out there. If you send 13 people, it is 40 miles a day, and it is not quite that easy for them to provide it. This ought to be well established in advance. If you leave it on the local level, the local officers, whether naval or whatnot, are very hesitant on stepping on the trust toes, and rightly so. They are not in a position to do many of the things that they might do on a stateside base. Anything that can be done in the way of administration earlier will help to get things done in better fashion.

I would suggest in that respect, too, that a rather firm agenda be supplied to the trust so that they will know who will be where under what circumstances, and leave us enough slack in it so that there can be made local adjustments.

But again this ought to be done well in advance.

DR. BUGHER: Yes, there are two ways of laying down supplies at Majuro. One is through Quadjalein Naval Station and another way is essentially privately through the Island Trading Company, which is due to fade out this fall. But it still exists and is looking for business. I think the point you make would hold equally true there. Unless there is planning well ahead, it will be snafu at that end. So these requirements will have to be drawn up rather rapidlyand everybody should be informed about them completely.

COL. BROWNING: I have a rather large map of the Majuro area, and it looks as though that particular island is about two nautical miles right across the lagoon to Church. I meant to ask you this before. Is that settlement down on the end of the lagoon?

CDR. CONARD: The island with the church on it?

COL. BROWNING: Yes. The map shows a church, a couple of radio towers.

CDR. CONARD: That is at the opposite end of the lagoon, as I remember. The main island is the extreme southern end of the lagooon. There are a series of islands there that are connected by causeways. I was asking Dr. Bugher whether this island was accessible by road, but apparently it is a separate island.

DR. BUGHER: My understanding is that it is not.

From a landing on Majuro to the launch landing to this settlement would be five miles. But that is hearsay as far as I am concerned.

CDR. CONARD: I don't remember this particular island.

DR. BUGHER: This was not inhabited previously.

COL. BROWNING: The chances are that they have some sort of water transportation set up at the present time.

DR. BUGHER: Yes, they have two launches there, I believe. The trust people seem to have no worry about that transportation link there. When we get back to the Rongelap-Uterik atolls, then they are in trouble, and they really can't move without the Navy moving them.

Are there other points that occur to you? Does that seem to be adequately covered?

CAPT. KELLUM: Yes, it seems to be well covered.

DR. BUGHER: The project officer and reports. Whoever is the project officer is to be responsible for the compilation of the report of that visit. Is that your thinking, Gene?

CDR. CRONKITE: The main thing I would like to clarify is what is the report channel? To whom does one report?

DR. BUGHER: To the Division of Biology & Medicine.

CDR. CRONKITE: It is not a task force report that goes through the WT channel?

DR. BUGHER: The task force has terminated, or will

be pretty soon now. So it is a continuing study outside of the original task force responsibility. These reports, however, will go into AFSWAP. They will go to the then existing task force so that all the various people who hve and will be concerned with operations there will have this material available to them. But that would be handled in the normal way in which AFSWAP takes care of these things.

The report itself would come here. We would arrange duplication at Oak Ridge through the customary procedure. The main job would be to get the report produced. After that, the reproduction is not difficult. For example, when Vic has this next special trip, then the report will come via NRDL into -- whatis the channel there? To you?

CAPT. YARBROUGH: Via BuMed to BuShips.

DR. BUGHER: And then here to us. If it is from MNRI, it will come from your office.

CAPT. KELLUM: Yes.

DR. BUGHER: Depending on where the team is based. Any service that we can render in helping the thing that is something we will be glad to do, whatever it may be. In that sort of function, we are simply trying to help and not as a matter of authority and command, but naturally expect everybody to give a lift whenever you can. We come to the natural adjournment time for luncheon, I take it.

After luncheon, I think we should discuss a little more some of the things which we should do in the course of

the environmental surveys, and go on to some of the other special things which are mentioned here, before we adjourn at the end of the afternoon. So in that case, we are only three minutes behind.

(Thereupon at 12:33 p.m., a recess was taken until 1:30 p.m., the same day.)

AFTERNOON SESSION 1:30 P.M.

DR. BUGHER: Let us take up some of these considerations of the survey which will aid the group that eventually has to do it.

Attention is called to accomodation for air and ground surveys, the distribution material in domestic animals and natural foodstuffs, and a long term metabolic study of the fission products in those animals and in the flora, which would of course involve radio chemical studies.

I think perhaps John Harley might have some thoughts with regard to the air and ground surveys, and whether an air survey would be helpful at the present time or next year or whether it should be entirely a groun d operation on these atolls which would actually be all three atolls, Rongelap, Rongerik and Uterik.

MR. HARLEY: Our feeling on it has been that at least the preliminary survey should be by air, because so

far at least under a given set of circumstances, you should be able to get good correlation, as we do.

Gordon might not always agree, but when we looked at such places as our estimate of dose, the Rongelap, which is of the order of 200 r and so on, I don't think that you are far off. Therefore, as the first step in a survey, I think it would be done by air. I don't think it should be limited completely to these particular atolls. I don't know how much we can do about it now, but on the last shot, for example, we found rather intensive fractionation of material, when the separation between spots was only a little over 100 miles.

I think the decay rate was more than doubled at the farther out location. We would like to get more data of that sort for that purpose. After preliminary aerial survey, you might be able to decide where to take your ground samples.

