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ASHALL: (continued) on how peaked you think that this map looks like ahead of time; and one of the functions of a real pilot program would be to find out precisely this. Any sampling design has to produce really two things. One, an estimate of the quantity you are after and also an estimate of its intrinsic variability. So it turns out if this thing is very peaked, the way to go at this problem is to pick a few points initially at random, and then sample around it in big clusters. This would mean that you take many samples near this point, if the thing is very peaked. If it is quite smooth, then one spreads his samples around so that this gives you the optimal estimate for a fixed amount of money. Where it's usually the case that if one samples in this way, around a certain location, it costs less per sample. So that some guesses are going to have to be made. One wants to get down to list of just what it is that we want to sample — we can't really just be satisfied with just saying New England. You have to talk about what it is you are going to do in this particular area. Now in thinking about this problem, the only portion of it that I've looked into in any way is the human sampling problem. Assuming setting aside all these problems of getting the actual material, I think that we ought to aim at getting a correlation between something calculated from Eisenbud's data, for particular areas. What I would have in mind due to the fact that the food distribution in this country is so widespread that one would try to take weighted average of say a circle of roughly 150 miles around a city which generally represents its

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RSHALL: (continued)

milk supply and water supply area. The country-wide average on some average weighting is very heavily in the midwest for the food producing areas. Then, we should try to calculate for a specific area a potential exposure to this risk and then relate this to the amount that one found in peoples' bones, taking account of their ages. I think that in view of the fact that it is probably going to cost us a fair amount of money to get the samples, they ought to be designed very carefully, and also a good deal of ingenuity and money put into the analysis. We can't assume ahead of time that we are going to get very good answers, but at least we ought to have in mind that we should produce an answer that might allow us to give rough predictions from a series of detonations using all of the work that will go into the transport and so on, telling us if a certain number of bombs of specified yield are set off roughly in these locations at this time of the year and transported, what is the hazard in the various areas? Eisnebud's data indicates that it falls off fairly regularly, i.e., the fallout - from the site of the detonation. On the other ~~XX~~ hand, if some of the material this morning indicated that if you are looking at soil, this thing is liable to look very peaked, then you are going to have a big problem. If you are looking at human bones which sort of draw in an average over the whole country, one might expect more regularities. It's hard to say. It's going to depend on whether this local food supply - the milk and the water, i.e., the big components

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MARSHALL: (continued) in the picture, or the more widespread foodstuffs. After a little calculation, if it turns out that say a hundred grams or so of bone material were enough to allow you to take a measurement with a guess of a hundred dollars for the analysis of this particular sample, one, I think, could in this country — the calculation I did for ten cities assuming that one takes account of various age groups, start say with an age group of from 0 to 5, 5 to 10, 10 to 15, 15 to 20, and 20 and over, perhaps, that one might take three samples in each age group totaling 300 grams, randomly sort the pieces into 300 gram units, analyze each one, and get his count. This is all for one city, and for the various age groups, and get an idea of how the fallout in the area and the countrywide averages related to the amount of Sr^{90} in human bones of the function of age, from the past experiences. From this, one might be able to try some predictions, depending upon how variable one found it and one needs, I think, in each one of these areas, at least two determinations at each age group, for the purpose of getting an idea of the variations even for diets within a particular area.

LIBBY: Take a ~~[illegible]~~ milorganite case of the human sewage of Milwaukee. You've certainly got an enormous averaging already in the sampling.

MARSHALL: Yes.

LIBBY: Now, what would you do in addition in order to get the variability parameter? You certainly have in that, a pretty fair average for the food and so on for the city of Milwaukee.

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MITCHELL: (continued) to have enough trouble measuring it in bones: you are going to have infinitely more trouble measuring it in anything else.

LIBBY: What is the stuff that makes you get your teeth clean? Is that calcium containing? Think of all these things you can get gratis, and then use your maximum sensitivity, and see if maybe you can sample people without killing them.

SOLOMON: Plasma has five hundredths of a percent of the total body calcium as against 98% in the bones, and almost 1% in the teeth. Everything is weighted against you.

VOICE: Is there anything you can feed people to make them slough off a layer of calcium from the bones?

SOLOMON: There is a possibility that some of the versinates (?) may bring some calcium out. I don't know. But it has been supposed to bring lead out. Calcium versinate has been proposed in order to bring lead out.

COLMAR: It's very, very hard to bring calcium out.

LIBBY: As you say, a tolerance limit of one microcurie..

MITCHELL: Well, the most sensitive methods will pick up about one-millionth of that, so you see it is a possibility. I don't know what the average person would be in terms of tolerance. I suppose very low. One doesn't know, but the bones of a person who is growing through the period of exposure will essentially carry an integrated dose of anything that might be, say, in the bloodstream. So if you

[redacted] pick up a sample of blood today, you have x amount, you will have the accumulated amount of x times a certain numb

[redacted]

SOLOMON: (continued) the blood from clotting. Now if this were so, this ion exchange resin is discarded; it isn't used any more. If it were so, in any large blood program, one could get a tremendous amount of ion exchange resin from which you would only have to dilute, which you could do quite easily..the calcium but I don't know enough about the blood program. Another possibility is what is thrown away in the gamma globulin.

LIBBY: Doesn't all that blood come from adults? That would be very good.

SOLOMON: Most of it, I suppose, does.

LIBBY: On the other hand it contains the lost calcium, and that's the stuff that would have the strontium in it.

SOLOMON: With an adult, bones are argued against because bones are made, but the calcium in the blood is still in the blood, and is still circulating, and so that's not a valid objection. This would require getting in touch with the Red Cross and learning some of the details of their blood collection. You can use dated bank blood too.

LIBBY: You think bone meal is something we ought to play with from a statistical point of view? Take the Chicago stock yards, for example.

MARSHALL: Well, here again I think it's a question of — what is the question you are trying to answer?

LIBBY: Well, suppose we measured the bone meal content from the Chicago stockyards, and we find this to be something or other. Now would

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LIBBY: Any one else have a question for Mr. Marshall? Before we leave the sampling program, I'd like to mention a subject which Colonel Holzman is connected with. Several weeks ago we were considering this stratospheric storage, and concluded then as we did this morning, that it probably exists, that there is probably a lot of radioactivity in the high layers of the atmosphere, and the question is how to prove this. It is obvious from remarks this morning that rainstorms and the vagaries of weather make the assay of rains a rather unreliable way of establishing it quantitatively. So we wonder how to sample the high atmosphere and get samples down which give us some qualitative notion of the content of radioactivity. There are two aspects — one is to get up there, and the other is to get the radioactivity out of the air. We asked Colonel Holzman to make some measurements on jet aircraft which are operating out of Kirtland, and I'd like to give him a couple of minutes to tell us about that. I know these data are very poor, but there is some radioactivity on these airplanes, and as I understand it, they were not in any atomic cloud that you knew of?

HOLZMAN: That's right, Dr. Libby. About a month or so ago, Dr. Libby requested just a yes-and-no answer as to whether any of our jets were picking up any radioactivity. So we ran a "quick-and-dirty" test on this. There were about fifteen aircraft. We made sure that the aircraft were not those which might have

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LIBBY: Would they have them in Korea? Colonel Hooks, would you know about this?

HOOKS: I don't know for certain. I assume not.

LIBBY: How about Europe?

HOOKS: Yes.

LIBBY: I have a notion we may get some definite proof of the existence of high lying radioactivity just by watching operational aircraft in this simple manner. Now, of course, putting on a filter or anything like this is bound to be some trouble, but if it is as simple as measuring radioactivity on the inside channels of a duct in a jet, I think maybe we might get something. Now of course it would be rough, but at least it may be worth getting. I just wonder whether it isn't worth trying. It isn't entirely crazy. There is a thermal gradient in the system, and this thing is a possibility of separating out particulate matter. The yield, of course, would be enormously low, but there is probably a lot of stuff in the large volume of air that goes through these airplanes that compensates. Another line of attack is to develop a sampling program for the high altitudes, and I think this would have to be done on a longer range basis. I think it is quite important to sample the higher layers before the CASTLE series, if we possibly can. If anyone has any idea of a simple way of using operational airplanes to get evidence of radioactivity in the stratosphere—well, above 30,000 ft., it would be quite valuable.

[redacted]

VOICE: Are any rockets being fired at White Sands?

VOICE: They don't stay up very long.

CADLE: I don't think that could compare with sampling from the jets.

KELLOGG: One thing we've been thinking about a little is the possibility of sampling, or getting a direct measurement of radioactivity in the stratosphere by a method which would involve sending balloons. The advantage of balloons is that they can get up, at present, higher than any operational aircraft that we have. I suspect that XXX since the height of the tropopause in the Marshall Islands is around 55,000 ft., we would just about have to have balloons in order to sample the stratosphere at these levels. In fact, if we can consider the transport anything like horizontal, then we would expect to see the debris come along at heights from 55,000 ft up and down when it reached the middle latitudes. In trying to imagine what balloon sampling would look like, we have been inquiring about some method of doing it similarly — on a basis similar to our present radiosonde networks. In the history of upper atmosphere research — I can't remember back, but I can read about the great cost of the early radiosondes — it was XX considered quite a trick to do it. Now we have upwards of 30 stations in the U.S. making two soundings a day on a routine basis for a very nominal cost. We inquired about the cost of sending up a piece of conductivity equipment which could actually be inserted into one of the channels of the ordinary radiosonde,

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KELLOGG: (continued)

taking advantage of the telemetering already existing at big stations all over the world. Measuring the conductivity would be one way of doing it. The other obvious way would be to measure the radioactivity. That is, by some method of Geiger counters properly oriented so as to eliminate cosmic radiation as much as possible. From what I have been able to gather, it is out of the field of the second, that is, the direct Geiger counters, a little bit out of the field of our specialty - but it would appear to be rather hard to do. It involves quite a lot of instrumentation. We have looked into the question of measuring the conductivity in the upper atmosphere, and it appears that this could be done with fairly reasonable equipmtnt, and it also appears that there is going to be a very large change in the conductivity wherever we have changes in the radioactivity. This will come out this afternoon, perhaps, when we give a few facts and figures about the atmospheric ion content in a radioactive cloud.

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KELLOGG: like to have us all think a little more about.

LIBBY: Well, fine Will, but how many years will this take?

KELLOGG: Some equipment was built for Sandstone so we would have some background in this instrumentation but I don't have the details of it.

LIBBY: It certainly could not be organized before Castle, could it?

KELLOGG: I wouldn't consider that it couldn't be. I think that, well, you see the telemetering is an important part of it. This is already available. If we can devise the output of our conductivity equipment to adjust a variable resistance which is roughly the same resistance range as the present elements. NRL is the place where I got my information from. They estimate $200. Perhaps Mr. Smith of NRL wouldn't like to be quoted too definitely on this, but he thought it could be made for about $200 per equipment.

LIBBY: What load will it carry?

KELLOGG: Oh, this is the gear exclusive of the balloons.

HOLZMAN: This was done at Sandstone very successfully. I get the feeling, Dr. Libby, that many of us here are thinking in terms of this material hanging up in the stratosphere for long, long periods of time. Although I have been getting more and more away from meteorology I think that this is a very wrong concept because air from stratospheric levels can come down many, many thousands

ZMAN: (Continued) of feet in the ordinary weather situations. For example, air that might be at the stratosphere today, might be down 20 to 15,000 feet in say 24 hours. Would this be a reasonable thing, Harry?

WEXLER: Sure, I'd even go further and say it would be down to the ground under certain conditions.

LIBBY: Then you would say that the mean life of stratospheric air is two days or of that order?

WEXLER: No, this is only under rather exceptionally favorable conditions.

LIBBY: Well, if you take an average what would be the length of time before it came down to sea level?

WEXLER: Well, you take an average between two days and six months, maybe, or a year.

LIBBY: But it would be in the order of weeks or months?

VOICE: The exceptional case would be high thunderstorms?

HOLZMAN: No, I am thinking really of isenthropic flow down slope. You see air doesn't flow horizontally, it flows down, and if you have an unusual weather situation with high pressure, low pressure and so forth depending upon the complexity of the weather, air can come down from stratospheric levels as Harry said, even down to the ground, and this is available for precipitation maybe the following or the subsequent days. So the atmosphere is constantly

HOLZMAN: (Continued) purging itself even at very, very high levels and for this reason I thought that some of the data that, well in one of our other programs might be carefully analysed and so give us a lot of information as to how much the atmosphere is purging itself of this debris.

LIBBY: Do you imagine that it might be at great heights? That the radioactivity might be at great heights?

HOLZMANN: Well, I got the feeling from your remarks and others that this thing might hover at stratospheric levels ....

VOICE: Isn't your point coming to say that your guess would be that the radioactivity, assuming a period two or three months after a shot, that the radioactivity would be pretty well distributed?

HOLZMANN: That is right. It is pretty well distributed but it is constantly being purged and that data even at lower levels can be analysed to give you a pretty good indication as to what is going on above. That is what I was getting at so that there might be sufficient data around with careful study that could give us some answers that we are seeking here.

VOICE: If this were true then it would seem that Eisenbud's data should show this, because let us assume that there is a six month half-life then he ought to have a six month slope on his fallout and he doesn't have that.

EISENBUD: Well, we don't know, we haven't had the time.

WEXLER: I think that this depends upon whether the stuff is getting up to the stratosphere. Most of his stuff is below the rainout layer.

BETHE: I think that it is the opposite. Namely, that apparently there is no obvious decay yet from Ivy.

EISENBUD: Well, I would like to go back to this Krakatoa. Maybe we could squeeze something out of that. Now there was a ten fold demanution in solar constant.

WEXLER: 10%

EISENBUD: No, ten fold because there was a ten percent increase making the 10 one percent difference and it took them three years to get this 10% peak down to 1% where they could no longer determine the change so that is really the ten fold _____ over a three year period.

WEXLER: Well, the only thing I said was that there was a 10% decrease as an average over the three years.

Eisenbud: Oh, I thought that it started as 10%.

WEXLER: No, you take 100% as normals. It went down to 90% and then back to 100%.

LIBBY: You are talking about dust content.

[redacted]

ENBUD: Yes, this is referable to the mass of dust.

WEXLER: It depends on the sizes. But about this stratospheric exchange, I agree with everything that Ben says about the lower portion of the stratosphere which you might say is isotropically connected with the troposphere. When you get up above into the stratosphere, then the direct exchange between it and the lower atmosphere becomes much more difficult to do because there are no isotropic surfaces that really penetrate up. They are mostly horizontal up there. So, to get things way high up they are likely to stay there except by a very slow process of diffusion or fallout. Very slow, but none of this very rapid quasi horizontal large scale of exchange that Ben was talking about.

VOICE: How high?

WEXLER: I would say that if you get up to 100,000 feet.

GRIGGS: My recollection is that in the case of Krakatoa is that they observed brilliant sunsets in the Sahara desert and other places and they persisted for a matter of months and possibly a year. These things were such as to indicate the presence of dust as scattering at a very high altitude well above the tropopause.

WEXLER: That is right. That is how they estimated the height to which the stuff went. At least 100,000 feet.

GRIGGS: There was persistence of this dust in the high stratosphere a long time.

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LER: Yes, and it gradually settled down. They are able to detect that by optical effects, gradual decrease in height of the main body of dust.

GRIGGS: Is this consistent with your picture?

WEXLER: Yes, I think so because that went well up above the low portion of the stratosphere where it was effectively sealed off from isotropic exchange for lower atmosphere. This is also born out by some moisture measurements that have been made. There have been about three moisture measurements made by a balloon going up 100,000 feet and it shows that in the stratosphere the atmosphere is extremely well stratified vertically. That is, there are layers moist or rather moist air and dry air. This indicates an extremely low rate of mixing. On the other hand, all the measurements that have been made indicate that the composition of the atmosphere, that is the permanent gases are extremely uniform. It sort of gives you an idea of the time scale of mixing. It is somewhere inbetween probably days or weeks where moisture, precipitation, evaporation and things like that can remain stratified and the time required to do uniform mixing. In that same connection, are the samples obtained by rockets too small to be analysed?

LIBBY: I don't know. We don't know what the content is .

WEXLER: They get samples down to the order of a couple hundred cc I guess.

[redacted]

OMON: This discussion seems to me to indicate that the pilot query which we have up there is only part of the question and the other pilot query which we had was what is the distribution of radioactivity for Strontium 90 in the world today? I think that they are two quite different problems. To determine as human hazard requires one set of measurements, whereas to determine its distribution in the world which is integrated into the human hazard requires a different set of measurements. You wouldn't need to make any stratospheric measurements to determine the human hazard as long as you had fallout.

LIBBY: I think, on the contrary, that you probably would. In order to back up finding the radioactive Negroes in South Africa. You find a radioactive person in South Africa and say how in the world. Maybe he took a trip or maybe he ate Alaskan canned salmon, and you begin to investigate and find that the rain is radioactive with Strontium.

