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Complete Transcription of Project SUNSHINE Conference

Held 21-23 July 1953

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Gabriel

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LIBBY: This is the circulated agenda for the RAND-Gabriel Conference, which I hope all of you have available. The starting hour was wrong, it was a mistake of ours and I am sorry, so it means the morning session will be a half hour shorter and I am going to clip my introductory comments.

The purpose of the Gabriel Project you probably know, most of you. The question is whether an atomic war leads to widespread destruction of human life separate from blast and ordinary bomb effects. That is, for example, whether the radioactivity introduced by the explosion of atomic weapons is dangerous and at what levels it becomes dangerous. Now, there may be other aspects; I see Colonel Lulejian here - which are nonradioactive but which might have worldwide effects. A suggestion of the Colonel's - just from atomic weapons, I think a very interesting one, which might have widespread effects on the climate. So there are various aspects of the Gabriel Project which are nonradioactive, but I believe it is most likely that the most serious effects of the atomic war falling in the province of Gabriel will be the question of worldwide radioactivity. Now the purpose of the RAND-Gabriel is to consider this problem in all its aspects in a summer study and to present the conclusions to the Division of Biology and Medicine, the Washington AEC, and the U.S. Air Force I believe is also interested in it. There have been several Gabriel studies in the past which have been very good, I think, but which have had the common weakness that they did not proceed to a testing of the conclusions. We have taken the point of view that no matter how reasonable a set of

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LIBBY: (continued) assumptions is that you can make, in considering this problem, it is so serious a question that it must be put to experimental test. So the purpose of the RAND-Gabriel has essentially been to set up an experimental test program to determine which of the various possible sets of assumptions one might make in the attack of the Gabriel problem. So we shall be discussing a problem from the point of view of what we may learn in the future more than from the point of view of what we know now. I think one would be quite certain that the earlier Gabriel conclusions are probably roughly right. I believe that the data will probably substantiate the correctness, but it is just that the problem is such a serious one that if there by any doubt about the correctness of the basic assumptions we must settle it by direct experiment. This is perhaps a new slant which we hope that the RAND-Gabriel will have introduced into this problem.

We have asked you gentlemen to come here to help. The great wealth of experience represented in this room can in a short time, I think, tell us whether the directions we intend to take - that is, the directions which program we intend to recommend - are likely to be right. There are several basic questions involved and these will come out during the course of the presentation. We hope that the agenda will prove useful and interesting and on the second sheet we have set forth, for the afternoon sessions, some discussion panels on various problems. We intend to convene this morning until about 1:30 this afternoon to present a sort of general review of the

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LIBBY: (continued)

whole problem. Wednesday and Thursday will be filled out in more detail, but in the afternoon we hope to split into groups of seminars or discussion panels on several subjects. The first of these is connected with the meteorology part principally, the second one is a sampling on the worldwide basis, the third is a problem of the measurement of the samples, the fourth is one which we wish now to defer until, say, tomorrow afternoon, though there may be some hallway discussion, we would hope to put it off until knowledge of the technical aspects has been presented, perhaps. If there is any wish to see further we might discuss it, but I think at the moment we will put it off until tomorrow afternoon on the classification policy programs.

It may be rather time consuming, but I would rather like to introduce you gentlemen to one another. I will just call your names - would you mind holding up your hands or standing. I will just go down the list. Some of the people may be absent.

Roll call

We would like very much, as you listen to the presentation during the next two or three days, if you would make notes on your particular field of interest. Particularly if they are critical, if you don't agree with the presentation or the conclusions and write these down and leave them behind so

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LIBBY: (continued)

that we can use them in the preparation of the final report. This, I think, would be one of the most valuable contributions to be made and I hope there will be enough time left in the afternoon for you to do this.

We have, in the disbanding of Panel D for today, a problem of redistribution. These eminent gentlemen selected on Panel D, I would suggest that you distribute yourselves among the other three panels, if you so choose. I think that the sampling problem has very important aspects which you might find interesting and in which you might be helpful. The problem of measurement of these small amounts of radioactivity which are involved in some of these samples might also interest some of you, and the Panel A, which is principally meteorology might also be of interest to you.

Before we disband, in about 20 minutes, for morning coffee or begin the formal presentation, I would like to raise briefly, for your information and consideration, a rather important problem in the Gabriel task. This is what we call the classification question. We have two parts to the RAND-Gabriel that arbitrarily decided all the dissemination of atomic debris that occurred in the first week after implosion of a bomb, would be what we called local distribution, and this would automatically be classified information. And then we said that

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LIBBY: (continued)

the dissemination of bomb debris on a wider basis might well be unclassified information. Insofar as the purposes of Gabriel are concerned the isotopes involved fortunately seem to have very little significance for long-range detection or very little possibility of revealing any details of our own personal operations. The preoccupation of this question stems very largely from my own personal fears that the difficulties of doing the testing and measurement are so great that it would be almost impossible to do it on a classified basis. However, this is the question I want to bring before the group for your information and consideration, the question of whether it seems necessary to declassify it. Now, I must point out that it is not entirely a question of RESTRICTED DATA. I think at this point I might ask Mr. Morse Salisbury to make a sort of definitive statement. Mr. Salisbury is head of the Technical Information Division, is that correct? Technical and Public Information Division of the Washington AEC, and if you don't object I would like you to make a sort of definitive statement of the worries, outside the RESTRICTED DATA question, which may be involved. By the way, this conference itself is rather classified information, that is it is desired that the existence of this conference not be put in the newspapers.

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SALISBURY: Mr. Chairman, this can be very briefly stated in its main outlines. There is quite a bit of particularization that could be done if you wanted to plunge into all of the possibilities of trouble, not in the field of revelation of technical material that would aid the opposition, but in the field of psychological impact at home and abroad. At home your basic problem is that there is an under-current, a ground swell of uneasiness going through the population pretty much outside any rational consideration of what the hazards and the effects of atomic weapons explosions are. Part of it comes out of a superstitious feeling and after that, therefore, because of this idea a type of reasoning that the weapons explosion being followed by tornadoes, being followed by extraordinary rains have been the cause of these things. It has been quite troublesome this spring just from the sheer volume of handle of letters from people who are not malicious at all, who are quite sincere, but who are quite dogmatic about their belif that atomic weapons explosions are responsible for weather anomalies and ought to be stopped. This has manifested itself in statements of two Congressmen, even, and letters have come from Congressional Committees to the Air Force, the Army, the Weather Bureau and the AEC asking for statements on whether or not there is a connection.

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GRIGGS: May I ask whether this is an argument for or against classification?

SALISBURY: It is an argument against avowing that the United States government is making a study of this character.

TELLER: This is hard to understand. One does not combat prejudice with absence of study.

SALISBURY: One does combat prejudice with fact and the point here is that until the study is completed we do not have fact. You have a thing that is in process of becoming, not in process of being and the speculators go ahead and speculate about it and the propagandists in the international field go ahead and use it as a club to beat you with. There is also, in the domestic field, a growing feeling that the fallout business is putting the health of various communities at hazard. We have had to pay considerable attention to this idea in the semi-annual reports - both the thirteenth, which appeared in January, and again in the fourteenth, which will appear in July. The stirring up of another round of discussion on what are the hazards in advance of being able to say "our study has revealed, thus and so," is likely to be harmful rather than helpful. Certainly, I should think that no one would say that when the results are obtained they should be withheld, but I do think that prudent people will say that until you do have results in hand you don't touch off speculation on the subject. Now, in the international

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SALISBURY (continued) field you have, of course, the possibility that, as in the case of the germ warfare propaganda drives of the Soviet group, this - if avowed officially and made a point of openly for all the world - may be seized upon and made the basis for another type of propaganda campaign and we suffered considerably from the germ warfare campaign and are still suffering from it in the international relations field. These are the basic things clear outside the proposition of whether you want to withhold the facts, the techniques, etc., etc., from the world for putposes of protecting our military and technical position.