DR. BUGHER: Would people in the New York laboratory be willing to do it?

MR. HARLEY: We would be very willing to loan out the instruments.

DR. BUGHER: Yes, I know that. There is a question of uniformity of procedure here. It took quite a lot of beat-up before the cooperation which was necessary between the flight crew and the radiological mapping people had been established.

MR. HARLEY: I think you were speaking of the work

on the last couple of shots, were you not?

DR. BUGHER: I was thinking of the flight surveys out of Quadjalein, which were carried on during Castle. It took quite a background of experience which was built up during Ivy, which was drawn on to guide that.

Again, it would be in an aircraft out from Quadjalein. I suppose a P-2-V. One would like to get one of the crews that had previously done this sort of thing.

CDR. CRONKITE: I think the P-2-V squadron was relieved after the operation. It was only there for security purposes.

DR. BUGHER: It was to leave in toto?

CDR. CRONKITE: Yes. Admiral Clark mentioned that one time. They were only there for the security of the group.

DR. BUGHER: Not even a Piper Cub left, I suppose.

MR. HARLEY: I have no doubt that somebody would go out, but as far as supplying a full crew, I think that they would prefer not to if they could. That is, to send out a man who is a combination maintenance man and so on, if anything went wrong.

DR. DUNNING: Dr. Bugher, I am wondering at the moment what purpose these aerial surveys might serve. We are pretty well agreed that we are going to make ground surveys at the three atolls of immediate interest. Possibly we might want to make air surveys of some of the more distant

atolls. Again I don't know for what purpose.

MR. HARLEY: It was under the impression that you would be making ground surveys only at three islands, and not three complete atolls.

DR. BUGHER: Three atolls.

MR. HARLEY: Three complete atolls.

DR. BUGHER: Yes. I think the ground survey would have to make the whole survey circuit of each of the atolls here. You see, a monitor getting off one end and plotting at the other end of the island. I guess he would be picked up by launch. But I can see miles and miles of footwork in prospect for somebody.

DR. DUNNING: I think that was my point, that since we are going to send in ground surveys into the areas of major interest, we have a pretty good notion what the levels are going to be in the more distant areas, and that is rather low.

DR. BUGHER: I was going to ask that question. What do you expect?

DR. DUNNING: I don't have the data here with me, but I can dig it up in a hurry. They will be low certainly in terms of any health hazard. They will be so low in fact that I begin to wonder whether the accuracy of the air survey is worth the effort. In other words, why do you want to go down and make an aerial survey on these distant atolls?

MR. HARLEY: I will ask the question in reverse. Why do you want to make a ground survey on the unoccupied islands of these atolls?

DR. DUNNING: If we wanted to make a survey, we would be dealing with relatively small numbers. I mean the radiation levels would be relatively low.

DR. BUGHER: A few MR per island.

DR. DUNNING: Therefore, I am wondering how much confidence we can place in an aerial survey. To repeat your question, why go in on the ground? I didn't say to go in. I think this is perhaps a point for discussion, but if you want in with a ground survey ostensibly it would be to measure the low levels for scientific reasons and not health. You are dealing with such low levels of radiation, that is why I was wondering if the aerial survey would do the job.

DR. BUGHER: If one could substitute the aerial survey for footwork, let us say, in the expanse of the northern island of Rongelap, then make the ground survey as a number of spot checks, that would cut down the labor, I suppose, very materially.

DR. DUNNING: I was hoping that the ground survey teams would go into the northern islands of the Rongelap Atoll. Those are of the most interest.

DR. BUGHER: We have a pro and a con on this. Does anybody have additional comments?

MR. IMIRIE: There is one thing that comes to my mind, Dr. Bugher, and I have talked to Merril Eisenbud about it, Harley and Dr. Dunning; there has been a question from every survey group that went out as to what readings wereat various points. Of course, we all know what happened on these readings. In some cases there were uncalibrated instances and in other cases they were two feet from the ground or three feet from the ground or near the water or under a "hot". It is true that is where the people were. But in addition to that, there was an aerial survey taken which indicated a little higher r than most of the ground surveys. The aerial survey would tend to integrate the average dose on the entire island as compared to searching out hot spots and cold spots. If for nothing else, it would give an inter-comparison of one island between another island or one atoll against another atoll on an average integrated basis.

Further than that, two readings of aerial survey might prove out or disprove the centimeter which was used.

MR. HARLEY: We have data here, for example, on Rongelap taken at 32 hours, one with a T-1-B, and the other with the scintillater or from the air, and the difference between was essentially nothing. It is less than 5 per cent.

DR. DUNNING: Yes, but I was out there, and the first comparisons I made were between the air and the ground and differed by a factor as high as four. I have the raw

data here somewhere. Later, Al came up with a new correction factor. That brought the two readings in line. By two readings I mean ground and air. So I think one has to take a pretty close look at the data to see how valid these readings are. Maybe before the operation was over with, maybe he got this correction factor down a little better.

MR. HARLEY: There was a little difficulty out there at the beginning. I think it was merely a misunderstanding of Al's. The reason I am pretty sure there was a misunderstanding, even after talking with him, is that he was getting the difference using the centimeter on the ground and the air in the height conversion factors. We spent a lot of time out there after the whole crew was around in re-doing our conversion factors.