SOLOMON: But the fallout takes account of a great deal of this. In other words, one is operational and the other is science.

HILL: There is another important thing. If you analyse, say in your pilot study, over a period of months you find that a certain amount of stuff is biological material. You are still faced with the problem of what is going to happen in the future and until you arrive at something about distribution in the lithosphere and hydrosphere as well, I don't see how you are going to extrapolate with any competence.

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S. MON: I am not going to argue against it. It seems to me actually a much more fascinating problem than finding what the human hazard is. It is just that if one is trying to put these into words this is the set of words which don't exist in the sampling program and I think that they belong there.

LIBBY: I think that we all agree there, but there may still be some who still wonder why we study Strontium on a worldwide basis. I hope our presentation in the last two days helped to answer this question. It would seem that you might well say take the Americans as being the fair example, but our problems of worldwide circulation are obviously so serious and so important that I think anything less than a worldwide assay or an assay that doesn't have some samples spread all over the world will be unsatisfactory. We do not know whether Strontium goes with the ordinary fission products; this has to be settled. We have very good reason for supposing that it will not go with them, that there is a big fractionation and so we have to do an assay. Oh, the third thing is the long lifetime means that the mixing processes will carry it all over the world, certainly in the atmosphere and very probably in the ocean currents, at least as far as the surface layers are concerned. So, I don't think isolationism has any proper roll in this. We have got to take a broad point of view. Return to the original Gabriel question, it wasn't whether you killed Americans: it is whether you kill people, and I think that we must assay the world, not our own backyard. Are there any people here who object to this or want to discuss it.

ENBUD: Our own backyard is, after all, 3,000 miles in one dimension. Isn't that a fair sample of the world?

LIBBY: No, I doubt it because the world is so much bigger than that. Take the equatorial regions, we don't have representatives there. The equatorial regions represent half the world's surface and we don't have much of it, you know. They have great rainfall in this area and most likely if it is upstairs a large fraction of the precipitation occurs in these equatorial regions. I think that there are abundant arguments for it. I think that it is true that we would certainly take more samples in this country than in any other continent. We mustn't slip into the notion that because it is more convenient that this assay would suffice. I don't believe it will.

LONG: I wonder somewhat though whether this is a terribly important question for this conference though. That is, if you are operating in the league of the pre-pilot and pilot queries it seems to me that in that league you can build a very sound case for operation.

LIBBY: Well, I will answer that in the following way; it is a question of whether you will operate at all or not. It is a question as to whether you are interested in Gabriel as such. Nobody is interested in analysing a few isolated bone samples just for the fun of it. People will do this because they are interested in the Gabriel project, so if you don't keep the general purpose of it in mind you are not even going to get the pilot action. That is why Strontium 90 has never been assayed. Nobody has had any reason to do it. It isn't useful to Spence in getting radiochemical yields so it hasn't [redacted].

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[illegible]: I was impressed by what Mr. Solomon said, namely that the sampling thing, by its nature, had to be a developing thing and it just seems to me that worldwide aspect could very reasonably be put _____

LIBBY: The only reason that the development will ever go is the worldwide assay. That is my answer.

I would like to suggest that we break off for lunch in two or three minutes. Let me just tell you what I think we can do on a detection. Of course all of this has to be worked out and proven. But the detectable levels, of course this means that you are going to work hardest on these. The lower limit, I would say, is about 1 dpm and this to be contained in any amount of sample up to 100 pounds. Now what is one dpm - that is one-millionth of human tellerance. The person who is just beginning to feel the effects of Strontium; it is contained in one milligram of calcium in his body. In other words, there is absolutely no difficulty in determining Strontium in human bodies if it gets any where near tolerance. Now who knows what it is now? I don't even know what calculations can be made; I can't even guess. Is anyone willing to guess what it might be now? If you say that it is a thousandth of human tolerance at the present time then one gram of calcium is the minimum you will be needing for this sample. Now, the present amount of Strontium in the world. The present assay; well, we say a 20 kiloton bomb gives 1 gram of Strontium per KT roughly. With a 5% fission yield we have this many Strontium 90 atoms produced from kiloton bomb and

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BY: (Continued)

divide this by the area of the earth this number comes out 20,000 Strontium 90 atoms per square centimeter. If we say we we shot off say 1,000 bombs this would be per kiloton bomb, you get 20,000,000 Strontium 90's per square centimeter's at the moment. It happens that 15,000,000 give you 1 dpm so the present assay is 1 dpm per square centimeter. That is all you know from the rough overall yield figures. That is how much there is, in other words the limited detectability is equal to one square centimeter for uniform distribution at the moment. The human tolerance is equal to a million square centimeters. That is the ingestion of the Strontium in an area of a million square centimeters, that is 10 meters square - you swallow that much, you are up to human tolerance at the present time. I don't know that any other remarks at the present time would be particularly pertinent. Now, naturally these are the hardest things to do and it will be easier to do Richardson's, this can be done I am quite certain.

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ARAWISH: We would like this morning to discuss briefly the results of the separate meeting. If we can't tell what we do know, we should try to outline what we don't know—which I imagine is quite a bit. We would like to conclude the morning session, after the coffee break, with a discussion of the GABRIEL problem, giving some of the results of the "old GABRIEL" and giving some of the tentative conclusions of the classification policy meeting of yesterday which turned eventually into a technical GABRIEL discussion.

Will Kellogg would like to talk to you now, on the sampling program involved in Part I.

LLOGG: After the discussion of yesterday afternoon, in which we went over the possibilities, needs, and advisability of making some experiment where we could follow the debris in the atmosphere—what I have to say now may be really thought of as a review of the factors, and I shall try to state some of the discussion that took place here yesterday afternoon, briefly, for the benefit of those who didn't hear it. Because I think a lot of interesting factors did come up.

I'll take first, our original attitude toward this. In looking at the local fallout picture—by this we mean the fallout, rain-out, in the first few days—we found that one of the most difficult parameters to talk about, much less to get any quantitative estimates on, was the vertical transport of debris. Harry Wexler

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KELLOGG: (continued) mentioned that this vertical transport debris is sort of a dead horse—I think that was the term he used—and I am inclined to agree; a lot of people blame their troubles on the difficulty of handling vertical transport in the atmosphere. Undoubtedly, there will never be a really precise way of dealing with this because it's such a variable quantity. Yet, we do need, it seems to me, to have some sort of a better way of following it, even though we'll have to admit that it will be only for rather specific cases.

In order to find out more about this business of the transport of the debris in the atmosphere after the first few days, it seemed to us only logical to at least think about some sort of an experiment which would permit us to track it, and we wanted not only to consider the work that had been done before—many able attempts have been made before to track the debris in three dimensions, and we wanted to consider these; but we wanted also to see if there wasn't a possibility of doing it a little bit more elaborately, if it is decided and agreed that there is a need for it. The previous method has been, in general, to use aircraft to follow the radioactive cloud. I might mention, just briefly, that you can only follow the radioactive cloud visually for about an hour or two at the most. It becomes impossible to triangulate very well on the cloud from the ground, after about an hour; and even from the air it becomes rather hard to follow it after a few hours. One has to resort to methods involving

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KELLOGG: (continued)

other sampling, in which one exposes filter papers for known times and known parts of space, and finds the number of counts per minute. Or one uses some other devise, such as conductivity; and, of course, direct measurement of radiation in the air.

These experiments do give results; we have reports published by the Air Force in which they have flown horizontal tracks across the path of the cloud on regular schedules, and so have been able to trace the movement of the cloud across the country. However, in the cases that I have happened to have read, the aircraft never went up to the height at which the initial debris was distributed. They must have been flying through the fallout part of the cloud and not getting into the original mushroom. It would be possible, with aircraft, to get up into the mushroom from the Nevada shot.

We talked about the Marshall Island shot, and considered aircraft operations. I think, here, it is obvious that we could never get up into the mushroom cloud from such a cloud as the MIKE shot which, although the estimates vary, there is agreement that it did penetrate into the stratosphere. The stratosphere starts at around 55,000 in the Marshall Islands, so this is above any practical ceiling for test aircraft.

The other alternative, then, is to consider balloons, because balloons can get up very high. There is another reason for thinking that balloons might be a practical way of doing it,

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KELLOGG: (continued)

and that is because of the way they cut through the clouds. If we could have the first slide, I could remind you of the way the atmosphere is shaped, and the way a cloud moving in the atmosphere is shaped. This is a slide prepared by Jim Edinger, and those of you who were here the first afternoon saw the slide. It shows a sketch—and this is a fanciful sketch, but it is based on the way one would expect the atmosphere to behave—showing the cloud at the end of, roughly, one day, and taking two cases: one a rather unrealistic case in which there is no shear and in which the cloud simply spreads through to the action of gross turbulence and fine-scale turbulence together and, as Jim pointed out (we won't go through the arguments again), you would not expect it to be homogeneously distributed; it would be pulled apart and would present wisps and hot spots and gaps and patterns. Then the argument went on to indicate that actually we almost invariably would have shears, and so the cloud, instead of being in a pancake section of the atmosphere, would actually be spread out in a long belt. The dimensions here are conservative for one day; they actually would, in most cases I think, be even larger than this, and this drawn to scale. This is actually a scale drawing of such a cloud at the end of one day; as I said, these horizontal spreads are conservative. This shows it, as one would expect it to be, extremely flat. Now, in the case of low diffusion—and there was some argument yesterday to indicate that the diffusion

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KELLOGG: (continued) would usually be rather small in the troposphere, and even smaller in the stratosphere--actually it would be a thin ribbon cloud no wider than my stick. On a day when there was strong convective activity stirring, the layer occupied by the cloud might go down to the ground by the end of the day. Then it would be a little bit thicker.

I think this shows one thing; that is, if we made a horizontal traverse through it, we would get a long slice this way, whereas a few traverses through it vertically would intersect the cloud and would also provide a three-dimensional picture. I think it could be done either way. One can visualize slicing through this way, with the line corresponding to an aircraft flight; or slicing through it this way with a balloon--but I wanted to show this slide in order to show that one would not have to have a very dense network of balloons in order to intersect the part of the cloud.

LIBBY: What experiment are you proposing?

KELLOGG: This is an experiment which would be presumably to follow the cloud in three dimensions from the time of burst.

LIBBY: Are these balloons staked out, or do they go along with it?

KELLOGG: No, these are sounding balloons in the sense of a sounding being made from the ground upward, exactly as the radio sounds which are sent up twice a day all over the world now. The idea

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..ELLOGG: (continued)

of sending an expendable piece [redacted]quipment to measure the density of the radioactivity in the air was, I think, questionable to us at first; we were reassured, however, by finding that NRL had already built equipment which uses counters and which cost would be in the order of $200 per instrument, exclusive, of course, of the balloons. It turns out now, that Pete Wyckoff tells us that the Air Force is also in the preliminary stage of developing a similar equipment; using, however, instead of counters, the measurement of conductivity. A few flights have been made which show that the instrument is practical and, presumably—if I am quoting you right—it could be done between $100 and $200 per instrument, or something in that region, if one wanted to produce a lot of these.

The radiosonde network already provides a large number of telemetering stations. It also provides crews for launching balloons. This is an attractive idea, practically, because it means one does not have to set up the balloon-launching network—it's already there; one would merely tie on extra equipment when one wanted to try this experiment.

The question of how long we can track the cloud using conductivity measurements came up yesterday, and none of us was quick enough to make the calculations in our heads; however, I figured from what was said yesterday, that apparently the rate of creation of ions by cosmic rays in the region

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KELLOGG: (continued) in which we are interested, in the vicinity of the tropopause, is about one ion pair per cubic centimeter per second. Is this right—to those who might be more familiar with cosmic rays? I worked backward, using the recombination coefficient and the number of atmospheric ions, and came to the conclusion that it would be about one per cubic centimeter per second from cosmic rays at around 20 kilometers....What's that?

BETHE: That sounds rather low.

KELLOGG: Low?

HOLZER: Yes, I think it is low, Will.

LLOGG: Well, the number that was mentioned was a thousand atmospheric ions per cubic centimeter.

WYCKOFF(?): The mobility is very high up there, you know.

KELLOGG: Well, just taking the number of ion pairs—this number was mentioned—and if one uses the usual 10^6 for a recombination coefficient of atmospheric ions, then one concludes that 1,000 ion pairs per cubic centimeter and this rate of recombination would be equilibrium for one ion pair produced per cubic centimeter per second.

I think that we could raise this by a factor of 10 easily. Is it a facter of more than 10? Well anyway, the number of ion pairs formed by atomic cloud 20 KT, and roughly the size

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KELLOGG: (continued) which I have indicated there, taking into account decay would be in the order of 400,000 ion pairs per cubic centimeter per second in one day.

VOICE: What time element?

KELLOGG: One day having expended so that the volume, this is the average in the cloud, has expanded due to this kind of diffusion. At the end of two days, three days, four days, it will of course decay further and would--I have not made the calculation as to when it would get down to the level of cosmic rays, I judge it would be something in the order of a week before it would get down in the center of such a cloud to anywhere approaching the cosmic-ray ion production. This is one thing which had to be estimated first. I don't think this estimate is a very accurate one, but it does suggest that one could use the conductivity to track the cloud for at least a matter of days. The idea of finding where the cloud goes in the stratosphere is a very attractive one, because I think it is largely a matter of conjecture now as to how vertical diffusion does take place in the stratosphere. I agree with H. Wexler in imagining that it would be very slow. Vertical diffusion in the upper part of the stratosphere certainly would be very slow.

We have some evidence, though, that suggests that vertical diffusion in the lower part of the stratosphere, which is also stable, is rather high. This is based on observations of smoke

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KELLOGG: (continued) clouds which have been made in this part of the atmosphere, which show, in fact, that as one goes from the relatively unstable troposphere where we have the decrease of temperature with height, into the stratosphere where we have roughly isothermal regions, the smoke puffs which grow at a certain rate in the troposphere, grew even faster in the lower stratosphere. This was an observation made on the basis of about 20 smoke puffs over Holloman Air Force Base in the summer of 1949. This conclusion was rather surprising, because it was usually assumed that when you got into the stable layer the diffusion would be slower. Now, what takes place still further on up is now a matter of conjecture and, as I say, I would be inclined to agree that the rate of mixing was slower in this upper region. But I really don't think that we can say that definitely yet.

WEXLER: Will these measurements...do they actually determine a three-dimensional shape of the puff before and after?

KELLOGG: There were three phototheodolites, and it was possible to determine the shape. An effort was made to separate the effect of shear.

WEXLER: Was there any indication of non-isotropical...I mean, was there any elongation of an axis in certain directions?

KELLOGG: Yes, but this almost always in a horizontal .

WEXLER: How do you know that most of this was not due to isentropic

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WEXLER: (continued) motions rather than turbulent diffusion?

KELLOGG: The horizontal elongation was assumed to be due to shear, and so it was only the spread at right angles to the shear that was considered. When I drew this, I was implying that we've eliminated the effects of shear from the data.

VOICE: The Signal Corps has been trying to measure width by setting up smoke trails from rockets, and has been unsuccessful because the diffusion rate is so great that they don't even have a chance to photograph it.

This is the horizontal?

VOICE: Now this is just dissipation of the vapor trail itself, with the smoke trail.

KELLOGG: I've heard this too.

E. PLESSET: It just breaks apart.

VOICE: Do you think this in the region below 100,000 feet; or is this up in the very high region above the ozone layer?

VOICE: I don't think it went up that high. Thirty-five miles.

KELLOGG: Then they'd be getting into a region of steep lapse rate again.

I've had to compress it because of the size of the blackboard. This is the 35 miles here, and then we get a decrease of temperature with height, and we have very good evidence for a lot of

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KELLOGG: (continued) mixing turbulences up there. This is way higher than we're concerned with for even IVY MIKE cloud. The point is, that we don't have direct evidence for diffusion rates in the stratosphere yet, and we could get evidence if we could get balloons up through the cloud and track it for even a few days, as is suggested here. The purpose of mentioning it was simply to get ideas from you people as to whether it was practical or not. So far, I think the only real objection which has been raised is that there might be some other way of doing it more economically, because the idea of a large network of balloon soundings is rather horrifying, in a way, until one compares it to the cost of flying aircraft, which would have to be jet aircraft, up to these same heights, which I think would be equally great. The sampling people will object to the fact that we are trying to telemeter the information back and are not making an attempt to find out what the material is like.

It is, in principle, perfectly possible to recover samples from balloons; that is, to send sampling gear up on a balloon. Weights as much as a ton have been carried by General Mills polyethylene balloons up to 90,000 feet. We'd like to go higher. We could, presumably, if you want to cut down on the weight to a few hundred pounds; we could get up to a 100,000 feet with these big balloons. Therefore it does suggest that if we wanted to find out what sort of material is in the stratosphere one could also use balloons in this case. First of all, getting

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KELLOGG: (continued) one particular balloon with sampling gear through the cloud, I think would be rather difficult, but perhaps not insurmountable, because it is possible to predict where the balloon will go. If one had a number of launching stations one could alert the launching station whose trajectory would intersect the cloud. The problems of recovery are understood; they're not easy, but cosmic-ray equipment and other equipment which has been flown many, many times, have been recovered. It involves tracking and recovering the equipment off the ground by a land party.