LIBBY: Thank you, Mr. Salisbury. The principal isotope with which we are concerned is strontium 90 - as will be seen later - so what we are talking about is collecting samples to determine the distribution over the world and particularly in the biosphere and particularly in human bodies of this 20 year isotope produced in something like 5 percent fission yield from ordinary atomic weapons. This is the problem, to get the samples and to determine what is the distribution and what is the result of the firing off of some 40 bombs. Now, the previous Gabriels indicated that there would be no real danger. The estimates varied somewhat, but they all agreed that the present level of radioactivity is likely to be very small. Is this not correct, sir? I don't believe

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LIBBY (continued): any of them had any real worries at the present time. However, there is enough as a result of the bombs that have been shot so that wecan answer the very important question, what would be the distribution if you did have an atomic war. So you see, though we are very much concerned in determining the amount of strontium in your bones we have no fear that the amount to be found should scare anyone, so I wonder, Mr. Salisbury, whether there isn't some unnecessary worry about the magnitude. If yourealize that everyone who wears a wristwatch has much more radioactivity, exposure to radioactivity than would result from this kind of thing. That this is really a very tiny amount of material, I think that this puts it in a very different light. Of course, my whole persuasion about it is a question of the necessity of it. Dr. Bugher, I believe, feels that this may not be so, that the samples can actually be gotten. It is difficult for me to see how these samples can be gotten with sufficient dispatch and in sufficient number as in a sufficiently economical faction so that one can proceed with this assay on a classified basis. XXXXXXXXXXXXXXXXX

VOICE: If you wouldn't mind sir, I would like you to make a remark or two about this - if you wish. The question is of general concern. I would like the whole conference to think about it.

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VOICE: (continued) It is, in my opinion, perhaps true that we just can't make this assay on a classified basis. Now, I may be quite wrong about this - it is a question of procurement of the sense, a question of whether you think you can procure enough samples from all over the earth, particularly the type of human bodies, in order to make this measurement possible.

BUGHER: I think, as Dr. Libby has indicated, the discussion really of the problems of classification and such matters of policy will come somewhat later after the program itself, the scientific orientation of it, has become somewhat more clear in our minds. For that reason, I think he has chosen to have that panel concerned with those things actuated somewhat later. The points that he raises are, of course, very pertinent to the program with which we are concerned and Mr. Salisbury has made clear some of the problems which are not related to the scientific issue, but do concern themselves with public relations and political questions in which there are almost endless ramifications. From the standpoint of getting material, I have felt that a first thing is to define precisely what material we need, what material we wish to have and from where, and when we do wish it, under what circumstances must it be collected. Having decided those things then one is in a position to consider what are the ways in which one can proceed to accumulate this material - together with the necessary

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BUGHER: (continued)

documentation of its source and mode of collection. I am quite prepared to agree that a system of sample collection which is in itself classified is likely to get into considerable difficulty. On the other hand, there is almost always more than one way to skin any particular cat once you get down to that concrete problem of how do we do this particular thing. I, myself, do not feel particularly apprehensive about the problem of sampling collection without publicity, as far as the objectives are concerned. Dr. Libby has mentioned the importance of securing human material and which is, I think, very fundamental, in this program. Ultimately that is what we are going for, is an evaluation of the human risk which attends a large scale atomic weapons program, and, of course, behind that then lies our judgements as to how extensive the whole atomic energy program in this country may be so that the human objective is certainly pertinent and very much to the point. The problem of obtaining human material are usually far more difficult than those of classification. Those of you who may have from time to time attempted to get autopsy material, for example, in this country will appreciate that the problems vary from region to region of the country, and actually from community to community. There is much less difficulty, for example, getting autopsy material, generally speaking, in university communities where there are well known medical schools, where the idea of autopsies has been

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BUCHER: (continued) developed over a considerable period of time than there is in a remote community where many factors come into play having nothing to do with the question of medical motivation. There are very many difficulties in this area, but I still feel that we can solve them and, just in passing, recall to you that we have operating in Japan at the present time a program supported by the Division of Biology and Medicine devoted to the study of the human effects, in a long-term sense, of the bombs of Hiroshima and Nagasaki. That program does involve a great deal of autopsy work and human materials from that source, at least, I think can be assured with no very great difficulty at all. There are some other areas where we can obtain human material through other types of functional approach relationship. In each case a whole set of special problems attendant from that locality, but those are not classification problems in that case, and they revolve around other matters, so that I think from my standpoint, as I look at the problem of sampling, I certainly agree with Dr. Libby that samples are fundamental to this program and must be obtained. I am perhaps not quite so pessimistic as he is, that samples cannot be collected unless there is widespread information. The problems of cooperation of people and so on are, of course, complex ones. I hope that as the program is more clearly defined that specific objectives are named, then we would be in a position to discuss, for

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BUGHER: (continued) example, with the Department of State various persons who must come into this sort of thing and just how we go about to do particular things.

LIBBY: Thank you - Dr. Bugher. I would like to say that I have never had anything but respect for the magnitude of the problem of presenting this task or request for information to the public. I have never been convinced that it ought to be put into the papers. What I would say is absolutely essential is to be able to talk freely to people who might be able to help you. Now, I don't know, maybe it would be the best thing to put it into the paper, but I hand't that particularly in mind. It was more a question of being able to tell people frankly and clearly that you were after Sr^{90}, and why you were after it, and why you needed their samples and then they might be able to give you better samples than you thought about - and might take an enthusiastic interest in helping you, and send it to you and pay the postage themselves, and it actually comes. The government is such a lethargic way of proceeding. Things seem not to happen with any alacrity frequently. It is so discouraging for an experimental man to see how slowly things happen through the normal channels of the government, and this is the fear and almost a certainty - that this slowness will result from classification, and this is the basis for my remarks. You just won't do it.

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LIBBY: (continued)

It isn't that it couldn't be pushed through if you really wanted to do it - and give up a lot of other things - but it just won't be done easily and well on a classified basis, this is my position.

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LIBBY: The main part of the earth's surface, of course, is ocean. Do you think, Mr. Kulp, that we could obtain samples of the sea water from the top of the bottom, that is, the newly fallen sediments, and of the microscopic life in the sea? Could we obtain this on a classified basis? Will you tell me what your own slant would be about being asked to furnish these samples, (a) on the basis you were told it was on a Sr^{90} assay and, (b) you were told...I don't know what you would say, Dr. Bugher, that it was...for general purposes? For classified purposes?

BUGHER: We have some programs, for example, dealing with quantities of trace elements in human bodies from various parts of the country in which we don't say we're after one thing — we're trying to get as much information as we can. In the marine field we would like the same people not only to take Sr^{90}, but to think in terms of their own interests.

LIBBY: (or KULP) But for example, suppose you were making a cruise out of Woods Falls......(I may say for the rest of the people here that Mr. Kulp is connected with Columbia and the oceanographic work with Mr. Ewing.) What would be your slant? You see, I would say that you would react as follows: You would say, sure, we'll do this, but then forget about it, and you wouldn't actually take any real interest in it, and naturally it's real trouble for you to take some cores and strain out this fine microscopic life, bottle it, and then to bring back these many gallons of water in barrels; it's real trouble, and you just wouldn't quite

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LIBBY: (or KULP) continued) get around to it, whereas if you were interested in th[redacted] strontium assay problem and realized that the oceanography — there was no minor interest to oceanography itself in the results of these assays — that you might be so interested as to make the measurements yourself; not only collect the samples, but go right through it and furnish the data. Have I guessed your reaction correctly?

KULP: Pretty nearly. In the worldwide assay, I think the important point is that we're on a classified basis, and it would be impossible for these research ships to get a wide enough distribution in a certain period of time, whereas if you could just ask any ship of a major line and ordered some barrels put on board simply because the captain was interested in this problem, you could get a complete sampling of the Atlantic in a matter of a couple of months. Whereas if you had to send out a particular classified ship to get these samples, it would take you years to do the same thing. As far as the bottom samples are concerned, that's a problem of technique, and only a research vessel equipped with very special equipment to take it, but you might only want a few samples anyway.

LIBBY: I don't believe that I presented Dr. Bugher's point quite right. I don't believe you would have sent out a classified vessel. You would have asked Dr. Kulp to have given the samples without his knowing why you wanted them. Isn't that what you would have done? Or you would have given him an excuse for that sample which was only a partial reason why you wanted it.