DR. DUNNING: Understand, I am not taking a strong stand against aerial surveys. They were most valuable and especially in the early times after a shot, when we were pulling our hair wondering what was going on out there. I am not sure, but that it should be done. I am just raising the points that came to us out there, and again say just what purpose are we trying to serve by these additional surveys.

MR. HARLEY: My feeling still is that you probably would pick Rongelap Island for your ground surveys and stick to the air for the others, rather than trotting all over under that hot sun. At the same time, of course, you would

be able to get your cross checks from one to the other.

CDR. CONARD: Do these surveys give us any better idea as to what the original dose was by extraplating back?

DR. BUGHER: There would not be decay, and the decay with time has become more complicated by the weathering factors which come in.

DR. DUNNING: I doubt there would be any additional data that would turn up at this stage of the game that would influence our thinking on the original estimation. We were concerned in terms of the decay constants between the six hour and the fiftieth hour, which might well be a different decay than it is now.

MR. HARLEY: Your change in total dose would be slight, no matter what the decay rate was, after the first few days, because almost all your total dose is in the first few hours anyway.

DR. BUGHER: There is somewhat of a question as to how to get the most information with the least work here. Payne, do you have a thought here?

MR. HARRIS: When I read this, I personally could not see any particular reason for doing air surveys unless it was to continue attempting to calibrate between good round readings and air readings to get a calibration factor. I question whether this is necessary to go to Rongelap in the Pacific to do this. Could not this be continued in Nevada in the next series of operations?

Other than that, as far as an adjunct to environmental studies, again I do not see where an air survey applies to this. My feeling about environmental studies is that if you are going to measure the activity in a cocoanut, you want a survey made at the point that cocoanut is picked up off the island, or close to it, and not from the air. So what surveys were made on other islands than Rongelap, if you got samples from those other islands, you would still need the ground survey there in order to correlate in your final analysis of the data. In other words, an air survey on an island which was going to be used for sampling ground I can't see as accurate at all.

MR. HARLEY: If you are going to be on the ground, you might as well take a ground survey.

MR. HARRIS: Yes. The only way you can get samples is to be on the ground, and take the survey right there.

MR. HARLEY: If you are not going to take samples, then the question is, should you take an aerial survey or should you go in and take a ground survey?

DR. BOND: Are these surveys of areas already surveyed?

MR. HARLEY: Yes.

DR. DUNNING: About the only thing you can accomplish is to tell CincPac and other people that the activity on these atolls has gone down so much. That is the

only reason I can think of at the moment, and then aerial surveys would be called for.

DR. BUGHER: Another point of view, the only area where the strontium 90 component would be enough of consequence is probably in the northern island of Rongelap. That takes pretty detailed and careful ground sampling of soil, plants, and the animals and so on.

DR. DUNNING: It might be of interest to this group that according to the calculations there is about half of microcurie of strontium 90 per square foot up there on one of the islands, and I am very anxious to see how close these theoretical figures become because this is getting up in there, shall I say.

DR. BOND: Which island is that?

DR. DUNNING: N-a-e-n I believe is the proper spelling.

MR. HARRIS: If you collect enough, you might be able to sell it for isotope uses.

DR. DUNNING: Yes.

MR. HARLEY: Or make your own batteries.

DR. BUGHER: One other aspect of the aerial survey is one reason why I asked John if somebody from the New York lab would be running it, is that if it is done at all, the calibrations have to be done very carefully and at rather exhaustively, and the whole thing carefully controlled, or

else the results are not those which would command confidence and therefore of limited use. It is not something that you just send out an instrument to somebody on Quadjalein, and tell him to stick it in a plane and fly it over the atolls and send back the record.

MR. HARLEY: I would like to add, although I don't defend the comparative reliability of instruments, that people tend to have a great deal of faith in something like a T-1-B, and drag that out and make a measurement and come back and that is the fact. Whereas, ourselves, because we recognize that they are not particularly stable, we don't have quite so much faith. I think that the same calibration business has to apply to everything that is used out there.

DR. BUGHER: That is very true.

MR. HARLEY: We made a check on energy dependence out there. Actually we had it made here at the Bureau of Standards, afterwards, as you know. There is quite a difference between the scintillation unit and, say, the T-1-B. We found actually that the T-1-B cuts off higher than people seem to think, closer to 100 kilovolts, whereas the scintillation is sensitive down close to 40.

DR. BUGHER: Yes.

MR. HARLEY: So we have to consider calibration and energy where both sets of instruments are used.

DR. BUGHER: That is correct. The standardd

calibration of the T-1-B has been with cobalt 60. I believe the two instruments calibrated that way, they do agree. When you get into the large component of soft gamma, the centimeter gives a higher reading by a factor of two or so, I thought, early in this fall. It is something that is of that order, anyway.

I think that is a subject perhaps we ought to give a little more consideration to, as to whether the airborne survey would pay in view of the fact that we want ground soil samples and plant samples from almost the circumference of these atolls, which means ground survey anyway, as well as sampling. Whether the additional effort which would be required to get a good area survey into operation would pay is the question. I think perhaps we could discuss this somewhat more later.

Does anyone have a strong feeling one way or another here? We heard the pros and cons.; I don't believe there really is much more to say. I am just estimating what is the easiest way of getting the necessary data. We do want to document all the islands by one means or another on each atoll.