I want to throw the discussion open, now. I've seen a lot of people, some of them nodding, and some of them shaking their heads about these various suggestions. I would like to have some comments from some of the people here who may have ideas.

LIBBY: How far do you think you can track with Geiger counters? As I understood you, you were going to hand the Geiger counter on the balloon. How many days can you follow it before it gets too dilute or weak?

KELLOGG: I haven't measured the corresponding calculations for Geiger counters, but I would imagine that if one had an anti-coincidence arrangement to eliminate cosmic rays, one might be able to use Geiger counters even further than conductivity measurements, because in this case one has a possibility of separating out cosmic ray counts from the radioactive counts.

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WYCKOFF: They measured the SANDSTONE clouds over Washington, using Geiger counters and balloons.

EISENBUD: What is the percentage of recovery of these balloons?

KELLOGG: They had a project called MOBY DICK in the Air Force, and the original MOBY DICK gear were rather expensive and they wanted to get them back, so they put $25.00 recovery prize on it. Out of all the ones which they know came down in this country, they recovered every one. Every one was sent back, either by their sending out a search party or just being mailed back by some farmer. I think this consisted of about 20 boxes. Now they know some of them went down in the ocean, but the ones they know landed in the country—they got every one back.

KRAMISH: What was the air sampling achieved? How was the collection made up?

KELLOGG: That's a good question, and we talked about that yesterday. The question was, "How does one go about air sampling from a balloon?" Some of the suggestions were—since we were talking then about the small particles in the air being the current problem—what is the real distribution of particle sizes in the small range? It was suggested that one could use something like an electrostatic precipitator, as was developed by NRDL for aircraft use. Or, in this case, one has the time to sit up there and use a thermo-diffusion separator—that one could even imagine using this to collect these very small sizes.

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KELLOGG: (continued) The third alternative, which is, I think, obvious, is that one could pump up a tank with air and collect the whole sample. Whether this would bring you back your particle sizes in such a way that you could find out anything about them, I don't know. Presumably, be keeping to the sides of the tank after bringing them down.

CADLE: I'd like to point out that the thermal precipitater is called the thermal diffusion device, and I think the fastest break of sampling that has been achieved to date is something like 30 cc's per minute. So it's terrifically low; or, in other words, it's only really effective for very high concentrations for particles. I don't think it will be useful for what you have in mind.

KELLOGG: Supposing one imagined a constant-level balloon leveling out at the altitude of the cloud and running one of these things for several hours.

CADLE: Even at several hours at 30 cc's per minute you still haven't got very much.

EISENBUD: Well, they would be better off using the power requirements authority to run an electrostatic precipitater. I think it would be much more economical to handle. Filtration would be out also, because of power requirements in order to get air through any kind of filter.

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KELLOGG: And another thing, a balloon which hangs in one place tends to exhaust the air around it. I think you'd be recirculating air after awhile if it was a constant-level balloon sitting in the cloud. The other alternative is to do the sampling as you go up; in this case you will avoid any possibility of sitting in your own cloud of already sampled air. So you would need a little faster device for a moving balloon.

EISENBUD: Others have sampled air through an electrostatic precipitator with about ten pounds of equipment at a cost of somewhere around $200.

HOLZMAN: Will, it seems to me that the problem isn't focused too well here. You're not really trying to follow this cloud, are you; you're trying to find out how much activity is up there and what is the rate at which it falls out—isn't that the real problem here?

KELLOGG: I tried to; if I'd been writing, I'd have made a new section heading. The first section heading was, "Following the debris to determine the density in space over a period of time"; the second was, "The possibility of using balloons to bring back samples". I consider this as really two experiments, though. My first interest would be the first that I mentioned; that is, using some method for tracking the density of debris, regardless of what it is made of.

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HOLZMAN: I think that first problem can be handled without balloons. I don't see why you can't do it the way we have done it in the past—by meteorological winds. The latter problem—that is to find out how much is really up there after a long period of time and the rate at which it is being scavenged—it seems to me is the real problem.

KELLOGG: Of course your wind analysis doesn't tell you anything about the density; it merely says that a part of the cloud is started here, if it was all there would now be here, and what didn't fall out or diffuse would all be over here. Nobody objects to trajectory analysis to demonstrate that. What we don't know is how it moves in the vertical when it is following the trajectory, and it was suggested that we do have a very complete cut of the surface now, to show the density in a long two-dimensions at the surface. We really don't know what path is followed by the debris in getting to the surface. We can only work backwards, using a columnation of trajectories and guessing about diffusion. It's generally a rather poorly controlled experiment; we can only use our winds plus an observation of one plane, in this case the surface. I would like to see observations in 3-dimensions.

COONS: This observation in another layer of the atmosphere seems indeed a difficult one when you consider you are using balloons and having the difficulty of having them where we want them.

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COONS: (continued) Comparing that to the data that were on the board yesterday, showing the figures of maximum and minimum for fallout taken on the gummed paper, it seems to me like the range was terrific; even with the number of stations—40, 50 stations—it ranged from thousands down to tens. You have the same problem in the atmosphere, now, but you're only going to be able to launch so many balloons; you're only going to be able to do it for one instance through any one level, for a short period of time at one level. It seems like we're biting off a tremendous program of balloon sampling which might never give you an answer of great significance.

KELLOGG: As I understood, the reason for these big peaks in concentration at the ground was due to the conjunction of the cloud and the proper kind of rain storm, which resulted in the very high concentration at the ground. I would expect that there would be changes in the atmosphere, as Jim pointed out there would not be homogeneous mixing.

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KELLOGG: As Jim pointed out yesterday, there would be discontinuities in the free air because of the way turbulence acts, that is the finite size of the eddys involved. But I wouldn't expect to have the big differences which are due to the big differences in rain. When looking at the Thunderstorm Project reports, where they have the very fine network of rain gauges, you remember the extreme gradiants of rainfall in the rather small area that occurred there.

WEXLER: These are the eastern Massachusetts results someone spoke about the other day - showed a terrific variability of radioactivity in a small area, eastern Massachusetts -

VOICE: On the ground.

WEXLER: - expressed in terms of counts per liter of rain, as I understand; now that indicated a terrific fine structure of the distribution of this cloud which is exactly what Jim Edinger illustrated in the first slide we have shown. And the thing I'm at a loss at you understand, Will, is how you can with the present network, open network, ever hope to get this fine structure that is shown up by means of the ground samples and also from what you'd expect of Jim Edinger's argument. So therefore, if you can't get the fine structure, you have to be content with getting some crude approximation as to, say, whether most of the activity is within layers likely to be

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WEXLER: (continued) affected by rain if rain should occur, or above such rain-bearing layers. How would this information be of value to you?

KELLOGG: Yes, I think it would be, because although one would not know the fine structure, I think that we could perhaps get an idea about the fine structure by just one aircraft flight as to which — looking over the aircraft flights to get an idea of whether there was fine structure or not. I was impressed by this more or less orderly rise to a peak at some region and fall on the other side as they pass through the region.

WEXLER: But even then they grossly smooth over the real fine structure, I understand, by the sampling technique, taken over some time— a long time period.

KELLOGG: But it shows, though, that the size of the cloud, including the wings of the cloud, is large enough to be picked up as it moves through a network of the density of our present radiosonde network. And we would have to forego the fine structure — and the other point I made earlier was that I have a feeling that the fine structure on the ground in rainout is largely just a reflection of the discontinuities in the rainfall, rather than the discontinuities in the air to begin with.

WEXLER: But Will, that is not borne out by these Massachusetts samples which are, as I understand, all reduced to the same amount of rain, over a small area in a situation supposedly uniform rain.

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VOICE: Mr. Bell?

BELL: Our technique was, since we are a rather poor project — financially, I mean — I built with my own hands a bunch of funnels out of sheet metal and put them in - staked them out above jugs; and in some cases the jugs would run over, and in some cases they wouldn't. But these counts I'm giving are counts per minute per liter of rain.

VOICE: What were the variations?

BELL: It was sixty-five samples — of about ten samples — sixty-five times.

VOICE: What was the range of the rainfall variations?

BELL: Well, that's kind of tough, because I didn't try to measure the quantity of rain. I took that from those taken at the weather bureau and other people around there; because as I understand it you can't just go out and stick one collector out and expect to get any quantitative results. I don't know about that. I figure a tree or something nearby would change the reading.

WEXLER: You want to put it into a place where trees and other things won't drift into it. You're fortunately located in Massachusetts. You have a very excellent weather radar at MIT there which could give you some indication as to the uniformity

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WEXLER: (continued)

of the precipitation echoes in the horizontal direction, and you could see whether that is a more or less uniform precipitation. That recalls something in my mind I heard about Brookhaven Laboratory also this spring. They made measurements of radioactivity in rain, and for the first part of this storm there was nothing, and then at the end of the storm it was a matter of high counts. And there they figured that it was just a conjunction, that the radioactive cloud came into the place with rain to them bring it down. It was a rather sharp thing — rather sharp mountains there — which again indicates the very fine structure that would probably be grossly missed by our present coarse network of radiosonde stations.

I OGG: Supposing you had a layer, Harry, of radioactive material like the kind of layer that we see when a smoke plume comes out on a stable day and spreads in a big flat layer. Supposing as the Thunderstorm Project found — and you can comment on this, Dick Coons — as I recall seeing the results - I haven't looked at these systematically, but just looking at the various pictures that have been published, there seems to be a great variation in the height of the rain rises in a given - even in a rather small - area where we have many cumulus around. And one cumulus might reach up into this layer and another cumulus might not.

WEXLER: That's why I say I think that if you could get out some rough measurements as to whether your radioactivity was within the

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WEXLER: (continued) range of your rain or above it, that would be some useful information. But I don't think you're ever really going to get, to use your words, a three-dimensional picture of the cloud in all its fine structure. I think that's just beyond our present network's capability.

COONS: If you had such a picture, you still wouldn't be able to apply it, because then really what you need would be fine scale structure of rain, which, well maybe it exists from small rain networks, but

WEXLER: Well, radar could give that to you. Then I don't know what you'd do with it if you did have it, even then. What, really, would you do with it? Maybe I'm putting words in your mouth, but did you have something in mind of comparing the distribution initially in some column with the distribution finally — after a certain time interval — to see then what might be the vertical motions? Yes, but then if that were the case, how do you — that's the same column you are dealing with if there is all sorts of shearing motions that take a parcel from one column to a different column

KELLOGG: I don't mean to imply that it would be an easy analysis, but it would be a very difficult one — exceedingly complicated. We do know which directions the winds went. If we have a radiosonde network, we know where the debris was initially from observations at the test site. We can put these together

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KELLOGG: (continued) and the other factor is how fast does this thing mix? This is the one that we don't know about, and it seems to me that we could use our methods of three-dimensional plotting which meteorologists are accustomed to using to find out how this spreads.

WEXLER: But you speak about mixing, Will. Would you be able to distinguish between fallout and a turbulent diffusion of gas?

KELLOGG: Well, for an air burst there is virtually no fallout, as far as I can make out. That is, there are none - no particles large enough to really account for any gravity fall.

WEXLER: Then it would be a gaseous problem.

KELLOGG: Essentially gaseous. We have 20 micron particles and down — 20 microns has very small fall velocity. You get down to 1 micron particle which, it has been suggested, is probably close to the peak of the number density curve, then we would have no fall at all, measured in a matter of days.

SOLOMON: Do you get any radar echoes in any frequency from these clouds?

KELLOGG: From the radioactive cloud?

SOLOMON: Yes.

KELLOGG: Only very initially when it's highly ionized, as far as I know they can't after it stops rising.

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WEXLER: Well, as I see this problem, Will, you measure a certain distribution of stuff at the ground and you'd like to know how it got there. You know where it started; you know the distribution is the United States, and you'd like to fill in the intervening mechanism that brought it down. And you say this is an air burst, so we don't have to worry about fallout. What then are the mechanisms that you can invoke? You would like to invoke turbulent diffusion of which admittedly we know very little. There is some disagreement as to the intensity probably, differing by orders of magnitude.

KELLOGG: I'd like to say that we'd particularly like to know this about the stratosphere which we could obtain from tracking the big clouds in the Marshall Islands.

LONG: Could I get clarification on that? I don't quite see why we particularly wish to know this in the stratosphere for this reason - it doesn't seem to be terribly consequential to the local fallout problem and, for the world wide fallout, it seems to me one can say, well, this dust is going to be distributed extremely broadly and there's good evidence that it comes down very slowly. I can see where meteorologically this might be interesting, but I don't quite understand where it fits into the Sr^{90} problem.

KELLOGG: Um hum. Well, as I said earlier, I'm not ever ashamed of giving a meteorological reason for wanting to find something out; how-

KELLOGG: (continued) ever, I think there is practical justification for knowing how fast the material will diffuse in the stratosphere because if we are considering — a lot of the argument yesterday morning centered around whether the material did diffuse rapidly in the stratosphere or whether we could consider it as trapped there. This would be a direct measurement of whether this was true. Now for the local fallout, I agree, we're not so concerned with it because it is the lower diffusion that we're interested in for the local problem.

WEXLER: I haven't finished my statement yet. Well, I mean, if I were given such a problem the first thing I'd look for, and it is an airburst, I'd look for a rainfall map and see if there's any coincidence of high rainfall with high counts, and if there were, I'd be exceedingly puzzled, because this is not supposed to be particulate matter brought down. And then I'd say, well, this shows that this Langmuir collection theory does not apply to this problem. I'd ask myself "Well, what else could bring it down if there's a conjunction between high counts and high rainfall?" And then I would look into this air entrainment business, that is, air getting entrained in the rain. I don't know of any other mechanism that would do that; that is, as far as frontal precipitation is concerned. As far as convective thunderstorm precipitation is concerned, it might be this terrific turnover along the lines that we discussed yesterday. But it apparently takes

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[illegible]LER: (continued)

place from the fact that temperatures at the ground drop down well below the levels they're supposed to drop down if the air stays the same and if the air was cooled by evaporation and falling rain. Well, but suppose, on the other hand, you did not get a coincidence of the high counts with the high rains. Then that would eliminate the rain as a possible important factor and you'd have to look for other mechanisms not connected with rain. Well, then I would invoke, first of all, large mass movements of air vertically on isentropic surfaces. You may not have to go through a detailed isentropic analysis; you may just make use of general meteorological considerations of descending air connected with anticyclones and things like that. In that way I'd try to interpose possible mechanisms, depending upon how the data in each particular case seems to agree both meteorologically and radioactively. To aid in the interpretation and to aid in putting forth reasonable mechanisms, I thoroughly agree, Will, that it's just as important to have vertical traverses as horizontal traverses, which we're now getting. The vertical traverses are necessary, but I don't think that they'll ever give the fine detail that is covered by the words "the three dimensional picture of cloud." I think i7 will just be an extra bit of information to help us decide whether it was possible for this cloud - radioactive cloud - to have been caught in the rain area or not, and things like that. I don't think it will throw any light quantitatively that will improve our understanding of vertical diffusion.

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WEXLER: (continued) Maybe it will be a residue sort of a thing that we can attribute to nothing else, but diffusion, but somehow I feel that quantitatively it will not improve our knowledge of turbulent diffusion. In view of the fact that I still maintain that in a stable atmosphere it's probably very much lower than the other processes that are spoken about with isentropic motions, and in an unstable atmosphere much lower than penetrative convective processes such as thunderstorms.

LIBBY: You would say then, in view of the sort of thing that Mr. Eisenbud's doing and Machta is doing - and also collect more rain?

WEXLER: Yes, and I go along with Will's recommendation except I don't think I'd dignify it by the word experiment; just say, yes, just as a continuation of the present observations - which is mostly in the form of horizontal traverse by aircraft - make vertical traverses by any means whatsoever - aircraft or balloon - as they become available.

HILL: This is for clouds - not necessarily for stratosphere, but for ....This is not necessarily for radioactive clouds in the stratosphere, but for the lower ones which . .

WEXLER: No. This is the whole problem. That's the fallout we're discussing.

KRAMISH: Given a radioactive cloud placed in the stratosphere, does it ever achieve a uniform distribution, and if so, how long?

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W[illegible]LER: Is this relative to the present problem? If this radioactive cloud is within the stratosphere, is it a close-in problem - if it's gas?

KRAMISH: No, this is the point applicable to the long range problem.

KELLOGG: It has to do with whether you would want to get up there with a balloon or not and measure it.