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BUGHER: I think we're arguing about something we might perhaps be debating a couple of days from now. But I don't think that once we get this sample of sea water that we would confine ourselves strictly to Sr^{90}, and, the opportunities would be too great to do other things. I think we'd be interested in the organic constituents — elements which are present in various and all amounts.

LIBBY: Well, I'm not sure that, unless someone else has a pointed comment, there is any purpose in continuing this discussion at the moment. We'll have this panel perhaps tomorrow afternoon or maybe Thursday, but this will be the only time for the whole group to discuss this. I would much appreciate your leaving comments on this point, and if anyone has any remarks to make....Mr. Bacher, would you care to make a remark? I don't want to force you to, but....

BACHER: Well, referring to a remark that Morse Salisbury made, certainly as long as one is making a study of a situation, it's prudent in not being forced to make premature conclusions to avoid any announcements and discussions, etc., and I think this was the point that he was trying to make. This is certainly a prudent thing to do. On the question of how one goes about collecting samples, etc., I have to confess sufficient ignorance of the problems involved here that I don't think I'm really eligible to an opinion, but if you look further ahead in the problem of when you come up with some answers, and know a little about what

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B[illegible]ER: (continued)

your conclusions are, and then have a problem of final public relations, we're faced now with a situation in which there is probably no area in where there is more hocus pocus and less understanding than in the general problem of radioactivity and its effects on humans. And until we are able somehow or another to put down some of this mystery — approach this problem with a little detachment and a little understanding, we probably will never solve the final problem — this then comes back again into the final problem of public relations — and I'm sure that in that stage Mr. Salisbury would agree too, that you're going to have wide dissemination, much greater understanding of the problems of radioactivity than you have today, and this will only be done if a good deal of this problem and its findings are laid out in the open for more people to know. So, the question of what happens in-between ought in part to be judged not only by the problem of the difficulties of collecting samples, but also by the fact that finally this is something where the public is going to have to become much better educated than it is today if it's going to understand what the problem of the future is.

B[illegible]:

It seems to me that probably here the group at RAND already knows quite a bit about radioactivity and quite a bit more than the public knows, and it seems to me that we have everything to gain by making public today a large fraction of what we know just to alleviate public concern about the matter.

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TELLER: I don't think we'll ever know about radioactivity. Not so that everybody will be satisfied. I think that if we want to dispel these fears that exist, we must publish the facts as we learn them. Because of public relations, I think that it is quite clear that in trying to say things, one might say too much. When somebody publishes prematurely that he has a cure for cancer, he certainly does not do any good to the medical profession. It, however, had never been proposed to combat this evil by classifying research on cancer, but by the most faithful and better understood methods of professional ethics — if you do that you are called the name which you deserve to be called. It seems to me that insofar as public relations difficulties are concerned there is no doubt about it that they are tremendous, and that the most careful thought should be given to these relations. However, it also seems to me that one of the sharper criticisms that can be leveled against us, is, that we keep secret questions which influence the welfare of the population of the earth. And I think above all, that I would like to see that any justification for this criticism is avoided. I believe that today we know enough to quiet down a lot of the doubts which exist even in the minds of very intelligent people. I think that one should be very careful about what one is saying. I think that any attempt at keeping this general discussion secret will give rise to the king of suspicion which then will die precisely ...which will then, I should say, will die very much harder than the completely unjustified accusations about germ warfare.

LIBBY: These comments are [redacted] and enlightening. We'll have coffee and re[redacted] minutes.

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LIBBY: The presentation of the results of the RAND-Gabriel study. The first hour will be devoted to the local problem, that is, the fallout in the first few days. Mr. Will Kellogg and his associates will present this material.

KELLOGG: We will have to go rather fast in order to cover the work in the last several months in an hour or so. I'm going to start by going over, very briefly, some of the work that we've done. Then I'll turn it over to Bob Rapp, who will tell you in a little more detail about sample study. As some of you already know, we've rather arbitrarily divided the Gabriel study into two parts. We refer to them as Part I and Part II. Part I concerns the early fallout, and by this we mean the fallout which occurs in the first few hours or days, and Part II, the world wide fallout, which we've been discussing this morning mostly. There was a good reason for this division. First of all, it seemed that Part I, the early fallout, could be considered a type of weapon affect, and therefore it was obvious that it would have to be kept classified in most of its aspects. Also, we knew that there was a good deal more data available on the close in fallout, due to the routine checks that are made around test sites. So we felt that it was a good idea to treat this separately, and what I'm going to talk about is Part I, the early fallout.

Just to throw this problem into focus, let me mention a few things that I think are universally known about how the material is brought

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KELLOGG (continued)

down to the ground in the first few hours. First of all, we know that if sand gets into the cloud under the right conditions - we'll specify what these are later - that it may combine with the radioactive material and may then bring the radioactive material down to the ground, due to the fact that sand, at least in Nevada, is large enough to fall, and from the height of the mushroom, reach the ground anywhere from an hour to four hours after shot time. Another important mechanism which is universally recognized as a way for bringing down radioactive material, is rain. However, we don't have very many well documented cases where rain has brought radioactive material down to the ground, so we can't rely on the results from the test sites in order to establish how this might occur, and we have gone into the mechanisms whereby rain can scavenge debris in a good deal of detail, and I'll tell you about those in a minute. The small radioactive dust particles which are less than 20 or 50 microns are so small that their rate of fall is negligible, and we can consider that gravity will not bring these down to the gound in the first few hours, or even days, and we must look for other mechanisms to bring those down. I mentioned rain as a possible way to bring these down. Also, we must consider diffusion; that is, the verticle tranport by motion in the atmosphere. This is also a factor which we have had to look into.

The purpose of the present study then, was to understand these various factors that I described, and, in a sense, we have perhaps looked at this from a rather academic viewpoint, feeling that the

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KELLOG: (continued) first step was to get a basic understanding of the processes involved. We felt that we had to do this before we could go on to the next and real purpose of the project, which was to predict what one would expect in the way of fallout over a wide range of conditions - that is, conditions of burst, height, yield, and of atmospheric stability and humidity.

Taking up these factors more or less in the order in which they enter into the development of the cloud, the first thing that we wanted to look at was the way in which the cloud rose. There are several reasons for wanting to look into this early mechanism and describe what happens.

The first thing which happens, of course, for surface bursts, we have a mixture of surface material with the radioactive debris, and we've concentrated to some extent on trying to find out just how close to the ground one can get and still not have the scavenging by sand. We have some pretty good ideas now about how this can occur and I'll bring them up again later. We are also interested in what would happen as the cloud rose. We noticed, as I think everybody has noticed, that there is a strong internal circulation in the cloud, causing a sort of a smoke ring, toroidal shape of the radioactive material. We tried to get information on this, feeling that the cloud can't be considered as a homogeneous sphere, but must be considered as a doughnut, where we are talking about the radioactive material, and we have gone through innumerable films to scale off dimensions of this cloud, and I'll give you some of the results of this. We have

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KELLOG: (continued)

also been interested in where the cloud stabilizes when it stops moving upward, because the initial distribution in the radioactive debris in the atmosphere has an important bearing on how it will come back down to the ground. Some of you are already aware of a tentative conclusion that if it goes very high it will be more difficult for it to be scavenged by rain. Perhaps if I show this first slide it will demonstrate the point a little more clearly. We have a plot here of height of positions 40,000 30,000 and 20,000 in feet, versus yield in kilotons - for instance, 5, 10, 15, 20. I have plotted here some data points, in this case, all from the Nevada tests, and they don't follow any very systematic pattern; that is, they don't ~~fff~~ follow exactly on the line which I have drawn. But I think they give a good idea of the trend. In a few cases we've also plotted bottoms, and we are still in the process of scaling all photographs to get more accurate and more points to plot on these curves. These dash lines here I'll mention because they refer to a theoretical calculation of where the centers of clouds would go for various atmospheric conditions. The first thing we tried to do was to see how this mean curve, if you will, the colored line, would change if we exploded them under conditions which are very different from those in Nevada. We'll discuss this more in the afternoon session, but very briefly, this top line is for summertime conditions in middle latitude, so we have relatively unstable conditions and a very high tropopause. And this lowest one is where we have wintertime conditions which are extremely stable.