Let us turn to the question of internal hazard. Domestic animals and natural foodstuffs were suggested. On Rongelap I think you could say that the domestic animals that were there are no longer there.

CDR. CRONKITE: Rongelap Island, they were not there, but there were quite a few hundred chickens on the northern island.

DR. BUGHER: To the very far north.

CDR. CRONKITE: Yes.

DR. BUGHER: Do you expect to find any surviving chickens?

CDR, CRONKITE: They were supposed to be there. Whether they are alive, nobody ever got up there. Which island did you get to?

LT. CHAPMAN: Aniola. That is about five miles north of Rongelap.

CDR. CRONKITE: You didn't get any there?

LT. CHAPMAN: There are no chickens there. They are farther north.

DR. BUGHER: The farther north you go, the less likelihood there will be of a surviving chicken. What was the estimated total dose on the far north?

DR. DUNNING: The infinity dose was 7,800 roentgens.

CDR. CRONKITE: Chickens won't survive 1,000 r.

MR. HARRIS: Are there any rats left out there?

DR. BOND: I didn't see any at Rongelap.

DR. BUGHER: I think you have to assume that all the vertebrate population in the northern islands would be gone or not there any more. Chickens and pigs will be put back on

Rongelap when people return. They have been put back on Uterik now. So the domestic animal supply is going to be practically speaking animals introduced after the enviornment has become acceptable for human habitation. Can you get much out of that or not? That depends on how much low level studies one wishes to do over a period of time. It would certainly appear to be worthwhile to have a certain number of domestic animals with the idea of sacrificing them after a time, or lease accumulating bones after a year or so from the standpoint of their uptake of fission products.

DR. BOND: Dr. Bugher, along that line, wouldn't it be as good or better to return material to the most active areas? I am not interested in determining how much the animals pick up from the environment. If we know what we gave them and how much they took up, we would have valuable information.

MR. HARRIS: Might it be reasonable to suggest that some domestic animals be put back on the islands on Rongelap itself, when you go out the next time, and those animals could be left during this interim which might possibly give you an idea of what the translocation range is before the natives come back.

DR. BOND: That again is for animals, and will be eating different food from the human.

MR. HARRIS: I am not thinking of extrapolating

to human beings. These people will introduce animals when they get back there. There may be a concentration of certain isotopes, such as strontium 90, for instance, in these animals or in the meat part s of these animals, which would be subsequently eaten by the natives. If you put animals back, immediately you would get an idea at measurable levels of what this translocation rate might be. Remembering that people are going to eat these animals later on.

CDR. CONARD: They won't eat the bones.

DR. BUGHER: What about taking Rongelap soil somewhere else and doing studies?

DR. BOND: If you are going to do that, you should know what you gave them, and not turn them loose at random.

It seems it is a pretty haphazard thing of putting animals there to see if they do or do not take up the material.

DR. BUGHER: The sort of thing you are speaking of is a matter of past record. It is not the haphazard situation that you are really interested in.

DR. ROBERTSON: I don't think it is a matter of pas record. Things have been given in more or less purified chemical form, but I don't think they have been mixed with anything that would compare with Rongelap.

MR. HARRIS: Isn't the haphazard normal in this cas

DR. BUGHER: That is the normal.

MR. HARRIS: This is what you are interested in. I am interested from the point of view of the natives and getting a practical experiment, rather than a laboratory experiment out of it.

DR. ROBERTSON: Are we asking for both?

MR. HARRIS: Personally I am not interested in either one of them, but I am suggesting that be done.

DR. BUGHER: It goes to what you are trying to establish. If you are trying to document the environment and what a pig, let us say, at Los Angeles, let us say, might do with respect to that material thrown into its food supply, is a matter of rather remote relevance to the island situation. We have such studies that were made of mixed fission product from weapons tests, not necessarily from that atoll. We did have 500 pounds returned, some to Beltsville and some to Los Angeles and Hanford. We have a lot of work in Nevada which is concerned precisely with that, and the uptake from the soils contaminated with fission product outfall.

CDR. CONARD: I think if you put some dogs on the animal, they would be more comparable to the human situation. They are quite as dirty as pigs and chickens in their eating habits.

DR. DUNHAM: But if you leave them there alone.

CDR. CONARD: I mean after they go back.

DR. BOND: It has been quite a problem of getting

the animals out there, and having to keep them alive for a stated period of time. There is no one on the island, no water supply and no natural food supply, It is not an easy problem.

DR. BUGHER: Unless an investigation or experiment really is relevant to the main issues of giving us sound information on the character of the environemtn, I don't think we should undertake it. Just putting a few dogs on the island when there are no people there is a fairly major venture. Those dogs will run about $5,000 each by the time you get them on the island. Then you ask the question, what do you get out of the venture, and I think you would come to the conclusion that you would not get very much, that would be descriptive of the island.

DR. DUNHAM: What would you expect to get from the data that would be important to the data of NRDL?

MR. HARRIS: I don't know. In one edible part you might find some concentration. This would be in the liver, for instance. I assume these people eat the livers of their chickens. According to the NRDL data, at least in the case of the fish, there is a fair concentration in the fleshy parts of the fish that are eaten. I really don't know. This was just a suggestion that I thought might have some application in the real long range thing, which is Operation Gabriel

DR. BUGHER: Yes.