WEXLER: I would say this - that if this is an airburst and you get gas only, and if you know it is released/ well within the stratosphere, then I would think that Eisenbud's collection net at the surface of the United States would give you good indications as to whether it's possible for that stratosphere air to get down to the surface at such close-in distance. That would be a useful bit of information - if we can positively identify that information with what he collects with that particular test.

LIBBY: Well look, if we go as I suggested the other day, to Chile next Thanksgiving and find the stuff coming into rain, doesn't that prove that the stratosphere has radioactivity in it?

WEXLER: Not necessarily.

LIBBY: Why not?

WEXLER: Well, let me ask you this. Would you know initially whether the stuff was contained within the stratosphere completely to begin with and not below?

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L AY: I don't know much about meteorology, of course, but it seems to me that by the time next Thanksgiving comes around it will have rained out all this stuff that's down below in the weather layers. Anything else that comes is then leaking from dense. . . . . .storage upstairs.

WEXLER: Why do you think you've rained out everything down below?

LIBBY: Well, that will have been six months since...

WEXLER: But rain is a pretty spotty thing.

LIBBY: Oh, you think the washing time for the atmosphere might be longer than six months? I was assuming it would be shorter, to wash out all the dust.

WEXLER: Ask the advice of the meteorologist - my feeling is that it is by no means certain, but everything in the troposphere is completely washed out in six months. What do you think, Ben?

HOLZMAN: Well, I feel as apprehensive as you are. I don't think you can state for sure.

LIBBY: Well, let's put it another way. How can we tell? It would seem to me that there's a washing out time for particulate matter in the lower layers. Now we don't know what it is maybe, but this would be a thing which would certainly not be over a year, would it? It would be shorter, perhaps, than the time for stuff way upstairs to come down and diffuse

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LIBBY: (continued) through into the weather layers. If that's true, it would be shorter; and so, if you continue to make these observations, you're going to continue over years to see radioactivity come out in a more or less uniform fashion all over the world.

WEXLER: Now let me ask this, Dr. Libby. Is it possible for the stuff once it's been deposited on the ground to get swept up to the atmosphere again?

LIBBY: I should think not.

VOICE: Why not?

LIBBY: How could it?

VOICE: Attached to windblown dust, or something.

LIBBY: Oh, but isn't that very minor as compared to — I mean, the chance of a given dust particle being picked up and put back into the air is very minor indeed and I imagine...

WEXLER: I don't know, we have terrific dust storms, as you know, in certain areas in the world.

PLESSET: I think it also comes back to the question of the observation Ben Holzman made between what's way upstairs and what's intermediate. Is there a continual mixing at all layers — those that are closer mix more and those that are farther apart mix less? Can you really sharply distinguish between what's upstairs and what's intermediate?

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LIBBY: Well, probably not, but the real question confronting us is whether we're going to have radioactivity raining out for the next twenty years even if we don't shoot bombs. Isn't this the real question?

WEXLER: And you're thinking of the stratosphere as sort of a dead storage and that the atmosphere can almost draw...

LIBBY: It diffuses almost molecularwise, I mean, very slowly; very small diffusion coefficient comes down and then gets into the weather zone and gets rained out.

WEXLER: Well, I wish we could think of one thing at a time. I thought we were talking close-in problems to begin with, and that's a separate mechanism, and what not. Now if we're going into the world wide, long time, long range.. that's another problem. Why don't we finish this first problem?

GRIGGS: I don't think you people who are arguing are differing in any respect. You want vertical traverses, and he wants vertical traverses.

VOICE: That'll be a matter of language, but we'll return to it after this.

KELLOGG: Yes, I think the point is, are we talking about local or are we talking world wide? Actually, what I intended to talk about was a proposal for an experiment to find out where the material would be, and the application of the results to both problems, I think, is rather evident.

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COLE: Incidentally, it may be pertinent to point out that this business of the dispersal of dust might become quite important. At least the air pollution boys, those that are concerned with such measurements as rate of dust fall in the city and this sort of thing, are sometimes quite concerned by the fact that what they are measuring may, that they may be measuring the same thing several times. In other words, the dust falls and they measure it in a spot, and it is redispersed by a windstorm and comes down again, and they've measured considerably more dust fall than is actually produced in the air.

LIBBY: I don't know why, but it strikes me as unlikely that a dust particle would ever get up once it's set down.

WEXLER: Oh, boy, you ought to get over to the dust bowl area when it's really blowing.

GRIGGS: My recollection is that the measurements that have been made in the very dusty regions of the Las Vegas area indicated that the secondary pickup of dust had been extremely diluted.

VOICE: Yes, but it may still be of an order commensurate with the leaking out of the drip out from the stratospheric storage.

VOICE: Oh, no!

WEXLER: Well look, suppose you had a real good rainout that brought stuff down, I mean suppose some of the stuff was completely... and then it dried very rapidly and then along came a good wind and

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WEXLER: (continued) picked up essentially everything that was, and would diffuse, and all that..

LIBBY: Well, most of it, three-fourths of it is ocean and you agree that it'd get in the waves..

VOICE: That three quarters of it.

LIBBY: Now then, you have inland lakes and rivers, and green shrubery, I just don't think very much of it could get back, even if...

VOICE: Well, could it get back to salt particles?

LIBBY: I think it's a one way street.

WEXLER: Well, I'm very glad that you've eliminated one possibility...to your satisfaction..

COONS: Well, getting back to the main problem, it's a general problem, to find out if there's any storage in the stratosphere.

VOICE: That's right.

VOICE: You yourself have recognized the possibility that over a period of time, if there were storage in the stratosphere, it would be distributed over the whole globe, or certainly over the hemisphere. Why do we need a big program consisting of samples from many places to find out if this is true? I should think that one traverse through the stratosphere with the proper instrument would determine once and for all if there were storage. Starting

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VOICE: (continued) right now..and starting later on, in the next big shots that are scheduled would seem simple enough. Now Pete Wyckoff has done this, but I don't think he wants to put in too much credit for this experiment, because they just were conducted, and not for this purpose. He didn't find anything last Thursday. This is the beginning..

PLESSET: But you need to know the leakage rate.

VOICE: Oh, sure, maybe you have to do it two or three times.

VOICE: How high could you go?

WICKOFF: Seventy thousand, and we have gone up to eighty-five.

HILL: Have you done any calculations to see if this was the kind of activity - what you would expect from...

VOICE: Say, what was a reasonable fraction of Mike here and compare it with that normally likely to be there.

HILL: You're working against some kind of a background.

VOICE: Yes.

WICKOFF: Well, it seems to check pretty well with what we would expect from cosmic ray values -- any deviation it was within the instrument error, so if there was anything up there from Mike, and there undoubtedly is, it was much smaller than the cosmic ray value.

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VOICE: Wouldn't it have been smaller in any case? I mean, what sort of instrument did you use?

WYCKOFF: Conductivity meters.

LIBBY: Don't have any checking for the sensitivity.

WYCKOFF: It's quite sensitive. We were able to chase the GREENHOUSE cloud for instance, for three days, and get very positive traces of the outline of it, and other people have chased it much longer.

KELLOGG: This was in aircraft?

WYCKOFF: In aircraft. And this was from the fallout only, not the cloud itself.

LIBBY: And that might have been a thousand square miles big at that time?

WYCKOFF: Oh, yes. Well, on the third day, for instance, we were unable to get around it, it was much larger.

LIBBY: Well, you have two things on the Greenhouse, on the Mike, then? You have the fact that you were three days, and the Mike is now, whatever length of time it is, quite a bit less, radioactive — and according to our notions would be pretty well distributed say, over the earth; I wonder if you could have detected it?

WYCKOFF: I question if we would have been able to find it much below our level of detection.

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__SENBUD: Did I understand Dr. Kellogg to say yesterday that it didn't require dps per for something like a hundred cc of air?

KELLOGG: Yes. There would be one disintegration per 10 cc to equal cosmic ray background.

EISENBUD: That's an exceedingly high concentration.

BACHER: I should think that it is pretty big.

LIBBY: If you really spread it through the air and figured it out, it's about 10^{23}

BACHER: It's 1,000TH of cosmic ray background.

KELLOGG: Which is 1,000th?

BACHER: The Mike cloud.

LIBBY: So it couldn't be checked, but it's still an important question, even if it is only 1000th of cosmic ray.

HILL: What you need is a sample, a large volume of air for radiostrontium ...the most practical thing I can see is rain sampling on a global basis, in periods when they haven't shot bombs for a long time, so we ought to get busy and start sampling all over the world more or less right away, until the CASTLE series starts.

KELLOGG: Well, I think, there are two things I'd like to bring in here. The first is to go back a little to the session of how long would

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KELLOGG: (continued) radioactivity stay in the air. I have here an NRL report which some of you have already seen published in the Journal of Geophysical Research. The NRL people made studies on the concentration of natural radioactive decay products in the atmosphere, and came to the conclusion after their measurements, and after making certain assumptions about the rate at which they were produced, that the "mean life of each substance due to non-radioactive loss," that is due to some sort of scavenging or removal process, "was about ten days." I think that answers the question of how long radioactivity, once it got into the lower layers, would last.

WEXLER: How low?

KELLOGG: Well, their observations were made at the ground, but presumably on decay products which were produced throughout the atmosphere.

VOICE: Don't these things come from the ground?

KELLOGG: This is not a cloud, these are natural radioactive...

VOICE: Isn't this radon from the ground, going up?

KELLOGG: (?) Yes.

KELLOGG: Radon from the soil and it's decay products.

~~LULEJIAN~~: (?) Any idea how many readings were taken to come up with a statement like that?

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LULEJIAN: Well, I talked to the man who wrote that, and he was agast at the fact that this might be used for the world wide problem... it might be the same way with Thomas and Baily here — I talked with the Tracerlab on that — Shearer, who was in on that and told us about it. Before he left he was somewhat perplexed, and he wanted to get out of that..people were using this for the world wide problem, so I don't know whether we can say washout rate of anything, six months and three months or ten days, ten years — because I believe with Dr. Wexler and Colonel Holzman whether there is any uniform washing out in the whole atmosphere, and certainly that radon is not going to answer it.

LIBBY: Colonel Lulejian, do you think it possible that if we find rain in Chile next Thanksgiving that is radioactive that this is stuff that has been below 30,000 feet all of the time?

LULEJIAN: The answer to that is we actually don't know, so why say yes or no to a thing like that? Certainly it is not based on that report of radar coming from the ground which we have looked at ourselves. We find that it seems to be a function of the surface inversion how much lack of build up there is in this rainout.

HOLZMAN: I don't know why you think that this is an esoteric reason for a storage layer up there. You might think of that as being uniformly distributed and then as coming down slowly. I think this is the evidence of your getting some radioactive washout from one of our previous tests. Now whether it is stored up there or what, I just think it is a sort of scavenging process.

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LIBBY: What we are debating is whether it is worth taking this bucket of rain and what we can learn from it. Now we have seen evidence yesterday that there is to be some fission products we can't account for. Now maybe this is just poor counting, or sampling, but there seems to be some evidence that about half of this stuff is missing.

HILL: The same techniques were used for Tumbler/Snapper as for Ivy, and there is a big discrepancy. If there is stuff missing it is presumably in the air. The most likely place would seem to be high levels, it seems to me, Colonel. Of course, I believe the weather washes out of the bottom 30,000 feet a lot better than it does the rest of it.

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WEXLER: high level storage. For example, it is possible for meteorologists to propose a reasonable mechanism. They can bring stuff down to Chile, in shall we say a week, under proper circumstances and give you rain within a week. That is neither an argument for or against high level storage.

HOLZMAN: I think that it would be interesting to notice that it gives a background information of the distribution.

LIBBY: I think that we ought to make measurements anyhow though; they are easy to make. You have a station in South America, don't you, Mr. Eisenbud?

EISENBUD: Yes.

WEXLER: Make the measurements and then go on from there and see if the meteorologists can explain this particular rainfall, whether he has to involve high level drip or unusual transport across the equator.

VOICE: Can't something be done to make these fallout measurements more useful in terms of rainfall and at the same time take very quantitative rainfall measurements to get some idea of how the radioactivity is related to rainfall intensity--or has this been done?

LIBBY: It has been done.

HILL: He has charts plotted where they have barographs from fallout measurements and rainfall.

EISENBUD: That has been done at Upshot-Knothole. What they did was alert three stations around the country to go out when precipitation starts and collect rain at 20 minute intervals for the duration of the precipitation.

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EISENBUD: Those data should be very interesting.

VOICE: One thing that seems to me would be very important is exactly how efficient is your method of collection. The amount of stuff which is up there varies. We just took 50% out of the hat and this is something that you must be able to control quite accurately. We would have to see what that efficiency is and then we would be able to at least get rid of one uncertainty.

WEXLER: I think that it is very important to distinguish between stuff that is brought down by rain and stuff that is not brought down by rain to be absolutely certain that you have those two categories.

VOICE: And this in periods of quiet when you don't have a lot of local, well-defined, radioactive clouds floating around during periods between tests.

LIBBY: I still hope that we can think of some way of using operational aircraft to get some idea of the stuff being upstairs.

GRIGGS: There is an airplane that will fly nearly to 60,000 feet.

WEXLER: Well, if you can do that and have a sample--but Ben Holzman's experiment won't help because you don't know what height that stuff is picked up at.

LIBBY: That's right. It is so dilute that you have to take quite a large volume to get anything, and it is so old that you have to take quite a lot.

HOLZMAN: I don't want to be used as an argument against Will's proposal but I do think that, say, a balloon measurement would tell us something about how the thing is distributed with altitude and maybe some orderly program where we might be able to get some rates of scavenging or diminution of the activity might be worthwhile. My remarks were mainly to be used

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HOLZMAN: against doing this from an understanding of turbulence and things of this sort. What I am interested in finding out is what the distribution of this stuff is with altitude and how long it stays there. You might be able to have some minimal program of ballon activity measurements which might give you this. Distribute them around the earth or something of this sort. You could get a balloon program, on a very small scale which would give you some idea but the focus of the problem would be to determine how much is up there and what its distribution is and what its loss from the atmosphere would be.

KELLOGG: Did I understand you to mean in a little different way, and it is also I think what Harry meant, that we won't be able to use this data for any fancy analysis of diffusion rates but it will be some direct evidence on where the material goes which we do not at present have?

WEXLER: And if you can just throw additional light as to whether the vertical movement of the stepdown is done mostly be rain or by nonrain. That nonrain is extremely important because if this is stratospheric stuff, it has got to get down from the stratosphere to rain-bearing levels so that you have to throw light on both of those mechanisms.

LIBBY: Would you gentlemen hazard a guess as to the diffusion coefficients?

WEXLER: I lambasted those things yesterday and therefore I have disallowed myself. I just don't believe a number can really express the complexity of this whole problem. Comprising a whole range of something that effects cigarette smoke to thunderstorm. They all enter into this business but I do think that we have plenty of evidence, indirect evidence, that

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WEXLER: the atmosphere is capable of maintaining stratification boosts for long periods of time as shown by moisture which argues against these moves and then at other times it is capable of going through tremendous overturnings. Either vertically, by means of convection, or slant-wise, by means of adiabatic processes.

KELLOGG: I think that we all agree with you on that it is very difficult and runs through a big range of variation.

KRAMISH: We will forego the individual reports of the committee chairman in the interest of getting a discussion of Gabriel started. As you probably already know, the conclusions of the old Gabriel report were that it would require of the order of 5×10^5 or around 10^6 nominal bombs to bring the world up to a "mean lethal" level of Sr^{90}. In the policy and classification meeting yesterday, a calculation of this was redone, and I understand there was some consternation regarding the results. I think that it has been ironed out, and I would like to ask Dr. Bethe to give a short talk on that.

BETHE: Most of us were rather perturbed at the wide latitude of figures which were floating around, and I am not sure whether we really can come to any definite figures but at least we have tried to straighten out some of them. One of the questions which we discussed (this was particularly Lauritsen, Bacher, and myself) is the question of the tolerance dose and lethal dose for beta rays as compared with alpha rays, and we had several things to go by but the argument which we finally decided on was the following. There is an accepted tolerance figure for gamma rays for whole

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BETHE: body radiation which is 300 mr per week and 50 mr per day. Now suppose you had equilibrium of Sr^{90} in your bones, you can ask how many milacuries of Sr^{90} you need to have in all of your bones in order to get that same power of dose. This is a very simple matter of arithmetic and I believe that this is sound because the particles which finally do the damage are electrons in either case so they are particles of low specific ionization, and if you have uniform distribution, I suppose you should get about the right number, except for one thing, namely, that your bone is probably not the most sensitive organ which you have in your body and the tolerance dose is meant for the most sensitive part of your body. I think you get anemia first. If you assume the same tolerance dose of 50 mr per day, then this figure ought to be 10 microcuries of Sr^{90} throughout your bones.

L[illegible]BY: Didn't you have to take the bone volume in this calculation?