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We happened to choose a mean condition for continental polar air over Europe. You will notice that there is about ten thousand feet of difference in these two cases and to some extent we can say that this corresponds to the change in the height of the tropopause, because this does act as a very important lid, but actually in this case it never did reach the tropopause, and so we can't rely entirely, particularly for the low yield bombs, on saying that tropopause is the height at which it will go. Now I mentioned earlier that one of the reasons we were concerned with this question of heights was because we felt that it was important to determine what part of the cloud could be scavenged by rain. These blues dash lines represent the height to which the rain in rain-bearing clouds usually goes -- the maximum height to which it goes in summer, and the implication is obvious, I think, if you accept our data, that if we got a bomb of more than about 5 KT under Nevada conditions, that all of the debris will go above the maximum height of the rain in rain-bearing clouds, and therefore we wouldn't expect it to be rained out initially. We'd have to wait for it to come back down by some other mechanism before it could be rained. The next thing which we wanted to take into account was the transport of the debris, both horizontally and vertically, after the cloud stabilized. The gross transport is due to the wind and I think meteorologists can usually provide good information on where the cloud will go for the first few hours, based on upper air winds. We can also tell how it will go on the average, because we have a pretty good backlog [illegible] of data on upper air winds. Another effect is, of course, the shear in the atmosphere. This is a little bit harder to get at, because it is not usually observed directly. One has

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KELLOGG (continued)

to deduce it from the change of winds with a different elevation, and the effect of the shears are important in that it causes the cloud to be distended, elongated, and diluted. So it is important to get information on shear, which we have tried to do. Then the question of diffusion in the atmosphere, which perhaps is in a sense small scale shear, is equally important because the turbulent diffusion in the atmosphere, something about which meteorologists are still undecided in many aspects, is the thing which will transport the debris vertically, and this is obviously very important in the absence of precipitation or sand scavenging, it's a very important mechanism for bringing it back down to the ground. We have tried to represent diffusion in terms of diffusion coefficients, which is a poor way of doing it, but as a first approximation it given us some answers which we can sue. The next subject which we have tried to get information on is the question of the scavenging. I mentioned the rise, the transport, and now the scavenging, and first scavenging by sand. We have a good deal of information from Nevada as I mentioned, and the important thing that has come out of this which I think we can explain, is that when we have air bursts there is no material brought back down to the ground by sand even in cases where the sand apparently came in contact with the mushroom. But when we have tower bursts and surface bursts there definitely is a scavenging by sand. We are indebted to Colonel Lulejian for supplying us with some of the test results in this respect and I have prepared some slides from Colonel Lulejian's data. I'll present what this looks like. We have not applied any fancy analysis to this because we felt that the test data was illustrative of the kind of thing we get in Nevada and we would like to go into this in more detail eventually to show how it might be

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affected by surface conditions. For example, supposing it were exploded over a city or over an agricultural country or something like this. For the time being let's just look at this and learn what lessons we can from it. This is ground zero here, and these lines which are drawn, are lines of equal radioactivity on the ground which were measured a matter of several hours after, or even a day I guess, after the shot occurred. This is for TUMBLER/SNAPPER No. 5. We'll just look at it briefly to show the typical pattern which demonstrates a large fallout here, and then a secondary fallout at some distance away. This is past the Lincoln mine. Unfortunately I don t have a scale of miles here, but it s on the order of ten to twenty miles from the ground zero, and if you'll correct me if I say anything very wrong about this. What's that? Seventy-five miles. Sorry, I should have had the scale of miles on here. These arrows that are plotted here correspond to the hypothetical trajectory which a 100 micron particle would take it is started from any level and fell following the winds, and fell at a rate corresponding to Stokes law. The center would correspond to the fall of a 100 micron particle from about 20,000 feet. I mention that in passing. We will discuss that in more detail this afternoon. Next slide.

WEXLER: (first, question about the other maximum).

KELLOGG: (??) This other maximum down on the side, to go back, well, presumably from another section of the cloud, another wind structure. Do you want to comment on that one?

LULEJIAN: Dr. Wexler, that other maximum is that one percent that we can't explain by Stokes law, and it always occurred that way; it might be topography, it might

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be the fact that we shot the bombs in the early morning when there was a strong inversion, but that one percent of the total, no explanation.

LONG: Could you give the value of the radioactivity on the highest contour of that secondary maximum as compared to the first in ground zero?

KELLOGG: Yes. The lifetime dosage of ten roentgens. It says 5 roentgens here. Down here it's one.

TELLER: What is it at ground zero?

KELLOGG: Down here it's not marked.

LULEJIAN: Actually sir, right at ground zero it was really high, 2,000, but it was a very small area, very, very close. However, here is something that will answer your question. Around ground zero, if you will take the whole percentage of the bomb that was deposited -- it was approximately 1%, whereas in that secondary maximum it was approximately 25%. In other words it;s a small amount around ground zero. Fall [or total?] activity of the bomb deposited on the ground -- the percentage would be 1%. Although it's a very high rate, it's such a small area that the total curies are very small. 25% off.

TELLER: Do you happen to know the height of the tower? And what was the yield?

300 ft. tower, yield 11.8 KT.

LIBBY: I deduce from these numbers that 2/3 of the bomb didn't come down.

KELLOGG: I'd like to go into more detail later since we don't have very much time

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KELLOGG: (continued) this morning, so let's go to the next slide, which shows another case: TUMBLER/SNAPPER George shot, shot number 7, 13.8 KT (call it 14), and again a 300 ft. tower. This is drawn to the same scale as the other one, so I would judge this is 50 miles or so. And the center in this case is more intense -- it's 20, and the next one is 10, the next is 3, 2, etc. Again though, the important thing I think is to note the secondary maximum, and that is the point I wanted to make, this primary point here, the secondary maximum corresponding roughly to the time it takes for sand to fall back down out of the mushroom or upper part of the stem. The next slide....

WEXLER: What was the total fallout?

L̲̲JIAN: That was 7%, and incidentally, if you have a minute .... In this case we have very low shear, so the maximum was greater, however for some reason only 7% came down.

TELLER: Is 7% of 25% more usual....

LULEJIAN: 7% is usual under usual meteorological conditions.

LONG: 90% remains up.

Lulejian: If it's a 10 KT, 300 ft. tower, 90% usually stays up.

KKELLOGG: Now the next slide: This is TUMBLER/SNAPPER tower shot number 8. Fourteen KT, 300 ft. tower. Again the secondary maximum: Do you want to tell the fraction brought down?

L JIAN: That was 5%.

KELLOGG: 5% total fraction. The center here corresponds in this case to 5 roentgens

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KELLOGG: (continued) lifetime dose.

The next slide. This is an attempt to explain something that came out of Colonel Lulejian's data in terms of the fraction which is scavenged as a function of the height of burst and yield. As a matter of fact it was presented to us as a function of a ratio, the ratio being height over yield to the 1/3 power, and there seems to be some consistency in the fact that at a surface burst when this ratio was zero, height was zero, we get a very large fraction. There is differing opinion between some of the people who studied the JANGLE surface shot, which is the only Nevada surface shot that we can point to. There seems to be some differnce of opinion as to just how much did come down in the first few hours, but it seems to have been very large, perhaps between 70 and 100% is a representative figure, although I must talk about this tomorrow with Dr. Cadle. As we go to larger values of this ratio we approach zero scavenging, and the interesting thing to us was that zero scavenging occurred when the fireball was only just a little bit above the ground, and when all the pictures showed the dust going up into the mushroom, we though that this was something that had to be explained before we went into it any further, and so this is a tentative explanation.... This is the air burst -- I don't think we need to comment on that. The mushroom goes up, the dust column is always formed, but it never reaches the mushroom, so we wouldn't expect any dust scavenging. Here we have the other extreme -- the surface burst, or very near surface burst, where we have dust going up and mixing with the hot fireball when it's still in an early stage, and then I think in this case we would expect it without going into any more detail than just saying that the dust and the molten and vaporized radioactive material were together. Then we have the case of a low air burst, which is a difficult one to explain, because it does not show

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KELLOGG: (continued)

scavenging by dust, and as a tentative explanation I think we can invoke the fact that we have very strong internal circulation set up immediately, and at the movies this afternoon it will demonstrate this I think, and the strong internal circulation causes the dust column to move into the fireball in this way, and the air gets in first. The radioactive material almost immediately spreads itself out into the doughnut part of the cloud, and later the dust which continues to pour into the mushroom merely circles around the doughnut, and never reaches the radioactive core, which is now here and here, and this, as I say, is a tentative explanation, and we simply felt that we weren't happy about observations until we had some idea of how this occurs. Now if we can have the lights....