MR. HARRIS: Then this might have some long range applications as far as Operation Gabriel is concerned.

DR. BUGHER: In this consideration, how do you feel about simply taking specimens at intervals as the islands are visited from their pigs, from their chickens and dogs, and from the people as they die, too, if you can possibly get the material, without making a special planned location of experimental animals? That is our Gabriel program which is essentially an empirical one, soil, plants, and animals, and locations of fission products. That costs almost nothing, then, in terms of manpower and time.

Wild animals might have been mentioned here. It is really implied in the fauna. The shell fish and crabs. Some of these islands have high populations of crabs, or did. I don't know what the situation would be now. It has always been an impressive thing that at Eniwetok, certain of the islands are favored localities for particular species of crabs. You find large populations of particular species on a particular island. I believe that holds true on the other atolls generally.

CDR. CONARD: Are these edible crabs you are speaking of?

DR. BUGHER: Yes, I presume they are.

CDR. CONARD: I have seen a lot of crabs there, but I wouldn't eat them, probably.

DR. BUGHER: It depends on how hungry you are. Is the big cocoanut crab eaten?

CDR. CRONKITE: They didn't list that crab, as I recall, in their list of items that they used. They at practically everything other than that.

DR. BUGHER: There must be come reason for that. In the surveys, specimen material of these various indigenous fauna would be desire to these varbus groups concerned with these analyses.

Under natural foodstuffs, do we mean the plants or the plants and fish?

CDR. CRONKITE: All of it.

DR. BUGHER: Specimens have been taken of cocoanuts, I believe.

MR. COHN: Cocoanuts, bannanas, papayas. I think, however, at the time the specimens were taken, it was too early to expect any incorporation of the fission products into the plant material itself. However, it will be desirable to study this at later intervals. That material does get into plan ts. We have pretty good indications of it in the fact that there is very high activity in the sap of the cocoa- nut tree. This is bound to be incorporated into the fruit some time later.

DR. BUGHER: You had cocoanut samples, too.

MR. COHN: There was no activity found in the edible

portions of any fruit.

DR. BUGHER: That is interesting. I had the impression that the cocoanuts also were showing activity. But the sap of the palm did, is that correct?

MR. COHN: Yes.

CDR. CRONKITE: Is there any possiblity that those samples could have gotten mixed up? There was one sample of juguru that was sitting out in the open, and there were other samples that were taken from the palm.

MR. COHN: We have three samples from Rongelap and three from Uterik, and they are all pretty consistent. The Uterik samples are one third of the activity of the Rongelap.

LT. CHAPMAN: In each instance there were two samples of material that had been exposed for five or six weeks, and one sample from Rongelap and one from Uterik which was collected fresh, and they were so labeled.

DR. BUGHER: No question of contamination from the container or anything.

MT. CHAPMAN: No, sir.

CDR. CONARD: Only in the one sample there was no question of contamination.

LT. CHAPMAN: No, we took our own containers.

MR. COHN: The activity was of the level of one microcurie per liter, and that is away above the external

contamination.

MR. HARLEY: We ran some of those, and the interesting feature to me was that on the two islands the water sample and the cocoanut sample were almost identical. It was just about the same activity.

MR. IMIRIE: That is just what I got, too.

CDR. CRONKITE: Dr. Bugher, there is a point that comes up almost you might say in experimental design that where the method by which the sample is collected is so important in the interpretation of the results, I just have a strong feeling that people that are doing it in the laboratory or someone that is working with them should do the collection. I presume that this juguru did have radioactive material in it, but if it were collected in the way in which the natives ordinarily collected it, I think it would be difficult to prove that it was coming from the external surface of the palm, rather than incorporating into the sap.

MR. HARLEY: You could tell that from the radio chemical studies.

MR. HARRIS: This is so hard. It is so much easier to have somebody there on the spot who is doing the actual collection, and who goes back to the laboratory and sits down and tells everybody in the laboratory what has been done. I think you might run into the same thing on the fish. Dr. Donaldson and those people from the fisheries

lab, will certainly collected plenty of fish, because they always do, but the ones who are doing the project, like Vic's group at NRDL, who are going to do this project, I presume, they should have somebody there at the time these fish are collected, and they should have a representative sample of the fish collected to take back and do their own analyses on. You can't take analyses from another laboratory and correlate them with those from a previous laboratory, and make a lot of sense. It is so much easier to have somebody there out of the operating group at the time of collection.

DR. BOND: This is precisely the reason why the people want to go to the field and collect the samples. They will then be well aware from where the sample is being derived.

Also, the reason why they wanted a more easier method of collecting that could be accomplished by air as accomplished previously, there was a continuous rush, continuous routing by the pilots to get back.

DR. BUGHER: You can't do good sample collecting under those circumstances. I don't think there is any argument there.

In regard to the plant samples, I think it will be recalled that this type of information is extremely interesting to plant physiologists, and bears very much on the concept of how contamination moves in plants, particularly

those supplying edible things. This very matter of finding materials in the sap of the palm and less or little in the cocoanut at that time, is a very significant thing, and points to the critical character of the sample collection. It has to be done very carefully, and with regard to rather a great variety of interests which other people may have in the material.