BETHE: In this calculation we assume the usual 7 kilograms of bone, and we said that the ionization, the number of ions falling, is in proportion to the mass, which is very nearly correct.

COMAR: I must point out that one figure has been used to go from what you would get for even distribution compared to what you would actually get from the particulate distribution in bone, and this is a value of ten. In other words, this is not a good value but it is the best we have so we would say that the Sr^{90} would be deposited not in all the bone but in about 1/10th of the bone of the body.

BETHE: I suppose that this depends again upon the age of the individual. The way we wanted to proceed, I think, is very close to what you have now been

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BETHE: saying. Namely, in a very young individual who is exposed to a certain level of Sr^{90} you would get a more or less uniform distribution. In an adult, I suppose, you would get deposition mostly at the surface, but if you had the same level of Sr^{90} in your calcium then this level would also exist in that part of your bone which is recently deposited, and therefore you would have to get the same level of Sr^{90} per calcium to get a damaging radioactivity.

SOLOMON: I think that Evans has got some unpublished estimations that there are hot spots of calcium 45 in the bone that are as much as five times the mean deposition. I haven't read the paper, and this is just some gossip I have heard so I think that it may very well be quite an unusual distribution.

LIBBY: The mean free path of the radiation is probably about one milimeter. This is sort of average. This would be a sort of mean free path.

BETHE: Well, you have about two million volt energy losses per gram per square centimeter for fast particles. Now the limit of the beta spectrum is 2,000,000 volts; we assume that the average was 600 kilovolts and 600 kilovolts is not quite a fast particle so you get 3/10ths of a gram per square centimeter range.

LIBBY: Probably a little more dense than water.

BETHE: Yes, so you may get 2 millimeters.

LIBBY: So any structure finer than that is of no consequence.

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SOLOMON: I haven't seen it but I would consider that these are depositions in areas larger than 2 milometers.

LIBBY: What is the scale?

COMAR: These are rather gross structures, on the order of 0.1 mm. There are definitely large spots that you see with your eyes.

BETHE: Anyway, isn't it true that if you have a constant level of activity, constant through your lifetime, all this doesn't matter?

COMAR: This is very true, and if you are talking about that, then assume that even distribution.

BETHE: I think that we can assume safely that radioactive atoms are deposited the same way as nonradioactive atoms because the body certainly wouldn;t know the difference before they are deposited,and from what we heard before, I have assumed that strontium is deposited the same way as calcium and that there is no differentiation. If you are talking about a level constant with time through a lifetime or even level constant through enough time to deposit, let us say 2 millimeters of bone material, then it doesn't matter what the details are.

LIBBY: You say 10 microcuries equals three tenths or an r per week?

SOLOMON: I just don't know enough about anatomy to know what the travecular approach to the hematophylic system involves. Does this go down close to where the red cells are made or not? Because if it goes down close to where the red cells are made, then you have got the problem of inducive leukemia. I don't know but it ought to be taken into account.

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BUGHER: I think, Dr. Bethe, that the problem is somewhat simplified because this is an intricate one which has been worked on by a number of radiologists and the National Committee for Radiation Protection and derived at the accepted tolerance figure for Sr^{90}, which is 1 microcurie, which comes not from the 300 milliroentgens per week but in the relationship of the radium sample, and that has been reconfirmed by conference with various groups from Britain, foe example, and from Canada and has been further adopted by the International Commission so that one could, I think, accept that as the standard to which we are working.

BETHE: Well, I'm sorry I can't agree with you on two accounts. You say that 1 microcurie is the acceptable dose of radium? I thought that you said yesterday that there was a factor five in effectiveness per energy.

BUGHER: There were two standards, actually, and they are not entirely consistent by about a factor of five. The 300 milliroentgens per week refers more to a gamma ray type of situation.

BETHE: I was trying to argue that you should use that standard.

SOLOMON: The density of ionization along the beta track is different than it is along the gamma tay track.

ERNIE: But that is what you measure when you measure roentgens.

SOLOMON: No, but a factor of five or 10 for alpha rays as opposed to beta rays, and this is just because of the increase in the ionization along the track. This also is true for beta rays.

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COMAR: In actuality the tolerance value calculated by the two methods, Dr. Bugher, I think comes out very close if you put in the factor of 10, which is what the group has done. In other words, it comes out almost identical.

BETHE: When you take 1 microcurie of radium and if you then take into account the ratio of energy of radium as compared with strontium, which is about 10, and in addition the factor five for effectiveness of isotopic versus beta topics, you come to five microcuries--which certainly agrees within the accuracy of these numbers.

BUGHER: I am just pointing out that there has been an international agreement on this case on the tolerance figure, and that is the one on which we worked and it is the only one which exists, and the general acceptance is that of 1 microcurie body burden for Sr^{90}.

BETHE: Well, it strikes me as a somewhat low figure.

COMAR: Well, I think that it makes a difference whether you are going to talk about the stuff going into an adult with a bone formed or being formed altogether. As it goes into the adult it only goes into one-tenth of the bone, and you have to take a tenth of your figure which brings it down to one microcurie. If it is formed right from the beginning, why then it is attributed to all the bone, and you wouldn't have to take a factor of ten. It seems to me that it is in agreement.

BETHE: With this I agree entirely.

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BETHE: Only the ythium. That is essentially just the ythium. Well, we took 2,000,000 volts maximum beta ray energy which, understand, is the ythium number and that is about 600 kilovolts average energy, maybe it is 900, but I certainly won't argue about 50%.

CLAUS: Doesn't a good deal depend here on what you want to use this figure for? The accepted permissible body burden of 1 microcurie presumably has the safety factor of ten, so if we are attempting to keep the average load of body burden of the population down to an absolute safe figure, shouldn't we stick to the 1 microcurie figure? If you have some other purpose in mind, then probably ten is more realistive than 1.

BETHE: Well, I wanted to get a figure which could be compared to other figures, which are commonly used other than a figure which is taken more or less by arbitrary agreement.

HILL: Well, there probably is a safety factor of ten in the three hundred mr per week, too, isn't there? This is also supposed to be real safe.

BUGHER: Three hundred mr per week is probably not as firm a standard as one-tenth microgram of radium. That is one figure comparison--that is why we like in these internal emitters to go along the one-tenth microgram radium standard rather than the gamma numbers.

BETHE: Now as far as a nonuniform distribution is concerned, I think you will be all right as soon as you talk about a level of strontium relative to calcium which is being absorbed rather than about total amount of strontium, and I think it is important to try to eliminate the compounded

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BETHE: factors of safety or uncertainty which you bring in by first calculating one thing and then another and forgetting what you have calculated before, and that is why I would like to calculate a uniform distribution and then ask what is the ratio of this to total calcium, and this ratio to total calcium should be all right even if we have a nonuniform distribution.

SOLOMON: But if you consider the ratio of strontium to calcium there are three sets of figures that are available I think. One is that Comar finds that animals take up strontium at the same rate that they take up calcium. This is one set of figures. Secondly, there is a set of figures in the literature about a couple of other investigators in which it looks as if animals take up calcimum twice as rapidly as they take up strontium. In other words, there is a two-to-one competition factor. Thirdly, there is the data the Krieger has which based upon just the distribution of the normal strontium to calcium ratio in matter as it now exists, there is a factor of 100 between the soil and man.

BETHE: In which direction?

KRIEGER: Soil has a higher ratio of strontium to calcium.

BETHE: What ratio do you get for soil?

KRIEGER: The typical Eastern American soil is the ratio of about 38 strontium atoms to a 1,000 calcimm atoms. In the results of investigators at UCLA, the ratio for adults, that is from 3 years up to 72 years, is about 0.3. The .7 is on the weight basis rather than atomic.

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BETHE: This is 38 atoms then per thousand?

KRIEGER: Yes.

LIBBY: Could I have a few more of those figures?

KRIEGER: For natural water the figure is about 33. For sea water it is about 11. The average for the earth's crust is about 4. The average for plants, that is the legumes, vegetables, grasses, trees, and bushes is about 8.

KRAMISH: Could you also give the data you have on a different cultural group?

KRIEGER: Some Japanese figures are rather incomplete. Asari tested or made assays on 11, what he called proto-historic, specimens. He found from 592 to 2,114 parts per million of strontium. Using the figure of 375,000 parts per million calcium for American human bone ash, the ratio turns out to be .7 to .25. He also reported a result on one present day bone, a tibia, and the result there is 2.2. There is some, there is decided discrepancy.

LIBBY: How many skeletons did he have?

KRIEGER: Eleven, twelve actually. Eleven for that range from .7 to .25.

KULP: Isn't there reason to question those high numbers for the soils natural waters? They all came from that one very early investigation, didn't they?

KRIEGER: Within a relatively recent publication of the Department of Agriculture they still refer to the results of Robinson in 1917 as the best so far.

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KULP Odum got the value for sea water around 10 — which I think is right.

KRIEGER He reports the figure of 9.23.

KULP Didn't you have Knoll's figures?

KRIEGER Knoll gives two values, 8 and 9, for surface and subsurface waters.

KULP What I was getting at, Knoll also ran some rocks — didn't he? — feldspars and things like that? Now, also soil is rock flour and clay, and it is very hard to see why there should be a 10 or a 100 fold enrichment over the rock flour which I think reads 1-2 on the scale.

KRIEGER In the neighborhood of 2 — probably a little higher.

KULP That was Knoll's data and I think the theory was rather carefully done, so that is all the data we have on the soil, but I think there should be a question mark after it on that basis.

KRIEGER It is true that these figures have to be looked at very carefully, at least a derivation of these ratios causes uncertainty in the values of the measurements.

DEHAVEN The natural water would cut right through some of these soil.

KULP You see the water value there, so maybe the soil represents more nearly the water value, and the deeper rocks not.

KRAMISH There is some evidence of that exchange to the sea.

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WESTERN I would like to post some conflicting data. I don't know the source of it, I mean the ultimate source. Odum, from the University of Florida, in 1951 published in "Science" some work that he had done for a Doctor's thesis, I believe, in which he gave data like this. Water into the oceans from the rivers had about — well depending on the dissolved strontium — 2.2 parts per thousand — atoms per thousand — in silt 3.4 and volcanic organs 2.7. Sedimentation from the ocean had a value ranging from 1.9 to 3.4, depending upon whether it was sandstone, shale, limestone, red clay, etc., blue mud, etc.

SOLOMON Is this part to the thousand, or is it atoms per thousand calcium atoms?

WESTERN It is a ratio of strontium to calcium in atoms per one thousand, which I believe is consistent with the other, and he quotes the analyses of some 50 fossils of ocean life from the early paleozoic to recent times, as giving values ranging from 1.4 to 10.5, although only three of these had values greater than 4. It was his thesis that the ratio of strontium to calcium in geological cycles is approximately constant, and that the ratio had not changed greatly over recent geologic ages.

KELP I would like to say that Odum did his work with flame spectrometer, and we did a fairly comprehensive survey just as carbonate rock and fossils using a spectrograph and checked those very closely using independent standards and methods.

WESTERN There are no soil analyses as Krieger says. I talked to Robinson about a year or so ago, and those old analyses that he made in 1914

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and 1917, as far as they know, are the most recent ones that have been made, and I suppose as good as any.

KULP I talked to Dr. Fleisher at the Geological Survey some time ago and he was very skeptical about the wet chemical strontium data.

COMAR My evidence I just put out in self defense is based entirely upon the up-take body animal of Sr^{90} — as compared with say calcium 45, and also the endogenous, so we feel very definitely that they are treated quantitively in almost the same way. Of course if you might have different fractions in the soil, although you think that strontium would occur in the same chemical state as calcium, but there might possibly be a fractionation there, but certainly after it gets to the animal we feel that there is no fractionation.

KRAMISH Perhaps we ought to proceed with the calculations. As long as we have an idea of what the uncertainties are we can discuss these separately.

BETHE The next section — how much available calcium there is in soil. I understood yesterday that this is supposed to be 1 part per thousand — is that correct?

SOLOMON This estimate was gotten by calling up a man whom I have never met at the Waltham Field Station - and saying - if you had to make a guess, what guess would you make, and so it is not terribly good.

KRAMISH The old Gabriel used that.

LIBBY Can't we do better than that?

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LOMON It varies tremendously from place to place. What I was getting after is what appeared to me to be the most valid.

LIBBY Available calcium may be equal to the calcium that goes into grass.

SOLOMON This is available calcium in tilled soil and soil that is under cultivation. It is just like Jerry Hill trying to take Eisenbud's data, the number of places at which you have made the measurement is the small part of the earth's surface.

COMAR This varies tremendously with your location.

VOICE Yes, but we have it figured for average plants. Just use an average — but realize what you are doing.

LIBBY Is this going to affect the number of bombs linearly?

YTHE Yes, sir.

BUCHER Is availability synonomous with exchangeable?

COMAR Of course, you are leading up to another question and that is whether the deposited strontium is uniformly mixed with the available calciumg which I think would be a big question mark. This comes on the surface and the plants you are feeding the roots down two or three feet below, you wouldn't get mixing. I think the stuff is pretty well fixed on the top 2 or 3 inches.

BETHE Wouldn't rain sweep it down?

COMAR No, I think the finding is that calcium and strontium are fixed pretty well to the soil particles and the movement is practically nil. Mr. Larson has some data on that.

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LARSON We have observed the activity from the Alamagordo bombs over a period of 5 years and cannot find any penetration deeper than 2 1/2 to 3 inches — this is confirmed by the plutonium extraction only. That is the gross fission product activity vs. the alpha activity. Another experiment that we did was to take some 0 to 5 micron fraction material from Snapper 7 and leach it with 80 inches of water in a column that was 6 inches in diameter and 2 feet long. After the 80 inches of leaching we were not able to find the activity penetrating the soil any deeper than 1/2 inch, if that much.

KRAMISH The plants that the human will ingest will come from cultivated areas will they not? There will have been considerable artifical mixing due to plowing and other human operations. So we probably have a different situation here where plants can get the strontium.

HILL This is the important area too.

WEXLER As a dumb but daring meteorologist, can I interject myself into this subject of which I know nothing? Lets just take the number of people and multiply it by this figure here to get out how much strontium you want to innoculate each person. Won't that give you at least the minimum number of bombs?

BETHE That would hardly come out to be a little less than 1.

WEXLER That's something I didn't know before.

DR. MITCHELL The figure is of 1 gram Sr^{90} will 150 million people to maximum permissible dose.

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[redacted]

WEXLER Well, you've got a lower limit now. Are you trying to get an upper limit?

BETHE No, I am not trying to get an upper limit, but I am trying to get a reasonable number.

WEXLER Why don't you try to get an upper limit first?

BETHE I don't know how to get an upper limit, however, may I provide you with what I did and maybe you can then criticize it.

DR. MITCHELL You took total amount of Sr^{90} that was produced so far and if we assumed that 99.99 per cent of it was unavailable, it would still bring the total population of the world to 7 1/2 per cent of the maximum permissible tolerance level. It would bring the total population of the world to 7 1/2 per cent, wasn't that the figure you came out with last night? Assuming that after you dropped all the bombs you dropped so far . . . about 9 kilograms and Sr^{90}. Assuming that that found its way into the bone at an efficiency rate of .01 per cent; all the people in the world would then have 7 1/2 per cent of its maximum permissible dose.

BETHE We tried to avoid just that sort of thing. And instead what we wanted to assume was that the strontium which has been produced is distributed over a 6 inch layer. Now this may be a wrong figure.

SOLOMON 7.5 inch is the standard top soil and there's 2×10^6 lb. top soil per acre and this works out to 220 grams of calcium per square meter of crop land.

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BETHE 220 grams. This is your 10^{-3} calcium.

SOLOMON Yes, that's right.

BETHE Then we have used essentially the same numbers and with these numbers what I get is that 4 mg/sq. mile — I'm not sure if that's the number you want. 4 milligrams of Sr^{90} per square mile will give you in equilibrium of 1 microcurie in the bones of an adult person.

VOICE That's in 20 years growth?

BETHE In equilibrium

MITCHELL May I ask you how that comes out to 270 micrograms per acre? Are they comparable figures?

BETHE Yours is higher, I'm sorry to say. This is 150 milligrams per square mile, I think, is what you said.

MITCHELL That's right.

BETHE Now suppose I state my assumption in detail. You have 1 kilogram of calcium in the bone and 1 microcurie of Sr^{90}, which is $5x10^{-3}$ micrograms, therefore this is $5x10^{-2}$ of the amount of calcium.

MITCHELL First of all you are coming to a falacious conclusion in this kind of reasoning. You are going to finally state that the amount of strontium deposited is related to the amount of calcium, and that is only true up to the point of certain optimal calcium intakes and beyond that it doesn't hold. In other words if you start out with a calcium deficient animal and feed him both calcium and

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strontium you get a certain amount of strontium deposit, now as you increase the calcium you decrease the strontium deposit up to a point of optimal calcium intake. Beyond that point the addition of more calcium does not cause a further reduction in the deposition of strontium.