We're going to move on to what we've done about rainout, and rainout is a more difficult thing to talk about, because as I've mentioned, we don't have any well documented cases of rainout, where we can make a hypothesis and say, well, it happened in such and such a way on such and such a date, that these were the conditions, therefore out hypothesis seems to be borne out or not as the case may be. We have almost no information on rainout except some information from the Harvard people, Dr. Bell, Dr. Thomas, who have made careful observations of the readioactivity in rain, and however the thing that's lacking there to some extent is detailed information about the structure of the rain storm which brought it down. So we've had to rely on a theoretical approach to what one would expect from rain.

First, and I think the most obvious approach, would be to say what happens when a falling drop falls through a cloud of radioactive particles. We approached this by looking at the Langmuir theory for collections by a

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KELLOGG (continued) drop of small particles, taking into account the fact that these particles were probably solid and had a higher density than that of just water, and came to the conclusion that we would only have a serious scavenging by rain for particles of greater than about 2 microns. The very small particles simply flow around the falling drops; they follow the stream lines around the drop and never come in contact with the raindrop. This has been predicted by Langmuir's theory and we felt that there was something incomplete in this treatment, since the results of the Harvard people indicated that rain did bring down radioactive material, even several days after the explosion, radioactive material must therefore have been very small, and further when they looked at the kind of material in the water it appears to be in very finely divided particles, I won't go into the details here, and if Dr. Bell wants to comment on this....

BELL: I don't want us to be responsible for saying that this material is in finely divided particles, not that it is or is not, but we just don't know, because we are sanitary engineers, and we suspect that it perhaps is, but we don't want to be held responsible.

KELLOGG: A very cautious and timely statement.

WEXLER: You mean you suspect it's smaller than 2 microns? Is that what your suspicion is?

BELL: Sir, I just don't want to say.

WEXLER: Well, I mean you're suspicious about something, what is it you're suspicious about?

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BELL: I'll say that my <u>suspicion</u> is that it's smaller than 2 microns.

LONG: What was the evidence?

KELLOGG: I think this is a very important question, and one that we will have to talk about a great deal more.

LIBBY: Let me take two minutes. The rain in Massachusetts is radioactive, and when subjected to the normal water purification processes, the radioactivity is removed to only 50%. Now this normal purification process is precipitation of aluminum hydroxide. Is this correct?

BELL: That is correct, yes sir.

LIBBY: And this makes a flock which is filtered out, and it;s a good water purification process. This thing removes coloring matter, the tanic acid, which makes the water colored. It's a good water filtration process. There's some form of radioactivity which resists this.

LONG: You sound as if you're making a good case for it being in solution.

LIBBY: Well, you have to take those things in solution which would not be co-precipitated with aluminum hydroxide, and then you look at the fission product yield curves and you say what are those radioactivities. It's something of a puzzle. We on the basis of this were contemplating what we call "hundred angstrom rubies" and it's a concept. I am no longer convinced that this type of evidence proves the existence of small colloidal particles, but it certanly does indicate that there is something which needs to be investigated about the physical form of radioactivity which come down in Massachusetts rainstorms.

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KRAMISH: Now Bill, we might mention that MIT has been interested in the same chemical processes, and using Oak Ridge isotopes in solution has removed 90% by the same process, which only removed 50% of the bomb debris.

HILL: Actually, this was a statement by the Harvard group.

BELL: I wish to apologise about that and I would like to do more of an elaborate study on that. We made the statement, in fact I made the statement, and I would like to verify it. I suspect it's true, but I'm not sure yet.

These people have studied several of the actual precipitations, not always by precipitation. I think, the public welfare people at Oak Ridge have also done some work on this -- I'll have to dig that out of the library.

TELLER: I believe I have the correct suspicion that a nuclear reactor produced material over a long period, which therefore emphasizes the long-lived elements, whereas the bomb has instantaneous production, and therefore empahsizes the short-lived elements, and therefore you get a completely different sort of distribution.

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LAURITSEN: Is it not also possible that raindrops could pick up very small particles if one or both are electrically charged due to static electricity? Does that come into this?

KELLOGG: That is something that we certainly looked at. I was just going to say that we went on, having decided that at least the Langmuir theory would not account for the small particles, and we felt that one should certainly investigate all the possibilities for bringing down the small particles — we leave open for the moment whether they are small or not. The evidence seems to be that there are - for an air burst - a great many small particles where there is no contact with the sand.

LAURITSEN: Isn't it true the raindrops sometimes are electrically charged?

KELLOGG: That's right. Stan Greenfield looked into this in a good deal of detail, and observations in a normal cumulus cloud show 30 to 40 - or so - primary charges on a cloud droplet in a cumulus type cloud, so in a normal type cloud there is a charge. It occurred to us that there might also be a charge on radioactive particles - due to the fact that they are emitting electrons, those that are beta emitters. Well, we didn't get very far into this investigation before we discovered that the dominate factor is the small ion content in the atmosphere, and that where we have radioactivity we have a very large production of small ions, well above the normal background which occurs from cosmic rays and natural radioactivity in the air. Therefore, the droplets

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KELLOG: (continued)

are very quickly discharged and would fluctuate rapidly from a unit minus to unit plus and back to neutral again because of collecting small ions in the air, and the same would be true in general of the small radioactive particles. They also would be neutralized rapidly by the large atmospheric ion content, so it appears that we could not invoke any electrical charges as the cause of coagulation. However, if we just went back to a more or less classical mechanical model and looked at what Brownian in motion would do, where we had certain concentration of cloud droplets and a certain concentration of radioactive materials of sub-micron size, it turns out that Brownian motion could be quite effective in bringing the radioactive material of less than about .01 microns to the cloud droplets...That is, quite effective in the course of an hour. When we go to a larger particle, that is between .01 micron and about 1 micron, then Brownian motion becomes quite unimportant and the turbulent motion would bring these particles together. I hope you will stop me, Stan, if I say anything that is differing from your findings. The turbulent motion can account for a slow collection by the cloud droplets of the radioactive material, but it is much slower. This slide is just a sample of the calculation in which, to demonstrate the factors involved, some specific assumptions have been made about the conditions, but it could be calculated for other conditions and give essentially the same results. This is a logarithmic scale of particle diameter - that is diameter of radioactive particles - starting here with a

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KELLOG:
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hundredth of a micron, 10th of a micron - 1 micron. The ordinate is fraction scavenged out of the cloud, in which the rain cloud and the radioactive material have been together for one hour. Brownian motion and turbulent effects have operated for one hour to bring the particles into the cloud droplets. At the end of one hour, rain starts and a moderate rain of 2.5 millimeters per hour occurs for one hour. Now, the rain falling through the cloud - as Langmuir predicts - will scavenge out the cloud droplets quite effectively, the cloud droplets being in the order of 20 or so. So in this region, off the scale, we would have something that might correspond to the scavenging of the cloud droplets themselves. Now, this large value here corresponds to the scavenging of the radioactive particles directly by the cloud falling raindrops. We have large collection efficiencies for the large radioactive particles - as I mentioned. Over here at this end of the curve for very small particles we have a large fraction of the small particles taken into the cloud droplets, and so when the rain occurs it scavenges them - brings them down in solution. In this intermediate region between about a tenth of a micron and 1 micron we are at a loss to find any good mechanism for bringing the radioactive material down in rain - unless we imagine that the cloud droplets and the radioactive particles could last together for a matter of a day or more. In this case there might be time for some of the radioactive material to be taken into the cloud droplets. A

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KELLOG: (continued) verification of whether this analysis is right would be to find out what the particle size in the rain was, and at present we are looking around - in the absence of any information of particle size that is brought down by rain - and merely trying to say what one would expect to happen - and this is our best guess at the moment.