Are there any other comments?

CDR. CRONKITE: I recall in the survey group, Col. Byers was following the general philosophy on plant life that the initial ones would be where most of the naturation of the fruit had taken place prior to the fallout. Then somebody was going to take the initiative to investigate what is the normal life cycle of a cocoanut, and these various edible things, and then base the subsequent sample collection so as to get some that are maturing and some that are completely matured, and get a third or fourth generation of fruit. It seems that the whole sample collection will fall down unless it is tied into the life cycle of the fruit.

DR. BUGHER: You are speaking of the life cycle of the particular piece of cocoanut you have on your hand or the cycle of the fruiting of the palm? With the cocoanut it is a continuous process practically. Some are mature as others are just coming on in the infleureseence. That

becomes a matter of description more of the specimen at the time of collection, then.

Byer's group did take various specimens. Where did they go -- to NRDL?

MR. COHN: Yes.

DR. BUGHER: Are those still in process?

MR. COHN: These have been thoroughly analyzed. These are the results just mentioned.

DR. BUGHER: There you got a whole docoanut which was then opened in the laboratory.

MR. COHN: Yes.

DR. BUGHER: So you control the matter of how the material you analyzed was actually taken with reference to external contamination. Did you get much of anything from the cocoanut milk?

MR. COHN: No, very little activity in the cocoanut. In the pandana was the only case where there was any internal activity, and here the question is possibly contamination by washing. The foods were all washed externally, and the external wash was analyzed radiochemically. The pandana has a rough core, something like apineapple, and there is a possibility that the material was washed into it. But outside of this, there was no activity.

MR. HARLEY: It is very hard to determine how to clean a pandana.

MR. COHN: Yes.

DR. BUGHER: Do we need further comment on this phase of the matter? Dudley is here. You have heard this discussion. Do you have some thoughts in regard to the types of samples which should be forthcoming here, and the way they should be handled?

DR. DUDLEY: I think perhaps one should give some consideration to the isotope mainly interested in, which is mainly strontium, but perhaps not exclusively that. One would I think try to collect samples which conceivably would have considerable concentration of that. For example, I believe at Hanford they find comparatively little concentration in fruit. In the tomatos they find much more in the leaves. I think one should bear in mind the element one is interested in, and what type of plant is likely to concentrate. I think everything should be tied back in case of plants back to the soil.

MR. HARLEY: As far as we are concerned, you would have to have corresponding soil and plant. I think everybody is probably in the same boat. If you are thinking of uptake studies, that is what you would want.

DR. BUGHER: One of the complicating things here is that if you take a cocoanut palm, the degree of absorption from the leaf surface is something that I have no idea about. Maybe you have some data on it. But assuming that it does

occur, then the material of the palm sap and that which comes into the fruiting bodies may be derived either from leaf surface contamination or by way of the roots from the soil. The palm frond is rather persistent, surviving for two or three years before it gets old, and then eventually dies and falls off. The contamination, therefore, of the frond may persist as a factor in the situation for a considerable length of time. There may be a discrepancy between the soil levels and the apparent uptake in the fruit. There is where I think more precise plant physiologist, the one who knows a lot about palms, comes to the fore, that is, in guiding us somewhat on what vegetation specimens one might take to advantage. It makes a big difference on which palm frond you take.

CDR. CONARD: One thing that might be interesting to do that is not a great deal of trouble is to take leaf radio audiographs before the leaves get wilted. It is very simple to put them between film and enclose them in paper.

DR. BUGHER: That would be a very good guide. It is an easy technique and thoroughly reliable in its application. Obviously, them the group taking the environmental specimens will need to keep in close touch with a number of other groups and organizations on the sample collection project.

Now, we have a topic here of radiochemical studies, the extent to which radio chemistry should enter this environmental analytical picture. John Harley, that seems to be very squarely in your area of interest.

MR. HARLEY: There are a lot of other people involved in that, too. We have been doing some work on things that I am not sure are entirely useful, such as this fractionation -- geographical fractionation of material -- and that is certainly something that we would continue. In other words, our initial work was all on strontium. We now find that we can use cerium moderately well as a long term base line similarly to what they use molly for in short term studies. That seems to be fairly constant.

I think perhaps after some of us can talk this out a bit, it is going to be very-necessary to decide what is run in these different places so we can use the results from one laboratory to another. Certainly I think something like cerium might always be valuable for us in our comparisons.

We know pretty definitely that we may have the ruthinium problem here, and it certainly is present in high percentages. If what the British say is correct, we will be wanting to get a lot of information on that.

We have found that relations are not such that we can get all the information we would like. Therefore, we frequently have to scratch around with other people to work

up procedures that they should be running rather constantly.

For example, we had to work out completely new ruthinium procedures to handle many of the types of samples we have. I thinkPayne was mentioning that they were taking other methods and trying to adapt them. I think sometimes we have to exchange experiences again in these wider fields just as we have gone through in the strontium procedure.

DR. BUGHER: Yes.

MR. HARLEY: We have done an awful lot of our work oin these initial samples on just mixed fission products in trying to collect total activity. I don't know how many other people are doing that sort of work. We use itas a rough guide for a lot of things. If so, we would like to get together with them, too. I think it is going to require a certain amount ofcoordination or the final report is not going to mean too much. We have to decide both what we do as a general procedure, and more or less how we do it. Then individual labs may want to run half a dozenother things but that is up to them. I think for general procedures we are going to have to get together. You are thinking then in terms of strontium, cerium and ruthinium as specific isotopes.