LIBBY What is this minimum? 20% of the strontium?

MITCHELL I couldn't give you a quantitative figure, except the curves for animals show that as you reach the optimal concentration of calcium that the curve of the strontium levels off as far as the reduction, in other words that the curve goes down like this, and its linear up to a certain point and then it starts to level off. The reason it levels off is probably because of the fact that strontium is not behaving exactly the same as calcium. There is a difference and when you get to a certain point in which the body has its maximum amount of calcium being deposited any additional calcium you give in, first of all will be taken out by excretion methods and the strontium at the point may have another type of mechanism of deposition as far as the quantitative relationship is concerned and it would be at that point, for instance, where the addition of more strontium might keep the curve going down whereas calcium wouldn't.

BETHE I'm afraid I didn't understand. I do not understand whether you are talking about the ratio of deposition to intake, or whether you are talking about the ratio of strontium to calcium.

MITCHELL In other words if you run a series of experiments with various amounts

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of calcium intake starting with a low calcium intake and a standard amount of strontium as you increase the calcium intake (and this has been done with radioactive strontium). The strontium goes down until you reach a certain point of calcium intake then this curve levels off, then the addition of further calcium does not cut down the deposition of strontium.

COMAR I think this is a single dose experiment which doesn't enter into this type of calculation, in other words this is sort of an isotope mass effect and that's our old picture of a single dose when the bone is already formed.

MITCHELL Well, I could be wrong but I would certainly think the same thing would apply in a growing animal.

COMAR Not when your mass of strontium is so very small compared to your calcium.

MITCHELL These were tracer experiments.

COMAR This is a single dose. You could do the same thing with calcium 45. If you took calcium 45 instead of strontium 89 you would probably get the same results; that is an isotope mass effect.

SOLOMON I think this is a second order correction. I think that we are on such unfirm ground, I think this kind of a variation is less important than other things that we are considering.

BETHE This is a theory related to what we discussed before and to the point on which we couldn't agree before, namely if you fed strontium and

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calcium in a certain ratio is it deposited in the same ratio or in a different ratio? Now I have assumed that it is deposited in the same ratio, as it is said there is some evidence which was presented before that strontium is deposited in a smaller ratio than is fed. What I am doing therefore now is to make perhaps a slightly pessimistic assumption, but one which agrees with Mr. Comar's findings, namely that is that they are deposited in the same ratio in which they are fed.

MITCHELL Well I thin that statement is incorrect now. To my mind it's incorrect, because it varies depending upon the particular ratio you are using.

SOLOMON All one is arguing about really is the factor of 2. We're in the area of where factors of 10 are already small. It seems to me it comes out to an order of magnitude where the correction isn't very good. The difference between thede two numbers is a factor of 4 doesn't account for that.

BETHE Now I say $5x10^{-12}$ of the calcium is the tolerance level corresponding to one microcurie because it is over all the bones in the body. Then I further say that in one square centimeter of soil, going down 6 inches, which is 15 cubic centimeters, which I call 30 grams of soil; and I say that calcium in this is 30 miligrams available calcium and now I permit therefore strontium to the extent of $5x10^{-12}$ times .03 which means $1.5x10^{-13}$ grams of strontium^{90} per sq. centimeter and that is the same as 1.5 miligrams per sq. kilometer or 4 miligrams per square mile. Now if the area of the earth is 500 square kilometers and therefore you can permit over the entire earth 800 kilograms of Sr^{90}. One kilogram of Sr^{90}

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is produced in one megaton of fission and therefore this corresponds with 800 megatons. Now that is not a large number, that is about 100 times what we have had.

VOECK Is it necessary to take the count of the natural strontium which is available here in the soil here in these calculations or not?

BETHE It is only necessary to the extent that the uptake of strontium and calcium may be different. If we believe say take some average figure from this column. Then it would seem that the strontium, calcium ratio in the soil is about 10 times higher than in the human bone which contradicts Mr. Comar's experiment, and if that were so then you could tolerate 10 times as much Sr^{90}.

TCHELL There is certainly enough physiological data to indicate that strontium and calcium are handled differently in the human body or in the animal boady and that is the fact that at the point of mineralization, in other words the uptake may be similar, but if you take a rate, for instance, and put ih on a real low calcium diet and try to supplement stable strontium for that calcium he will become a ricketic animal; he will develop rickets, the point there being that the animal will absorb the strontium and will put it into the protien part of the bone but he will not mineralize it. Thereis a difference. Whether the difference is out of line completely, I don't know but at least it does suggest that there could be a point in the metabolism of strontium on a completely adequate diet where this type of thing won't hold.

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AUS — I think that's reflected very well in this table here. The strontium calcium ratio is only about from a tenth to a hundred of the calcium strontium ratio in various parts of the earth's surface. From that you should be able to throw in other factors somewhere from 10 to 100 into your 800 megatons, and that's getting to be a heck of a lot of tons.

BETHE — Except for Dr. Comar's experiment.

COMAR — Well I'd like to define again that we're dealing with a little different situation. I don't think these two ideas that are incompatible at all, I'm only saying that at tracer levels of strontium the material is handled in the same way. Its true you can't replace calcium with strontium. Your dealing here with ratios where the calcium-strontium ratio is tremendously high. Obviously if you put an animal on a zero calcium ratio and try to put strontium in it, its not going to work, but your not adding any mass to the natural appearing strontium in the soil here by any of the fallout so that under normal conditions this calcium-strontium ratio is still going to be very high unless you have some tremendously calcium deficient diet and so I think we'll certainly have to believe the analytical values as far as the over all effect is concerned in this thing. What I still say about Sr^{90} and calcium 45 given to animal behaves the same way I think has been experimentally accepted.

SOLOMON — Of course one of these is in equilibrium concentration and the other is going up to equilibrium. These are really quite different measurements. There is not thing that ought to be taken into account and that

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is that in the adults at least only about 25% of the bone calcium is exchangeable.

BETHE But I thought that all of the assumptions which we should make should be that the part which is exchangeable is in places which are close to tissue which can develop cancer. Isn't that right? To the outside of the bone, and therefore, only the level matters and not the actual amount.

MITCHELL I followed the same line of reasoning that Dr. Bethe did except that I did it with strontium instead of with calcium and one of the reasons that I did it, besides the ones I've mentioned so far, is that you assume that this 7/10 of a gram is the total amount of strontium in the skeleton. A growing person deposits an average of 100 micrograms of strontium a day and its pretty easy to calculate that he is getting far in excess of that in his diet, so that there is some selective mechanism in the way in which this stuff is being deposited.

KRAMISH Far in excess with respect to the excess to the excess calcium.

MITCHELL No one knows the connection between those two.

BETHE Yes, but tell me isn't the only number in which we are interested in, or the only two numbers — these two numbers, adults and soil, whatever soil may be? Well the other one I consider from the discussion before as discredited because it is an old measurement. I don't know whether that's right but certainly the new measurements seem to give very much lower numbers.

OVILLE It might be water as well as soil.

KULP Even if you take silt compared with the there's a factor of 10. Is that in your numbers there already?

BETHE No this is not in my numbers. I assume that the same ratio of strontium and calcium exists in the human bone as in the soil, and I further assume that in the soil there is one part to a thousand of available calcium. Both of these numbers may be off.

SOLOMON The things we're recommending will throw a reasonable amount of light on this because out of our pilot program assuming it is done one will measure the soil radioactivity, the plant radioactivity and the radioactivity in human bones. Among other things, we are also going to measure, if it can be done, the strontium content and the calcium content and so one should in a few months be able to move out of the range of fancy into the range of fact.

SCOVILLE Why do you have to get into the radioactivity part of it at all. Why not carry out a program where you measure the strontium in the soil and say in the water and in bone and re-do these and get good values?

BETHE How about measure sewage?

SOLOMON We suggested feces because there is some question sewage sluge contains all kinds of stuff which has gone into the sewerage and is not of human origin.

RAMISH Joe, I wonder if you could just discuss briefly that Japanese data.

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The reason I bring this up is that if there are wide differences in strontium content among different groups our limit will have to be dictated by that particular group. I wonder if we can have some discussion on how good these figures are.

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KRIEGER: I took the same calcium content as I did for the UCLA results, so that's where the source of error may be. The calcium content of Asari's samples may be considerably higher.

SOLOMON: There's a tenfold difference. One guy would have a skeleton and the other wouldn't.

KRIEGER: But what throws some reliability on proto-historic samples is the one modern-day sample that he quotes. He has a figure of 1860 parts per million in this relatively modern sample of strontium, whereas the UCLA figures are 160 parts per million for the fetal samples and 240 for the adult samples. The factor is almost ten percentage of calcium content. I haven't seen the original paper. These are figures taken from ten abstracts. We're getting copies of these papers by Asari from the Berkeley Library. These results were published in the Journal of the Chemical Society of Japan, and the holdings of a relatively small country.

SOLOMON: Japan is one of the places included in our list of places to sample, but of course this doesn't mean that in China the figure may not be six times higher than in Japan.

KRIEGER: It may be interesting to check the strontium calcium ratio in rice. The Japanese diet is probably high in rice and very low in protein.

KULP: I think there are good physical chemical reason for believing

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KULP: (continued) that Dr. Comar's results should be given the strongest weight, namely that in the exchangeable calcium and strontium. The ratio of that exchangeable material would be very similar to what you get in the bones, the reason being that the particular sites on these clay particles are ideal for the ionic radius of calcium and, therefore, as far as base exchange is concerned, you would expect an enrichment in favor of calcium on the surface of the clay particle, which would reduce it to the order of 1 to 3 toward the point 3. Since we have direct experimental confirmation that it is taken into the bone as it is in this source, it seems to me that's the number we should underline.

LOMON: Of course the soil is total strontium against total calcium, and your figures are exchangeable.

KULP: Yes, I'm saying that if we had exchangeable, which is not a hard experiment, that any of us could do it in a reasonable time, but the experimental results I would predict would be closer to point 3 than it would to three.

C. LAURITSEN: Isn't it also true that you have to use a considerably larger figure for the strontium than the amount that actually falls on an arable area on a cultivated farm, because you have to replace all the things that by fertilized system or water you have to replace the things that would pass through, so you're really drawing on the rest of the world for the

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C. LAURITSEN: (continued) strontium when you cultivate a piece of land. These figures may be very much too low.

HILL: On the other hand, I don't know what the source of strontium fertilizer is, but they might not be contaminated.

LAURITSEN: But you need to replace certain things that you take out of the soil.

HILL: If you're replacing though strontium with uncontaminated strontium, this is in your favor.

LAURITSEN: That is quite true, if you do that, but what if the fertilizer .....

[illegible]: I don't know. I just raise this question.

LAURITSEN: Most organic fertilizer, and that certainly would be from other parts of the world, and you would collect the strontium from the area where you raise the food. This would even be true of seaweed and fish bones.

HOLZMAN: Seems to me that this sort of ties in with the rather short-range problem. Suppose all the future bombs are surface bursts or near-surface bursts and then in the fallout, at least the major portion of it comes down, say within 1,000 square miles. You have a tremendous dilution factor when you start talking about dividing by the 50,000,000 sq mi of the earth. It seems to me that all these factors, in a realistic

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HOLZMAN: war campaign, would indicate a very serious hazard in the local area. It seems to me that problem ought to be treated pretty much - even for air bursts. We have every reason to believe we might go into surface bursts and you'd get tremendous scavenging locally.

RAPP: Concentration falls off with radius. You could easily have changes in concentrations that would be several order of magnitude different from some of the other factors that you are taking into account.

MITCHELL: ... include sources from other areas. In other words, the fact that the stuff falls out in an area doesn't necessarily make it a biological hazard. You can prove that by bringing food sources in from other areas.

HOLZMAN: Yes, but if it falls all over an agricultural area such as the European area, and so forth, the plains of Germany and other places, it seems to me on this figure of this one bomb that you mention that could do this, you have a factor of 50,000 times that you can take care of. It's a lot of these loose factors I'm worried about. There is a problem, from a local point of view, for a war campaign, would certainly seem to be very well pointed.

SOLOMON: One milligram per sq mi is what you need. That's not going to be hard to deposit.

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KRAMISH: Is the tolerance dose for say cattle the same as humans corrected on a weight basis?

COMAR: We don't have any indications of the tolerance dose. I'm just wondering - I'll only say this that I've seen some pathology on the fairly low levels, not near the tolerance. I've seen this a year after administration. Of course, this may get worse because you'd expect to see effects at low levels if you can hold it a longer time.

MITCHELL: Economically, most cattle are killed not much over 2 to 3 years, so we don't have to worry.

KRAMISH: Worrying about food supplies probably.

MITCHELL: You'd probably kill the animals long before they had time to develop sarcoma, don't you think so?

LIBBY: These are various Gabriel figures (studies).

KELLOGG: What are these numbers?

VOICE: A-bombs. Nominal A-bombs.

BETHE: Nicholas Smith and Dr. Claus - and what I just calculated was 4.10^6 tolerance, and I think a word should not be said how to go from tolerance to lethal

LONG: May I interject one question - Am I right - just as a layman trying to get a feel for this - Am I right that your figure

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LONG (continued) would go up from 800 or 8,000 if one applied this 3/10ths 3 ratio.

BETHE: If one would, yes.

LONG: Then the general picture seems to me that that would be a reasonable factor to put in, is that correct?

LAURITSEN: Yes, also that there are other factors in the opposite directions.

BETHE VOICES (Challenging figures on board) - No, $4x10^6$ - I'm sorry - 800 megatons

LUIEJIAN: Dr. Bethe, there's a question in my mind as to whether you used 1 μ curies to get that 800 megatons.

BETHE: This is one μ curies and I want to say a word about that this minute. In the case of radium we were told by Dr. Bugher yesterday afternoon that one tenth microcurie radium is considered the tolerance dose and that people have died from 8 microcuries and one person is alive with 40 microcuries. So that something like 200 (?) times the accepted tolerance dose might be the 50 percent lethal dose.

BUGHER: We didn't mention the lethal dose.

BETHE: You didn't, but you mentioned the minimum and the maximum, you mentioned that one person had died at 80 times the

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BETHE: (continued) tolerance dose and that one person had lived at 400 times.

BUGHER: One case of bone tumor, as I recall, was 8 micrograms of radium body burden.

EISENBUD: One case was 1.7 micrograms.

BUGHER: What is the value of that one lady in Chicago - a heavy body burden and is still alive.

EISENBUD: We've got one in New York with 15 microcuries and she's had it for 40 years.

BETHE: So you would say that 10 times the tolerance dose does on occasion produce serious effect.

EISENBUD: Right.

BETHE: If we were to go contrary to the international agreement and took 10 microcuries as the tolerance dose of strontium, if uniformally distributed, then that would mean that 100 microcuries would give serious effects and that would be $4x10^6$ nominal bombs.

MITCHELL: I would like to raise one other question with regard to the radium figure. That is the population risk in the study cases of radium is of the order of 2 billion people your 200 people might very well miss say 1 percent of the 2 billion who have a much lower tolerance than these figures suggest. In other

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MITCHELL (continued) words, the statisticians will tell you how much of a sample you would need to catch these other people, and they might be present in a population at risk of the size of 2 billion in very considerable number. So we don't know, I mean that's completely unknown figure in terms of the very small number of radium cases that are under study.

BETHE: The number of radium cases is something like 200.

MITCHELL: I would say about 200, will you agree to that Dr. Comar. Quite small ...

BETHE: So you could say then that the figure which I calculated here might indicate the point at which you get trouble is 1 percent of the cases. An that would be the definition of serious effect.

LIBBY: Is anything being done about the Japanese people? Did they get any strontium into them?

VOICE: From what?

LIBBY: From the bombing. Was there any fallout?

CLAUS: Merrill (Eisenbud) has some figures that he's not too proud of.

BUGHER: He has bones of an individual of Nagasaki, but there's a question as to whether the Sr^{90} came directly or subsequently by exchange of soil containing fissionable

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BUCHER (continued) material. How long after the bomb did he die - 24 hours?

EISENBUD: There are two sets of samples. The first group of people who died of leukemia, although they were not subjected to the bombing. They came in later in a rescue capacity, say Japanese military, within 2 or 3 days after the bombing. In that group a number of individuals in the last two years developed leukemia. It was thought that possibly these people were subjected to residual radiation. We were sent samples of their bones. We did find some traces of residual radiation in this material. It was in the order, as I recall it, of around a dpm per gram total activity. These samples will have to be looked at again. These may be very significant samples. The other set were two almost complete skeletons, which we got from Nagasaki. These individuals who were about 1,000 to 12,000 from the blast, were badly burned and died in about 20 to 24 hours. They were buried not in the common grave but in the same locality. These bodies were exhumed about a year ago and material sent to us. Those bones assay from 10 to at most 100 disintegrations per minute per gram of strontium 90. It's very puzzling. We've sent for soil samples from those graves. It's very possible that there may have been fall-out here.