WEXLER: Will there by given any consideration to another possible mechanism for bringing down radioactivity - say from the air - just by entrainment of the air in the rain.

KELLOG: We haven't considered this - although it will be implied in what I am going to say about what could happen in a cumulus type of shower. I don't think I have answered your question, Dr. Wexler, but perhaps we can talk about this this afternoon.

WEXLER: I mean any great volume of water coming down - take a shower bath - or Niagara Falls, as an extreme example - brings out a terrific quantity of air. You see, this just by shower curtains blowing away. The question then is: In an ordinary rainstorm - how much air is actually transported down without any resort to Langmuir's collection efficiency just by simple entrainment of the air in the falling water.

KELLOG: Well, if we can talk about something that we do recognize as a definite downdraft, that is the central rain core of a thunderstorm with its associated downdraft, I think maybe I can answer part of your question.

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GRIGGS: Could I ask a layman's question - Is there any experimental evidence that indicates whether radioactivity is incorporated when the H_2O is in the water phase bs. the ice phase.

KELLOG: No information on that at all. Sorry! Does anyone have any ideas on that?

EISENBUD: There is a case where hailstones fell in Washington. These were hailstones about 2 1/2 inches in diamter, and were markedly radioactive. The activity was not concentrated on the center of the hailstone. It was so diffused throughout the mass of a hailstone that we could not get a radio-autograph - despite the fact that we had a thousand or two thousand dpm throughout the mass of the stone to radio-autograph.

KELLOG: Wouldn't this imply, then, that the radioactive material was probably initially in a liquid form, since that is the way hailstones are formed - by liquid gathering on a core.

LULEJIAN: I am not sure on one point on Dr. Wexler's question - going back to sand again. In the sand, we have more or less concluded its 100 microns, more or less. We have a lot of evidence measured at Los Alamos - from Dr. Schulte's unit. They were with us and they had jet impactors, and they found the majority of particles - in numbers - were in the 1 micron range, and this came down in two hours from 40,000 feet. This must be due to entrainment in the fashion you are talking about, and it cannot

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LULEJIAN: possibly follow anything that Langmuirs' saying about the raindrop - and probably then, since the sand fallout certainly is not as concentrated as rainfall as far as mass is concerned, you might get more entrainment in rain. But we do have measured evidence that 1 micron particles that should come down in six months come down in two hours.

WEXLER: I am glad that you mentioned that, because that recalled something to me that I had completely forgotten about: When you have a thunderstorm come along you get temperature drops in a very short time, temperatures below that of the prevailing wet bulb/temperatures, showing that the drop in temperature is not something due to evaporation of the rain into the air there. The temperature drop is so large that it could only have come from very dry air at great heights that have been brought down to the ground and then cooled by evaporation. This has been a subject of some study some years ago by the people in the Bureau, and I think that some of their conclusions were that just on the basis of an observed temperature drop and the prevailing distribution of the potential temperature - that such air had to come down from heights of 20,000 to 30,000 feet within the course of a single thunderstorm.

KELLOG: Would these have come from the central part of the thunderstorm or would they be outside of the thunderstorm?

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WEXLER: They would be outside the thunderstorm. That is, they use the radiosonde - prevailing before the thunderstorm situation, and

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WEXLER: (continued) the only way they could match the temperature minimum observed at the ground was by air that must have been brought down from those heights and then cooled by evaporation.

KELLOGG: I want to get this clear - this is outside the thunderstorm system.

WEXLER: Yes, it is outside the thunderstorm. I will have to look that reference up - this was written in a paper sometime in the '30's by someone in the Hydrometeorological Section of the Bureau - and I forgot about it until Lulejian mentioned it. I think that this air entrainment probably is a fact that should be looked into.

KELLOGG: I am sure it does. Oddly enough, this leads directly to my next point. We have been wanting to take up several specific cases that might occur. That is, there are so many variables in this problem of how debris gets back down to the ground that we felt that it was better to fix our ideas by considering some rather explicit and definite cases. The first case we can dismiss quickly - that is where we have a surface burst. We have already discussed this a little bit, and we can say that it depends mostly on how we set the weapon off and not on the atmospheric conditions. It is determined more by the condition of the ground than of the atmosphere.

However, let's consider a case where we have an air burst. Now, I am making the assumption tacitly when I talk about an air burst that here we have small particles that cannot come to the ground

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KELLOG: (continued)

by gravity, and we leave open whether they happen to have a maximum density MR at 10, a tenth, a hundredth of a micron. Let's say they are too small to come down by gravity, but that they could be scavenged by precipitation. Supposing now we consider the kind of precipitation which can reach the highest into the atmosphere, and as Mr. Wexler mentioned, thunderstorms are the obvious things that come to a meteorologist's mind when he thinks about a precipitation system which reaches high into the atmosphere - causes large vertical transports of water and of air. So, the first thing that we wanted to talk about, then, was what would happen if we had a thunderstorm reaching up into the radioactive layer. The first slide here shows this in the form of a sketch. This dirty brown is supposed to represent a layer of radioactivity between 30,000 and 40,000 feet, that corresponds to about the height where it would be for a weapon of between 20 and 30 kilotons. Now, this is above the usual rain-bearing level. The usual rain-bearing level is around 20,000 feet. But in the thunderstorm it can go up perhaps a little higher. Some of the radar pictures taken around Boston in the summertime show columns of water - partly water - at least they give a good radar echo, which go up higher than 20,000 feet, and perhaps even to 30,000 feet. I tried to visualize what would happen in the course of the development of a thunderstorm. This is the cumulus stage, and this is the next stage, when it is called a swelling cumulus, and you will notice that my arrows are drawn based on the report of the thunderstorm project. These arrows show that

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KELLOG: (continued)

apparently in this swelling cumulus stage we do not yet have any great rain density. We have nothing but up-draft and in the center and in-drafts on the side, and then, at the top - as must be the case to have continuity of motion - we have up-drafts and out-drafts. This presumably varies from case to case, but this is the typical type of circulation system. Now, the next stage will be a fully developed thunderstorm in which are drawn in green the column of rain in the center, and now I have indicated the fact that we do have strong down-drafts associated with the column of rain. If we only consider the rain I think that a tracing through of the history of the particles of air and clouds which are taking part in this system, that the radioactive material must have been pushed up and out at the top and cannot be expected to be in the control part of the thunderstorm. However, I have acknowledged ahead of time Wexler's contention that there would be a down-draft outside of the thunderstorm, although I did not know that it would be as strong as he suggested. So, as a result then, there would not be scavenging by this storm, but the result would be a mixing through the tropopause, and perhaps a possibility of scavenging for subsequent storms. Possibly if the downdraft is very strong for this gross transport of the debris downwards outside of the thunderstorm, the mixing would be very considerable. We will have to look into it to see if it could not account for it coming all of the way down.

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KELLOG: (continued)

The next case we thought ought to be considered to satisfy the people who might say: "What would happen if we had a thunderstorm immediately - not an hour or two later, as suggested before — but occurring right at the time of the explosion?" I tried to visualize what would happen here if we were to consider that the bomb went off on a thundery afternoon, in which we have convectively unstable conditions. I may now be saying something which may not be in agreement with everybody here who has studied thunderstorms. Let's say tentatively that we could have a condition where only a small displacement vertically <u>in the stem</u> could set off a thunderstorm at this point. I should say here, how, that this is definitely in the stem that the thunderstorm would start, because the time sequence for the development of a thunderstorm is in the order of one-half hour to more than an hour, whereas, this mushroom cloud goes zooming on up and reaches its ceiling at about five or six minutes. Now, if this thunderstorm were to develop here, we would have essentially the same story that we had before, the only difference now being that the debris might be incorporated in the upper part of the thunderstorm system itself. Since this is well above the minus 20° isotherm, we would expect this to be nothing but ice particles and, as I understand the picture which has been gathered from radar studies and flights through thunderstorms, we do not expect this material to fall down to the ground, because the ice particles in the upper part of the thunderstorm grow very slowly. So then we can say that in these two cases of thunderstorms we do not get rainout of radioactive material by the thunderstorm

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KELLOGG: (continued)

itself, but the thunderstorm can merely result in vertical mixing of the material.