What we would run on any sample now probably would be strontium, cerium, ruthinium, some of them still require barium because it is not all gone yet, and in

addition total rare earths and zirconium. With those you can generally come up with a pretty fair percentage. But you see when we send you a strontium result and a cerium result, right away you say what is the rest of it So somehow we want to get some sort of a total figure that will represent more or less what the activity is.

DR. BUGHER: The total activity.

MR. COHN: We have been doing strontium, barium and the rare earth group. On the fish we have broken it down a little further and try to do individual isotope analysis on zirconium and ruthinium.

Further than this we have not gone because of the difficulty of doing a large number of samples. It is quite a tedious procedure. Strontium, barium and rare earth group in most of our samples accounted for 70 to 80 per cent of the beta activity.

DR. BUGHER: How about plutonium in the soil?

MR. HARLEY: We have not run any yet. We have plans to. I don't know about Payne.

MR. COHN: We have plutdnium outside of the fruit, cocoanuts, papayas of the order of 10 to the fourth micrograms of plutonium.

DR. BUGHER: On a fruit, I take it.

MR. COHN: Yes. We have also measurements of plutonium on thatch taken from the top of the native huts,

on grass and soil, running about 10 to the third micrograms. That is at a fairly early interval.

MR. HARRIS: You might think of plutonium as one that you could follow. One thing I have thought of that would worry me a little bit would be the fact that in order for a long range application of this to Gebriel, it means a fairly long timestudy in which, due to leachime in transportation, s me of the samples are going to go down in activity relatively fast. Plutonium, however,is one thing that you can follow for a long period of time, much longer than you can some of these others, because of its decay rate, and its longer half life.

Another thing that you might think of is this: Do you have numbers on the coral as concentration of uranium, and this sort of thing? Do you have those numbers?

MR. HARLEY: We have a couple of them analyzed but we managed to sneak away a few museum pieces.

MR. HARRIS: There are numbers available on that area for blanks on alpha emitters in the coral, and these can be gotten if you need them.

Here is another thing. In some cases, you might be looking at an alpha, which might turn out to be uranium or thorium, if it was put on an alphaspectrum analyzer. At low levels where you get high grass location rates or high leaching rates, you might run down very soon to the point

where you would need to know what these backgrounds were.

MR. COHN: I think I better throw one correction in here. I said ten to the fourth. Those figures are ten to the minus fourth, and ten to the minus third.

MR. HARRIS: That is what I was thinking, that the whole island was going to go critical here.

DR. BUGHER: That is a lot of material, isn't it?

MR. HARLEY: That is per gram --

MR. COHN: In the thatch that is five times ten to the minus third per three gram sample. Soil is about one times ten to the minus third for a one gram sample of soil.

MR. HARRIS: Now you are getting down to the places close to the background already, and you will have to take sooner or later such as the natural blank alpha activity. They are available. The places to get them are from the radiochemical group at Los Alamos, who have to analyze this all the time. This has been done out there. There are other interesting things that might be of interest here.

For instance, among this coral that fell down on the island was an added amount of uranium which was set up alongside the experimental device to be able to subtract out the coral blank. [redacted]

[redacted] Various matters like this may have to be taken into consideration. You put so much there that the concentration is so much higher than the coral blank that when you add the coral blank into this mass the error introduced by adding the coral is very small and you can use the concentration that you find as a fraction of how much was set there.

DR. BUGHER: That is what they call keeping environment simple and untouched.

MR. HARLEY: Considered as a tracer.

MR. HARRIS: It was a tracer, yes.

DR. BUGHER: That is very helpful. I think perhaps if any of you have some thoughts from time to time on these things -- that is an important element which I missed entirely before, I don't know anything about the extra uranium being added to the blank situation -- I think all of us could very well spend a little time thinking a bit about things that are needed in this picture, things which should be analyzed for, the limitations, and if you have a thought like that, send it in and we will be glad to circulate it.

MR. HARRIS: These are things that come up and I just

happened to think of this one that occurred. You can also get such things as how much plutonium was made in the device during the detonation. This knowledge you will probably have some use for.

DR. BUGHER: We have that, yes.

MR. HARRIS: I don't think this can go into this unclassified report.

DR. BUGHER: No.

[redacted] You can use some of these numbers and come up with some better estimates. The majority of this information I am certain can be made available again by an on the spot situation by people who are doing this going to the radiochemical people at Los Alamos.

MR. HARLEY: One other point along that line. We found if you run a sample and find out it has five per cent of strontium 89, everyone says how does that compare with Hunter's curves. Is the data on fast fission readily available now? We have been working through Biology and Medicine, and have not been able to get it out of anyone yet.

MR. HARRIS: You mean the mass yield ratio and this sort of thing? Yes, I think that material is all essentially unclassified. The things you would be interested in, such things as ratio of uranium to 237 of course would not be. The majority of that is unclassified. I asked at one time a

couple of years ago about this situation as far as it pertained between ordinary 25 and 49. The best guess that Rod Spence was able to give was that the original curves which were made from U 235 should hold well for plutonium. I think that information is available somewhere to you and probably on an unclassified basis.