VOICE: Dr. Comar told me this morning that if they ingested the Sr^{90} in water you could expect a large fraction of what

VOICE: they ingested to be deposited in the bone in 30 minutes. On the other hand, if we consider the fraction of fission products they would have to ingest to show at this time, it is inconceivable. I's very puzzling but there was definitely strontium 90. We ran a large number of control bones and they were negative.

LIBBY: This was an airplane too so the local fallout was quite small.

EISENBUD: There was enough residual and radiation around Nagasaki to account for total body dose for around 3 roentgens. I don't know whether these people were in that part of the city where the residual radiation was high or not. These data were based on surveys by Stafford Warren and Shield Warren 30 days after the blast.

LIBBY: It would be very interesting to examine the soil in that graveyard.

EISENBUD: You asked whether anybody ever analysed the strontium 90 in the soil. We have found Sr^{90} in the soil.

SOLOMON: What are the assumptions on the other two Gabriel figures? How do they differ from this?

BETHE: Dr. Claus assumed 2,000 microcuries body burden as the lethal dose. Although he assumed a much larger figures here than I have assumed for serious effect, larger by a factor of 20, he comes out with a smaller number of bombs.

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BETHE (continued) This is obtained by different assumptions, namely assumptions of the sort that Dr. Mitchell mentioned before, then you assume that certain percentages of the strontium in the soil gets into the body. It seem to me that it is much more reasonable to talk about levels (?), that is that strontium 90 is a certain percentage of the calcium which you have in you. And I think that by talking about levels rather than talking about percentages of the soil getting into the body, you will arrive at a more reliable figure. No matter whether or not you take into account the collection factor of 10 as we have been discussing so much and therefore I consider this a more reliable figure to go with 100 microcuries.

KRAMISH: There is also a factor of 2-1/2 in the old Gabriel report which we would like to isolate, we can't exactly find out why. They assumed this is the case.

VOICE: This is Smith?

KRAMISH: Yes, all of the people, in writing the old Gabriel report assumed 2-1/2 grams of strontium per kt of bomb. All through the report, this we have been unable to find the reason why, we are assuming 1 gm per kt. Yes.

LIBBY: I think there is confusion here about what fission yield means.

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CLAUS: Also, it was pointed out by Teller a couple of years ago at one of our discussions that whatever appears in these things is simply something chosen by Nick Smith and have just been carried through up until the time Teller called attention to the errors.

HILL: There is a factor of 2 there if you misused the definition of fission yield, and I think there is a slight other factor there maybe on the number of fissions per kt that were used.

LIBBY: This morass of uncertainty I think is the strongest possibl e argument for the experimental program not only just assay but also biological. Is there any feeling on the part of any-one in the house that we should not shoot any more bombs until we find out? This, of course, is very importan conversation.

KRAMISH: I think it would be very interesting to take a poll - a secret ballot - just to get an idea of the feeling.

LIBBY: I don't think that's a good idea.

WEXLER: Ben's argument is very convincing - that is - a factor of 1000.

LIBBY: It is an interesting argument. Remember that the ocean is great cesspool and the natural thing is for the continents to wash into the sea and the strontium will be deposited

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LIBBY (continued) in the limestone evautually and we don't know how fast this occurs, so maybe things are all right, but there is a way in which radioactivity can be taken out of the life cycle and that is by putting it at the bottom of the ocean.

CLAUS: There is a point that I would like to call attention to that hasn't been mentioned yet as far as I know. I find it very difficult to get concerned about what's going to happen to the human race if 800 megatons or 8000 megatons or bombs have to be discharged before we can reach possibly a serious level. On the other hand, if power reactors come into being in any appreciable amount we will have very sizeable amounts of fission products and I once made a calculation which may or may not be absolutely correct - that in only 100 days, if we were to furnish the power requirements for the United States in 100 days we would have used up as much fissionable material as are involved in 10^5 nominable bombs.

LIBBY: That sounds to me too high.

CLAUS: That may not be absolutely correct. I went through the figures a time or two and didn't find any error in it, but anyway something of that order. It will not take an awful lot to create a tremendous possible hazard in terms of fission products if they are indiscretely distributed over the earth's surface.

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LIBBY: Yes, but my goodness, you certainly wouldn't do that.

CLAUS: Nothing has to be done, but they are going to remain on the earth's surface and they are not going to disappear for an awfully long time. And I think the results of studies of this kind will have a great deal to contribute to our final determinations of what to do with waste products from such things as our reactors on a large scale.

LIBBY: Yes, we certainly do want to dispose of them and not let them get loose, but this, it seems to me, is not an extremely difficult problem.

CLAUS: This is quite a serious problem. The question of sea disposal is one which has come to the fore many, many times.

LIBBY: I still don't see why you can't mix them up in concrete and let it solidify and drop it. Doesn't it leach out?

CLAUS: It's just not practical in large quantities.

HOLDEN: Are there significant amounts of strontium 90 released from reactors now?

CLAUS: No, it's in the fission product. I don't think it's released into the air.

VOICE: What about chemical processing for recovering your unspent fuel?

HILL: What's done with the long-lived fission products - do they run into the Columbia River?

CLAUS: No, as of now all the concentrated ones are stored in tanks, but they can't keep on doing that indefinitely. It's an extremely expensive proposition.

BETHE: Is the strontium let out immediately?

KRAMISH: That goes off in the solution.

BETHE: But in that case when   Let the strontium out too.

HILL: It is held isn't it?

KRAMISH: It is decayed already. It is not let out immediately (chatter) and that strontium is in solution and is readily available. Mr. Theis, what's the ideal way to dispose of a bucket full of strontium 90?

THEIS: Well, you know about the experiments at Brookhaven. (chatter) fission -- leach the fission products off on Wilsmite and fusing it and changing its mineralogic composition and rendering it relatively inert, but this I think, Dr. Claus, suggests deep disposal on land as well. There is some question about that but I think it might be feasible. I think, I don't know, the best way of disposing of the material - dry mines have been suggested.

LIBBY: I think that strontium could be gotten into limestone and be pretty safe. Of course, the other fission products would not necessarily be held but the strontium would be held.

CLAUS: Well, if you had your strontium 90 isolated in a bucket then it would be easy enough to put it into concrete and sink it.

LIBBY: I was hinting that since it is the one we're most afraid of - perhaps we should use the disposal which would take care of the strontium.

VOICE: You mean put it into limestone?

[illegible]IS: Insert it into limestone - I'll have to defer to the chemists as to how rapidly that process would take place, but I'd be a little bit afraid inasmuch as water moves most rapidly through limestone.

LIBBY: It might be well to precipitate it, pump it in, or deposit it, but I think the great bulk of the limestone as protecting it against leaching action in water. The strontium is less soluble than the calcium. [illegible].

VOICE: It would be cheaper than putting in in the ocean.

[redacted]

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LIBBY: No, it seems to me the ocean is the ideal place. Just to take it out and drop it. (chatter)

VOICE: Put it into concrete. (chatter) - in concrete blocks (or bulk?)

Claus: That's not practical.

LIBBY: It isn't?

CLAUS: Well, would you object to just pumping it down to the ocean deeps in bulk?

LIBBY: You can't do that - that's not practical - (chatter)

. .P: The comparable price for dropping blocks is many orders of magnitude more   drop it and let it fall down three miles and let it stay there.

CLAUS: The other possibility is to spread it over appreciable areas of the ocean's surface at not great depths, that would dilute off pretty fast, then the question is - you have got all this strontium in sea water - will it work back on the land one way or another to be a hazard?

LIBBY: Well, there is another question too which we haven't brought out here - and that is - if you disturb the plankton, do you disturb human lives? Of course, the plankton are pretty durable as far as radiation is concerned.

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CLAUS: From studies made so far we don't believe that we would disturb sea life seriously by that amount of material if we can get good, reasonably good - dilutation within a reasonable amount of time.

HILL: You don't have to go very deep before you get out of the area where there is much life either, do you?

LIBBY: No, that's true.

HILL: It's also awfully slow mixing.

KULP: How much would it cost to pump it below say about 300 m? I don't understand what the great cost is of making concrete blocks.

HILL: It depends on the bulk that you get your fission products in. At the present it is pretty big.

VOICE: A concrete ball would certainly fall.

VOICE: Don't you contemplate a strontium separation process?

VOICE: No, take the whole thing and say it is pure strontium, since strontium is the bad actor.

VOICE: Well, it is probably sheer bulk.

VOICE: Well, of course, they have the solutions, that's right, and they have to evaporate them or else they couldn't put them into concrete.

[redacted]

HILL: That would take a lot of calories.

CLAUS: Well, even as of now they evaporate down considerably.

HILL: Is there enough heat to do this in the pile to be utilized to do the evaporating?

LIBBY: I would think though that the waste is full that might be predicted on the assumption that the strontium is the thing to be most careful about.

CLAUS: Well, that's one thing that we'd like to find out, and I think we could get a lot of good guiding evidence from a study of this kind.

BBY: .... see how strontium moves if we did put it into the sea. These data may tell you how strontium moves.

MORSE: Well, what is there against burying it in desert country and into within interior drainage?

VOICE: It would probably be negligible after 1000 years, we aren't worrying about the next geological era.

BETHE: I must say that I'm considerably more afraid of an atomic war than of power reactors and I think this is rather evident from the figures that we have written down here, and if you say that a 1000 megatons would give the tolerance level and if you say that MIKE made 10 megatons, you need only a 100

[redacted]

BETHE: (continued) of these to get to the tolerance level ...

CLAUS: MIKE megatons weren't all fission products.

BETHE: Well, more than half of it was.

HILL: I've used the figure 2/3.

SCOVILLE: In MIKE shot only 1 percent of it ever came down that so far has ever been found on the earth's surface.

BETHE: That is true, if you can rely on the fractionation that only 1 percent comes down, then that is an entirely different story.

L: Well, I don't think that reliable over a long term period compared with the half-life. It's still coming down according to Eisenbud.

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LOGG: We have to remind ourselves too, that in the estimates we made on the overall fallout for Mike, there was this big uncertainty about the possibility of falling out within a few hundred miles.

HILL: This assumption you can't forget.

KELLOGG: If this were exploded over land, and this few hundred miles is still over land, I would expect that a good possibility is that an appreciable fraction, say 10%, would fall out in bomb area within a few hundred miles.

HILL: On the other hand if it's an air burst it wouldn't be bad.

KRAMISH: It might not be bad, as an exercise, to choose the worst possible conditions and see what happens to the expected tolerance.

BETHE: If we believe Magee's theory of particle size, the particle size should be larger for a large bomb. If it is larger for a large bomb, then we might get quite an appreciable percentage of fallout, even for an air burst, within a few hundred miles.

GRIGGS:(?) What kind of prediction does this make on the change in particle size?

BETHE: It makes a prediction that the possible size is proportionate to the time scale.

[redacted]

GRIGGS: Edward made some statements about it being 6th root of the yield.

BETHE: Well, we count on Scoville's memory, at least it seems to be a little more than the 6th root and a little less than the 3rd root. It's presumably, perhaps five times as much for MIKE as it is for standard bombs. Isn't that about right? Twenty seconds versus three seconds.

SOLOMON:(?) Would it be desirable for us to suggest a delay in future tests until you can get some better evidence.

LIBBY:(?) We must get busy and work these samples. I think the fears and worries of this group are a very strong argument for the government's collecting these samples. I don't think there's be any useful purpose served by taking a vote or writing down formally, but maybe it would. I don't think so.

SOLOMON: Certainly the pilot samples would be enough to produce most of the undertainties in any calculations.

VOICE: Well, I don't know, that's pretty hopeful, I'm afraid...you don't know what you're going to find.....we may just have to .... like, look at this skeleton over here.......My gosh, who would have guessed that result? Full of Sr^{90} in twenty-four hours!

EISENBUD: The skeleton has between 10 and 100 dpm.

KRAMISH: Are we ruling out completely the inhalation hazard. We are eventually going to have a continual drip of this stuff in the

[redacted]

K[illegible]ISH: (continued) atmosphere. I'm wondering if we shouldn't consider this... No, I'm thinking of a mechanism that Dr. Mitchell mentioned, retention in the hinge, what tolerances we can expect of that.

MITCHELL: I have those figures, but, I also have to have that as possible accumulation in the skeleton.

VOICE: Yes.

CLAUS: Dr. Western has been making some calculations on this for some time, and maybe he can get some.....

KRAMISH: Could you make a few remarks, Dr. Western?

WESTERN: The inhalation hazard is very difficult to estimate like all these other things. One has to make any number of assumptions. One of these things which I think would be important would be the problem posed by Dr. Wexler earlier this morning, that if the stuff came down in rain primarily, or whether it comes down in the air. If it comes down in rain I don't think you'd breathe very much of it. If it comes down in the dry air primarily, I think you'd have a very good chance of breathing all the material that we consider being small enough to be drifting down over long periods of time, so I should like to say in passing that it would be of some importance to determine, in establishing an inhalation hazard, whether it does come down in rain, or whether it comes down in the air.

But if one makes some broad assumptions about what the behavior of the stuff is after it enters the lung. I assumed

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[redacted]

WESTERN: (continued) that one might retain about 5%, retain in the skeleton about 5% what it inhales, and I get an inhalation hazard representing deposition in the skeleton of about the same order of magnitude as I obtained by making assumptions somewhat similar to those which have just been discussed from the point of view of indigestion. I have a factor of difference of 5. I believe these assumptions up here assume only exchangeable calcium don't they? Isn't that right, didn't you use a figure for exchangeable calcium up here?

VOICE: Yes.

WESTERN: And the figure I had was to correspond more to the total amount of calcium so on that basis the inhalation hazard is roughly about 1/5 of that indicated here. We are talking about the same type of hazard so that the question of what is tolerance does not enter in. I might indicate also that if one begins to consider the inhalation hazard as being relatively nonimportant, as one might if he finds a certain number of processes take place, preventing the stuff from being picked up by plants. Experiments of Dr. Larsen show that in time there is an unexpected cessation of uptake so that the inhalation hazard might become relatively important. Then one also might want to consider the hazard due to Sr^{90}. It is commonly assumed that what is inhaled is in sufficiently small particle to be soluble and gets to the blood steam and is deposited in the bone. We don't have the factor of safety here/that we have when we talk about it coming to the body through the food ________ where we

[redacted]

[redacted]

W..EEN: (continued)

have a compounded factor of low uptake both by plants and by the body. So that on can't Rule out inhalation hazard. As long as I have the floor I'd also like to comment on Colonel Holzman's suggestion that we might well be interested in highly concentrated fallout of rain the use of surface bombs. If one assumes that the material...that one is using a surface bomb, and the material falls out within a period of three or four hours, one may be interested in the denial of the use of the land in the future. But a rough computation indicates that the primary hazard to a population living in that area and more or less staying there is from external radiation rather than from something like this. The same amount of material that would be required to give tolerance effect which was computed up here would in the first day give something like 2000 roentgens to an unprotected person on the average. If the material were uniformly distributed over an area that you would have the number of curies of strontium to give this tolerance effect, whatever that figure is - it turns out to be about 5 curies per square mile I think.

Beginning with a period of three or four hours the external radiation is sufficiently high at that concentration that about 2000 roentgens would be the external radiation of an unprotected individual in an open area and during the first week exposure would be about twice that. So that I think that in that particular case, we probably are not interested in strontium or in inhalation hazard.

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LA[illegible]: Yes, but Dr. Western, if you do that, doesn't it mean that particular farm produces lethal vegetables from then on...?

WESTERN: Maybe. Well, the point of the question is probably more serious for the bombing of a large area. If we are talking about where the number of bombs becomes serious. The only point that I'm making is that if you use surface weapons over a farily large area, we're not going to worry perhaps too much about what might happen in ten, twenty, or thirty years.

VOICE: What do you mean?

WESTERN: Well, no, what I'm saying is if you produce the amount which would give you an average concentration of strontium, and would produce a tolerance effect, and this, of course, from our picture would require from ten to twenty years to accumulate in the body; this is only a tolerance effect. If the bones were laid down in such a way that the people who lived in that area were subjected to the fallout beginning within four hours of the explosion they would, during the first day, get something like two thousand roentgens, if they were unprotected in the open, during the first week they'd get twice that. So I'm saying that is a critical aspect, rather an the long-term strontium.

KRAMISH: Well, I think one factor to consider is the ratio of the number of people cultivating that area to the number of people dependent upon the products of that area, who are not living in that area, and I rather imagine in certain agriculture areas this ratio is rather high. [redacted]

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[redacted]

PLESSET: Well, what he's saying is, if you kill all the people in that area...

VOICE: In that area, but I'm worrying about the people who live off of that area.