I think that the next case that we will talk about will demonstrate another thing which is implied in the fact that when we say a thunderstorm can't result in bringing the debris down in the first few hours we would not have any very serious hazard from the rainout. Now I am not going to amplify on that statement yet, because I think that will come out in the next case, where we try to show what might happen under a more orderly system of things, where we have the cloud traveling through the air and diffusing in an orderly way and in such a way that it can come down into the rain-bearing level, and then subsequently be scavenged by rain. I would like to turn this case over to Dr. Rapp. Incidentally, before he takes over, let me say that this is the case that was discussed in an earlier RAND report, which a lot of you have read. It was also discussed in a recent AFSWP study of contamination by rainout.

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TELLER: It is possible for the circulation around a thunderstorm to be a complete cycle? It occurs to me that both cases of the late thunderstorm and the early thunderstorm would permit the rain to bring down the particles, not on the first way up, but after they had come down and then carried up again.

KELLOGG: Let me mention again a thing that could occur on subsequent thunderstorms. One thunderstorm lasts about an hour, and I wouldn't expect the cycle to be complete in an hour, but on an afternoon when thunderstorms may persist over an area for many hours the first thunderstorm would stir, the next thunderstorm would stir even more, until finally we could, I think, have the debris going back up into a later thunderstorm. I think that is a very difficult thing to say anything quantitative about it, except that there is this possibility

SOLOMON: How do you reconcile your findings with Dr. Eisenbud's findings about how much does come out in rainstorms?

KELLOGG: Let me have Dr. Rapp answer that.

LIBBY: May I ask how many and how common are thunderstorms that go above 30,000 feet. Is this one in a hundred or it is quite a common thing.?

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KELLOGG: Well, I don't know what the statistics are for the tops of the thunderstorms. We have been looking at the top of the rain in the thunderstorms and it hardly ever goes above 25,000 feet, but the cloud tops go up to the tropopause, and there seem to be cases even reported when they have penetrated the tropopause, although I am not sure how accurate the information is. I wouldn't have expected any thunderstorm to go above the tropopause.

COONS: Many of the results of the thunderstorm project showed radar returns which went off the top of the measuring radar at over 40,000 feet, and these returns supposedly represent precipitation in a solid form snow, giving the radar returns above 40,000 feet.

KELLOGG: That would change the height scale on the diagram some, but I think that the general conclusions would be the same, that the radioactive material would be pushed out the top first.

LONG: I am a little unclear as to what the next variation is going to be. Is it going to take up the sort of extremely obvious question of what happens if you set off a bomb in a good hard rainstorm?

RAPP: I hadn't planned on taking that up. I was going to talk about what would happen at some much later time.

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LONG: Isn't this a very obvious one in this category?

KELLOGG: Well, you can say the same thing about it that I said, about the thunderstorm occurring. That is, that the bomb goes up very quickly and passes out of the normal rain bearing level, and that we have to invoke some scavenging agency which gets up to the level of the mushroom. Now, for a low yield case - this will be taken up by Dr. Rapp - it stalls while still in the rain bearing level and is available for scavenging by rain.

LONG: One would also think that a combination of a ground shot in a hard rainstorm would be very horrifying.

KELLOGG: That I think would result in the highest contamination that one could imagine - a low yield surface shot in a rain. Then we have all the scavenging processes working in our favor.

RAPP: There has been a lack of observation of actual rain-out close in. One of the great guessing games has been to figure out systems whereby you could rain material down. The first such system that I know of was devised by Colonel Holzman in which he assumed the cloud to be a cylinder which was distorted by shear. The cylinder extending from the ground to about 20,000 feet. This was dictated more by the

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RAPP: (continued) availability of meteorological data than by any actual consideration of the cloud. This cylinder of cloud was then assumed to be torn by shear, expand at some slow rate and 100% of all the materials that came in contact with a rain area was considered to be deposited on the ground immediately. This study was the only one to my knowledge in which many bombs were considered. Colonel Holzman was trying to estimate the hazards from a campaign of bombs in a small area. There are several things with due apology to Colonel Holzman, that are wrong with this. One, the cylinder of atomic debris did not extend uniformly from 20,000 feet down to the ground. It is generally higher than this and so when the RAND study was made, I can call this the Mark I Model for scavenging of rain, we took a cylinder and expanded it assuming that the material was homogeneously distributed through this cylinder. We considered the observed height of the cloud, expanded it horizontally and vertically and assumed that all the material below the -15° isotherm was brought down to the ground in the form of rain. We didn't take into account the shear of the wind, so that was one thing that was missing. Then the people from AFSWP worked on the problem and they said well it is silly to consider all the material brought down as soon as it hits the rain and they allowed for a finite time of scavenging

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RAPP: (continued)

and they made another estimate. However, they stuck to the expanding cylinder with the homogeneous distribution and they did not consider a vertical change of the cylinder. Our latest model, Mark II Model, allows for a different type of expansion of the cloud. We have considered a cloud to expand according to a Fickian law of diffusion and we have chosed a diffusion constant that is as big as we can get it without tearing the cloud apart. We want to try to keep this cloud intact and expanding so we can't allow for too big a diffusion coefficient, this would tear the cloud. Also, because we don't want to tear the cloud apart, we haven't considered the shear. But this now gives you a picture of what our cloud looks like at the end of six hours. We start out with an elipsoid of homogeneous concentration 3 kilometers thick and 5 kilometers in diameter and we assume that the center went to a height of 10 kilometers. As the diffusion spread this cloud, the peak concentration started to fall and these lines here represent shells where the concentration has fallen to 1% of the original concentration, 0.1% to 0.01% of the original concentration. You must try to imagine that these shells initially, when the cloud is homogeneous, are all together, we are upat 100%. Then as it starts to spread these low concentrations spread out rapidly, then dilution begins to take effect so that after

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RAPP: (continued) a while after a 1% line has expanded it gets so diluted that the central concentration falls down and these shells contract again. I think this next picture will help you visualize it. This top section is the peak concentration, shows how the hottest part of the cloud varies. It is right in the center of the cloud and it starts out at 100% and falls rapidly in 4 hours it is 10%. This lower figure shows the height of the spherical shells that contain a certain fraction of the initial materials. You will note that the 10% line immediately starts to contract due to the dilution of the cloud and it disappears in 4 hours when 10% is precisely at the center of the cloud. The 1% line continues to expand for about 12 hours and then it slowly starts to contract and these other ones are still expanding out here about 24 hours. This is the type of diffusion picture we have used and now lets go back and look at this one again.

VOICE: How do you compute this?

RAPP: This comes out of the Fickian diffusion law. We used a diffusion coefficient of approximately 10^6 centimeters per second and this is simply the solution to the equation.

SPENCE: I don't believe that it matches with the instrument readings that were taken in Nevada.

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VOICE: This is a mass rating rather than a radioactive rating?

RAPP: Yes, this is a mass rating. This is just a percentage of the initial material.

PLANK: I would like to say that we find the distribution rate is inverse with the time to the first power. That is, the effective rate of decay of this thing is 2 minus 2.2 roughly, there is some scattering data which would show you the error.

VOICE: Is it the center?

VOICE: It is the hottest part. We try to find a place where there is the least diffusion. For example, an isokinetic streamline which happens to be running through the center of the cloud. If we can find an isokinetic layer this is where we fly, because this is where we get the most material.

LULEJIAN: That diffusion rate is for a very yound cloud in the first half an hour isn't it?

PLANK: Well, we usually started in at one hour and call it a day after about 5 hours.

RAPP: Well, let me say that this model is set up and is highly idealized. We believed that the cloud would not be continuous and expanding like this we are simply trying to see what such a model would bring down in the way of rainouts.

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VOICE: Are these values in close agreement or not?

PLANK: I don't know how to translate the diffusion rate which is inverse with time with his diffusion coefficient. Perhaps he could do this.

RAPP: If I understand Plank correctly what he is saying is that in the first two hours the peak concentration goes down inversely with time.

PLANK: Yes that is in the first ten hours.

BETHE: This is what you would get, it seems to me, if you had diffusion in a plane regardless of the diffusion coefficient.

LIBBY: Did you get Dr. Bethe's remark. That it is the approximate observational would indicate that three dimensional model should perhaps be squeezed down more to a two dimensional.