DR. DUDLEY: I am wondering if the discussion here of plutonium implies that there is to be a fairly serious study of plutonium from the point of view of Gabriel? Is this implied?

DR. BUGHER: Yes, it brings it in.

MR. HARLEY: That was considered definitely as part of it.

DR. BUGHER: Yes. In contrast to the situation before, where we had such small amounts that it did not really significantly come into the picture. Now with this very large amount of capture, it certainly is something we will have to go into.

DR. DUDLEY: The calculations relative to thermo nuclear weapons would not be different from the strontium to plutonium ratios than the original calculations for straight fission weapons?

MR. HARRIS: I think maybe they would be because of the presence in supers now of a fair amount of normal uranium which is much higher than the relative amount in

an ordinary fission weapon.

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MR. HARRIS: You mean half as much captured? I don't know. I haven't got specific figures on it but you could get these from radio chemists.

DR. BUGHER: You are speaking about captured fission ratio?

MR. HARRIS: What was that again?

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DR. DUDLEY: This would change the ratios by per cent and not orders of magnitude?

MR. HARRIS: That would change it in that case in orders of magnitude.

DR. BUGHER: I think the ratio here is not far from one. The figure we have been using is .9. You mentioned .8 yesterday.

DR. DUNNING: I did.

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DR. DUDLEY: I was thinking of the comparative hazard of the quantity of strontium and quantity of plutonium produced. Has it been suggested that the hazard may be comparable?

MR. HARLEY: My first calculations were that on an internal deposition basis, they would be about equal.

DR. BUGHER: That is something we have to keep in mind, and reconsider from time to time. I think that has been a very helpful discussion. We are coming near the time of compulsory adjournment.

If I can very compactly summarize what we have covered, we have reviewed the background and the data obtained from the first study of these people. We have

pretty well agreed that the type of study which was made in the acute phase will need to be continued for an indefinite time, but with a changing emphasis from what might be called acute problems to the long term effects which are particularly likely to manifest themselves in such things as shortening of life, the occurrence of tumors, both superficially and deep, and in bone changes, which may be of a minor nature. I think the expectation is that the long range results of the exposure of these people are not likely to be at all spectacular. One would have to look carefully and use rather critical statistical judgments in all likelihood to be able to say that anything will have occurred strictly due to the radiation. It points to the necessity of conducting continued studies in a very meticulous manner with precise recording of observations and data which will permit the type of statistical consideration that may be necessary.

We have agreed that the medical studies need not be tightly bound to the environmental studies; that two more or less separate groups can do these two things. I think, though, we all concede that everybody is interested in what everybody else is doing in this study and it doesn't mean that cross information won't be freely flowing. It looks as though the NRDL group probably should be called on for the first medical study of the Rongelap people, and also

the first environmental study and specimen collection.

The minutes here of this conference will give suggestions as to the types of materials to be collected and precautions with respect to collecting them. I think we will be able to further advise the collecting group about types of material to be sampled and precautions to be exercised in caring for it, and the places in which some of these specimens should go for further analytical study.

We may well find that two or even three groups are interested in analyzing for the same thing. Especially in these low level things I don't believe there is any objection to that. If we talk about strontium 90, we may want to send around to each of the participating laboratories a standard ash, which we do have, containing strontium 90. It has been useful in checking strontium 90 analysis in one place as against at another.

I think as to the organizational matters we have really covered that. This Division will attempt to be a coordinating center and work with the services, with the trust territory administration, and many of the problems we encounter we will have to ask for help from one or the other of the various services that have special facilities.

Some points are left somewhat undecided. For example, whether or not to use aerial survey techniques and the extent of ground survey. I think we will need to

discuss those a little more extensively.

Capt. Yarbrough, can you think of anything whih should be added here?

CAPT. YARBROUGH: No, sir, at the moment I have nothing to add.

DR. BUGHER: We will have plenty of detailed problems and we can sove those as they come up. We hope that the work on the report now goes along expeditiously, and we will all try to do our best in helping the people who have to turn out the report to get their job done. We expect two sections. Section 1 will come a little before Section 2, I presume, being a little less bulky. Do you have any further comment, Gene?

CDR. CRONKITE: No. I think it has been most gratifying to get clear in everybody's mind what the administrative machinery is and we shall now try to deluge you through our channels for a lot of things for you to integrate in the very near future.

DR. BUGHER: It will be a single integral, I hope, and not a double.

We appreciate very much your all giving your time to this program. I hope you realize that our statements of appreciation are really very much understated.

As far as the whole conduct of this program is concerned, that is true.

Incidentally, the Russian resolution yesterday

was rejected by the Committee on Petitions, the Russian vote being the only one in favor of it. So at least as it stands in the UN, the United States doesn't stand condemned as having been derelict in its duty.

There will be another set of resolutions coming up for consideration tomorrow.

MR. HARRIS: One thing you have forgotten. This name on the conference thing is rather long. I would like to propose a name for this project. Could you call this SBCC, the Super Bomb Casualty Commission?

DR. BUGHER: Very good. Thank you.

We appreciate all these things that result in shortened labor.

(Thereupon at 3:10 p.m., the conference was concluded.)