PLESSET: Not so significant in overall hazard as the primary level.

KRAMISH: I think it might be.

BETHE: We can evacuate the area, and then the story is that they can't return.

WESTERN: Well, to answer your question, Arnold. One is, ...well in the first place, I'm talking only about tolerance concentration of Sr^{90} as compared to extremely high concentration of external radiation, and second, if you are talking about localized areas, you have two possibilities in case of warfare. One is that you're going to use it for agricultural purposes that you can devote it to products which may be less critical than other products, that is, there are a number of possibilities of being able to use it productively at a lesser average risk than we're considering when you have the whole country uniformly contaminated. And another is that if it is a small area, in general the products from it, if they're used to feed a large population will be diluted with products from other uncontaminated areas, so you get some factors of safety there, and as I've already pointed out, you have a number of years to work this out after it happens, and you can do quite a lot to alleviate the hazard.

LONG: Now, there's one fact that I was curious to ask about. I got the impression that in Mr. Bethe's calculation, that the

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LONG: (continued) assumption is made that all of the strontium 90 which falls on the ground is available. One knows that soils have rather pronounced exchange characteristics...I can one guess...so that I would take it that this is a rather pessimistic assumption, and I was curious as to how pessimistic it is. Have experiments been done in putting strontium 90 into ordinary soil and then finding out what is available?

LIBBY: That's what you're doing, isn't it, Dr. Larsen?

LARSEN: Yes. We have been looking at various shots, and the one piece of data that is most complete is on the underground, which, as most of you probably know, is about a 1.2 KT. What we did there was to take soil flats from California soil representing 8 inches in depth, and about 4 sq. feet each box was in this dimension, and we distributed this over the territory of predicted fallout. We came back with half of what we had distributed as contaminated, which we could measure by survey meters. I'll take one, which represents one of the maximum activities to illustrate what we found. We had 196 microcuries total surface activity on 12/17/51, and we have grown 5 crops of radishes consecutively on that and the observed values, for example, on January 15, 1952, was 16.9 disintegrations per second per gram of plant dry material. The last crop came off in 9/19/52, of radishes, and this read 1.42. Now the controls that we had growing on the same soil, but without any contamination, read, in this crop 1/15/52 series, it was 1.69 d/s/gram and over here 9/15/52 we were getting about 2.

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[redacted]

LIBBY: Can we have some of the intermediate crops? Say July?

LARSEN: Yes, observe...this would be 4/15/52 reading at 9.2. At 7/1/52 we are reading 1.8, and the fourth crop was harvested...this is reading about 2.6.

SOLOMON: Aren't your controls just the thing we're trying to measure? I mean this represents the fallout that hasn't been fractionated in any test.

LARSEN: The controls supposedly have never received any fallout, because they were collected from San Fernando Valley.

SOLOMON: Yes, but if the stuff's leaking down all the time.

[illegible]: Oh, but there's radioactivity from natural sources.

LARSEN: Oh, yes, you've got K-42. For all the potassium in the world you have .012% that's radioactive.

VOICE: This was before MIKE.

LARSEN: You have rubidium, which is natural. You have the uranium, thorium series. You've always got that to contend with, and any time you fertilize, why you're adding radioactivity. Now the ladino clover was added or planted after we finished up with the fifth crop, and we took five crops of that off, and I have the last bit of data which came off on November 29, 1952. I beg your pardon. Just this last month. And the contaminated flat was 1.86 dps, the controlled 1.6 dps.

[redacted]

MITCHELL: Was there any final assay of the soil after you got through harvesting the crops?

LARSEN: Yes, we feel that quite significant quantities appeared. I don't have the figures with me right now.

BETHE: The 196 microcuries were distributed over how?

LARSEN: Four square feet.

MITCHELL: How do you account for such a large drop from the first crop to the last one if the radioactivity in the soil is maintained?

LARSEN: Well, by decay curves and energy curves, the only thing that we have been able to pick up here is Strontium 89. It has been a selective absorption, /// apparently. Now we know from other crop data that we have done where we have taken soils, agricultural solid from throughout California and New Mexico and contaminated at the rate of 100 disintegrations per second per gram of soil, there are 1600 grams to a plot. I can give you some idea of what happens here. On the Strontium if we take the soil to the plant and we also try to cover this up with the animal feeding, there are 100 disintegrations initially per gram. The plan in the leaf material which the bean was the most important, the barley was the least important and had 1420 disintegrations per second per gram of plant dry material and if this plant were fed to this animal our experiments have netted 280 d per s retained this would be of a dose fed. In other words, I got these figures from another

LARSEN:
(continued)

experiment we did. 1.8% of the Strontium is retained from a dose of 431,000 d per s fed daily over 17 weeks. Plant dry material grown on contaiminated soil which would give an average daily feeding of about 2700 d per s, that is plant dry material. Cesium is the least important, cerium is negligible, ruthenium may be important because this is a bad actor when we talk about the chemistry of it may act as a cation or an anion. We have studied the fused material from trinity on this sort of a thing, experiment, and we have taken fused material from Snapper 7 and fused material from Upshot 6 and in each case all we can do is to put down nil uptake. At least the instrumentation that we are working with and the techniques that we have used on our research work, as we call it, using those same studies or comparisons come up with this value. Now you may be interested in what kind of activity is immediately available to a plant on detonation. On this last series out where we, along with our fallout studies, we trapped animals, the native rodents and shot the jackrabbits that are in the field. We had good fortune in that there was Upshot 2 went in the north easterly direction and was not recontaminated during the period of our stay in the field. So we did serial sampling on it. In addition we had sampled that area in October '52 and November...I mean September '51. You may be interested in some of the things we found on that. On the rabbits d day plus 8, d plus 22, and d plus 32 days. The casium, lung, liver, leg muscle, and femur.

KELLOG: Would you define the casium?

LARSEN: That is practically all of the GI tract in the rabbit. The biologists object to calling it the GI tract. Now these figures I am putting on the board are disintegrations per minute, per 100 miligrams of ash.

(chart should be used here)

VOICE: What was the initial date?

LARSEN: It was March 24, I believe. Since we weren't such good marksmen in September '51, I couldn't sample lungs. The previous work has all been head shots. If you are dealing with a larger bomb and more activity drops down here your activities are going to go up, but they still hold that same general picture. If you plot the decay curves of, say for example, the activity here and of this sample you would find that the slope of the activity will approximate the slope that is represented by this decrease. The half-life us was last told or mentioned to me as about 32 days. I have some other things that, if I may back up to what we were talking about this morning on particle sizes. It applies to what we are observing here in the lung. We were able to determine on a few of our air samplers this time the actual particle size that was on the air path. This was done by a technique that we have tried to adapt to turbidimetric size analysis on the actual membrane filter and we find that about fifty percent of the activity is

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LARSEN: (continued) less than a half a micron at 43 miles of the airborne material. Now this occurred on a sample that was collected at two to four hours and the same picture held at six to eight hours. The soil sample collected 24 hours after detonation in this same location had the maximum activity in the particle size fraction of 175 to 350 microns. I sort of go along with the idea that if we study intensively what can happen in the first 200 miles after detonation we are going to come up with most of the answers that can be applied worldwide, if you will, or at least within the U. S.

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KERMIT LARSON: There may be some question with respect to the characteristics of a particle — you break it down to that in close versus that which goes out to 2000 miles. But I think this can be ironed out between the program that Eisenbud has and what the off-site people have been doing at Rad Safe and what Los Alamos has been doing and what we have been doing.

If we can find the time, we're going to try it.

LIBBY: It certainly seems to be very important. I notice you finding strontium 89; now strontium 89 has the same dusty character as strontium 90. It also has a krypton precursor that can only be chemically available, so it may be selectively absorbed in your radishes. Not because of the chemistry of the radishes, because its available, and the rest of the fission products are held back physically. This is what we are hoping to iron out. It will be most interesting to see whether this is so.

LARSON: We have work going on in clay fixation that is a study in strontium, cesium, cerium and ruthenium problems and we're not only going with respect to the usual definition of clayification, but we are using a biological indicator to prove whether or not that fixed so-called fraction is really, truly fixed with respect to biology.

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LIBBY: I think your report is extremely important Dr. Larson, that is, if you could possibly manage to measure strontium 90.

LARSON: You see, a program like this, Dr. Libby, we call on all contractors to send us people plus military people and we wind up with something like 58 or 60 personnel out there in the field. When we get all through with the field job and come home, there are only two people who have the job of analyzing all the data that has been collected.

LIBBY: It's possible you see, I think, to get some contractors to measure. I've been hoping that some of these commercial companies would start measuring — making low-level measurements for a charge so you could send a sample to, say Tracer Company X, and get a measurement of it at real low level for a price, or else have equipment which you can buy, or give this service so that it would be possible without having to do it yourself to get a lot of measurements made.

VOICE: You know, your samples are probably not very low level.

KRAMISH: Dr. Western, would you like to have the floor?

WESTERN: I was making a mental calculation a moment ago while I was talking and I was trying to coordinate my data with the values which Dr. Bethe had come up with on the board and

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I got one of my factors inverted; let me make a correction on one of the statements which I had made. I should have said that a value for external dose of material falling out in the concentration which Dr. Bethe talked about first as being a tolerable dose which I think was 8/10 microcuries per sq. mi., I'm using one microcurie per square mile here as being essentially equivalent to it. The external dose as the material falls out within 4 hours is the integrated dose over a long period of time and is 20 r (not 2000 r) and of which half (of which 3/10 or 6 r) the first day and 10 r in the first week. One curie per square mile would correspond to this according to the calculation which I had made previously and have on paper here. I got one of my fractions inverted in making my mental calculation.

If you want to go up to what he considers as perhaps being dangerous there, you would have to multiply these factors by 10,000; then you are getting up to the point.

LIBBY: Consider this point: In France whether you fight this war and you use bombs against the Russians, you win the war; but then, are the farms ruined? In other words, I would say 2,000 r, total dose, would be something like a tactical use where the bomb comes pumping into the ground and explodes low over the troops. To get that kind of con-

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tamination of the ground (100 times this 2000 r) then would that farm be ruined? I think this is Col. Holzman's point and it's a very interesting point.

WESTERN: Well, it depends on the interpretation of the data which Dr. Bethe had on the board. If he puts in a factor of a hundred, then we would say that the large areas of that sort, we would worry about them, they might get results; if they are very small areas, we might be able to use them for something.

COL. LULEJIAN: Sir, in that respect you might be interested in this. There was just one case of fallout which we claim now covered 2500 sq. mi. and we think that from these calculations it deposited 2 curies of strontium 90 per square mile, which would make it something like 2 or 3 microcuries, and the dose rates informally calculated would be as he indicated here (something like 60 r infinity dose) however, the reality of the situation in measuring, this is an actual case. At the maximum there was nothing over 100 roentgens infinity dose. Here is a situation where it falls out and you do get perhaps 5 roentgens if you live outside the vicinity and yet you do have 2 curies of strontium 90 per sq. mi. Now, even if you have no vegetation there and you do put population in, is it possible that you might just stir up dust by walking around and inhale a

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portion of this so that it might be a hazard locally so we would like to more or less disband in this case from Col. Holzman's point of view and actually ask the conference that in the absence of injection and in the presence of depositing a lot of strontium 90 per square mile locally in a tactical situation, is there any hazard?

PLESSET: How is this 2 curies per square mile observed?

LULEJIAN: It was calculated from an observation of what was deposited in the desert.

PLESSET: In other words you were assuming that the normal gross fission products . . . Well, then shouldn't the same reduction factor or something like it be a part of 2 curies per square mile?

GRIGGS: I want to find out what this is . . .

LULEJIAN: It should be 60 r infinity dose as calculated by Dr. Western's point of view.

LIBBY: But you've got a new question: Even though there is no vegetation, is it still dangerous?

LULEJIAN: In the absence of ingestion in the tumorer in vegetation or animal there is hazard if we put, in this case 2 curies of strontium 90 per square mile; but tomorrow, if we have a

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war and we do actually shoot a lot of bombs (possibly multiple drops on one city) this might go up, say, by a factor of 10, and yet no vegetation; is there a problem? In this case it must be the inhalation hazard; we don't eat the dirt; you might breathe it.

LIBBY: . . . how the Japanese study strontium?

LULEJIAN: No sir, that's a different problem. I don't see how he got residual activity mentioned in view of the air drop . . . I don't quite understand that. It could have been . . . It doesn't come with our experience in the domestic zone. I can't understand 3 roentgen lifetime dose. And incidentally, this is a tower drop and in a surface, of course, you would release 2 curies of strontium 90 per square mile; there will be other factors, however. You wouldn't cover 2500 square miles; you would more likely cover 100 square miles -- it will be a smaller area.

LIBBY: When you walk around on test areas up there, do you wear masks?

LULEJIAN: I don't.

LIBBY: Do they ever measure those masks for activity?

BUGHER: Those who are working in areas that are dusty and have a

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radioactivity often have the filters checked, and they might be quite high.

IBBY: Is the dust trapped in charcoal or a paper disk?

UGHER: I think that Col. Quinn and his crowd had quite a lot of those filters in connection with the operation.

OLDEN: Some of the NRDL personnel, after the underground shot, went into the test area to recover samples and they had respirators issued to test personnel and when the respirators were removed you could see about 20 m r per hour by nostril probing.

. QUINN: The material would get into the nostrils when you're wearing those respirators -- not through the filter pads, but comes into them by leaking around the nose because of certain facial contours.

Y: Concerning this strontium: If you got it into your lungs, would it be in your body? Would it be metabolized if it were soluble?

ER: I think that whatever it is we actually retain, it is not exhaled in the next breath cycles. It is, for all intents, soluble material. It will soon be transported to bone, and that was a point that Dr. Comar brought out: that to inhale strontium in that finely divided state is going to

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be actually a bone problem nonetheless.

SCOVILLE: In the Jangle underground shot they had animals exposed to the radiation where the total dose was something like 2000 r, and they autopsied those animals and found not even approaching a tolerance dose inside them in spite the fact that such tremendous quantities of dust were found.

BUGHER: Those were the sheep you are thinking about.

SCOVILLE: Yes, that's right.

BUGHER: The larger particles would tend to be pulled out in the nasal passages.

SCOVILLE: Still they had large amounts of dust in the lung. There was no activity.

BACHER: I just made a little rough horseback calculation here indicating that you would have to take up about 10 kilograms of that dust in order to get the tolerance dose.

LULEJIAN: Did you assume a certain size of dust?

BACHER: Well, I assume that you mix this uniformly and that when you kicked it up you mixed it up in, say, about a 1/2 a centimeter down. This seemed like a sort of . . .

LULEJIAN: In other words, the only way to get the strontium down would be to eat the vegetable or mineral. You'd have to eat 10 kilograms of dust. That's the question I wanted to ask, and yet you've answered that it does really solve the problem. I'd like to get together and calculate it.

BACHER: Look, you can do it with some simple numbers.

KRAMISH: Are there any who would like to make what they consider absolutely important comments before the conference breaks up?

GRIGGS: I was out this morning while Dr. Bethe was talking. I saw that the ratio of tolerance to serious effects as put on the board was different by a factor of 100. Yesterday we were talking about a factor of 1000 for this same ratio and I would just like to ask how this basis . . . We called it lethal yesterday.

BETHE: The argument for this was, in the case of radium, that 1/10 of a microcurie is considered tolerance dose and 1.4 had in one case given serious effects. One case out of 200 cases. This is only a factor of 10. Now I threw in an extra factor of 10. I think against the protest of Dr. Bugher between radium and strontium to account for the different effect of beta rays and alpha rays, that it was my contention that for strontium uniformly distributed in bone you should really consider 10 microcuries as the tolerance dose in order to be consistent with other numbers, in order to be consistent with a 1/10 of a microcurie for radium or with 300 milligrams per week of gamma radiation. This factor 10 and the other factor 10 between the tolerance dose of radium and the series dose of radium gave the factor 100.

GRIGGS: Now I understand the basis of the 100; could I ask about the basis for the 1000?

BETHE: Now yesterday we also were told that one radio worker died with 8 micrograms and one lady lived with 40 micrograms where 1/10 was the tolerance, so between 80 and 400 is the lethal dose.

GRIGGS: Is there going to be clinical evidence for the actual LD 50 dose in strontium?

BUGHER: I hope not! I think we are quite content to leave it as a speculative computation rather than having experimental confirmation; but I think what Dr. Bethe was trying to put in here (what he called senoris) was somewhere around 1% of the people showing definite lesion, and the top figure was something like 50% lethality or senoris lesion and that is as good as anybody can make at this time.

KRAMISH: I want to thank all of you on behalf of the RAND Corporation and its various contractors for coming to the conference and giving us your ideas; I think we derived a great many ideas to think about and I would like to conclude the conference and hope that you'll all take a quick lunch and rush up to the nuclear energy group and get your ideas down on paper.

Thank you very much.