RAPP: That's right. Well, we are quite sure of that. We tried to take the maximum vertical diffusion. We believe that the diffusion coefficients should be much, much smaller in the vertical than in the horizontal and therefore, it would correspond more closely to diffusion in the plane than this particular model. We tried to get here a model of diffusion which would bring the maximum amount of radioactive debris down into the rain-bearing level in such a way that we could make some sort of analysis of how much stuff would be rained-out.

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THE: Is there any experimental evidence on the magnitude of the diffusion coefficient?

RAPP: No, there have been some estimates of diffusion coefficients made from the clouds. Dr. Machta, of the Weather Bureau, has done some work on this, but these diffusion coefficients in the free atmosphere are extremely rough estimates. This one, 10^6, was chosen because this would give us a scale of motion smaller than the cloud and therefore not tear it up, but it was the biggest one that would give such a motion. If we go to a larger diffusion coefficient the scale of the motion is going to be big enough to tear this cloud into little whisps and pieces. We didn't want that to happen on this model. We have gone all the way back to Richardson's collection of K values, which he made in 1926. I don't think anybody holds a brief for those today. We went on to this and made a rough integration of the amount of material that was under the 6.4 kilometer level at different times. At one hour the gradient is steep (Figure 2) and you would expect very little in there. By about three hours you are just beginning to get appreciable amounts in there, you would probably get some rain-out but we would have had to have another line here to make some sort of a calculation. At about 6 hours we have made a calculation and assuming 100% scavenging over a small area of just about one-half a square mile we would have gotten a dose rate on the ground of about 70 miliroentgens per hour. If this diffusion had gone along and we had 100% scavenging. Now let us go back to Mr. Greenfield's picture. I will put it on here so that you can look at it. This represents

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P: (Continued) the type of scavenging you would have to deal with. 100% scavenging would occur only for particles larger than say ten microns. If we were down in this range, less than .01μ, we would get fairly high average efficiency and would bring this stuff down, but here, between 0.1 and 1μ, unless we can come up with some new mechanism for bringing it down, we would have very little scavenged out. We went on and calculated again for 12 hours and, despite the fact that the cloud was diffusing and being diluted, enough additional material had been brought down into this rain-bearing level so that the deposition on the ground was about twice what we got in six hours. The calculation of 24 hours shows that the deposition began to get smaller, the cloud had by this time been so diluted and the decay factor had entered into it strongly so that the amounts deposited on the ground are getting smaller despite the additional matter brought down. Now this process of building models and attempting to estimate the hazard has been going on for a couple of years now and I think we are still quite far from a reasonable sort of model that we might like to try to test on what little data is available. I think, for one thing, we must take into account some better law of diffusion. We use what is available now, we know that this doesn't give us a good picture of diffusion in the atmosphere and Mr. Edinger will speak more full on that this afternoon. We need some better law for diffusing this cloud. Another point, we need some more experimentally determined values of whatever constants might enter into whatever diffusion laws we need. Another point, we have to be able to consider all

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1. P: (Continued) all scales of diffusion and by this I mean we must take into account the shears. Colonel Holzman's paper took shear into account. None of the subsequent models have attempted to consider shear. So, this must be taken into account and I think another thing which must be taken into account in order to get a reasonable model is the initial conditions. We are sure that the material is concentrated in a toroid, in a doughnut, not a sphereoid or a cylinder, or any such models and I think this must be considered in trying to estimate the hazards of rainout. There is not too much point in making too many calculations on these admittedly inadequate models, we do it simply to see what the trend is and to see whether we are approaching what few observations of rainout are available. I certainly wouldn't want to guess as to what the estimate of the hazards would be, say by Friday afternoon. That is all I have to say.

LULEJIAN: Listen Dr. Rapp, is it possible for us to say that if we have a model, make that a straight line scavenging curve, mx would we loose anything in view of the fact that we don't know diffusion so well? Why not go ahead and say 100% scavenging and not have any of this refinement?

RAPP: Yes, we could do that. We could always have considered 100% scavenging, but if we were to try to get a more realistic model and if this minimum does actually exist this could make a tremendous difference.

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[illegible]JIAN: Well, that was my question, does that make a lot of difference?

LONG: That brings up whether you know the activity distribution within the particle size.

RAPP: If for some reason we could discover some mechanism that could bring down 100r with 100% scavenging or if the 3% factor were to enter in and make it only 3r I think this would be of considerable interest to the people who are worried about the hazard on the ground. Any further questions?

BETHE: I don't understand several things. First, I don't understand how there can be any doubt about the diffusion law. I thought there was only one diffusion law.

RAPP: Well, lets put it this way. As long as you are talking about mollecular diffusion then there is only one diffusion law. However, when you start talking about diffusion of matter by different scales of motion in the atmosphere which may range anywhere from the molecular scale up to thousands of miles, due to the large scale motions of the cyclones and anticyclones, then you have an entirely different problem of diffusion. It is intimately tied up with the size of motions which are spreading the clouds. We should be very technical about this and always speak of eddy diffusion so that this point doesn't bother people. Anything else?

BETHE: If the molecular diffusion were important it would seem to me that you can calculate the diffusion coefficient.

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RAPP: Well, the molecular diffusion you could, but this is so small - this accounts for so little spread in comparison with the movement caused by the motions of the atmosphere that we completely neglect the molecular diffusion in comparison. I think the K value for molecular diffusion is 10 to the minus one, where here we are talking of diffusion constants of 10^6, 10^7 and 10^3.

LIBBY: These units are square centimeters per second?

RAPP: Yes.

LIBBY: Other questions?

GWYNN: I have another question, Dr. Libby. For the study by AFSWP we talked with Professor Libby and his proposal, which was made an assumption to part of our paper and used for calculations, was that these small particles which form in the first after-stages of the fireball itself are incased in small water droplets. That is, one of the reasons that you see the cloud itself is due to the moisture that is collected condensing to the water droplet size. He has made an estimate of the calculation from that and given us the size 15 to 20 microns which, if he is correct in his assumptions, would cause the scavenging efficiency to go up fairly high in short times so that some of the boys at ARDC have turned out on the indicator 75 to 90% scavenging efficiency for heavy rains. That may be one mechanism that might answer that type of question.

RAPP: We had thought about this problem. The only thought we had on it was that we are talking here about the cloud that goes well

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RAPP: (Continued) above the rain bearing level. Then we have two problems; one, it is probably rather dry up there and it might not last for a long time and the other one is that it would have to somehow get down.

GWYNN: One theory is, I am not a meteorologist and since I am outnumbered here I had better be careful of what I say, there has been some theory proposed, by whom I am not quite sure right now, rather that there was continual interchange cycles between, if the assumption is correct it is encased in a small water droplet that is between 15 to 20 microns in size, it is as it rises partly evaporated but at the same time a concentration mass of this matter that it would be reintern encased in other water droplets. Whether that changes the size distribution I am not in a position to say.

RAPP: I certainly don't want to get into any argument about the sizes. As far as I am concerned it is the biggest question mark of this whole problem.

EISENBUD: I would like to take this opportunity to point out, for the benefit of whatever value it is to those of you who are interested in the theoretical approach, that there is a possibility of a very simple empirical approach which comes about because, at least from our observations and I think from others, the normal dust burden of the atmosphere is very similar in particle size and spectrum to the particle size distribution of this debris. There is the opportunity of measuring atmospheric dust burden before and after rainfall and also the particle size spectrum within the drops. It is my impression, I know that at least

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EISENBUD: (Continued) Dr. Holden must have done this, perhaps some of the others. In the old days out of curiosity I used to take dust samples of air after rainstorms and a lot of dust present is almost impreceptible compared with what is ordinarily present, particularly in urban air. There was so little that it has always been my impression that for all practical purposes under certain conditions the scavenging effect is 100%.

LIBBY: But you don't know if it is the same air that you are dealing with.

EISENBUD: No, that is right. It is probably more complicated than this.

HOLZMAN: Johnson of NRL, I think he is working up at Livermore now, did some work on measuring rain and testing its radioactivity and he noticed that with the onset of rain he got almost a terrific increase in the background and then it fell off sharply. He implied that thus it was 100% effective in scavenging, of course, this doesn't say much about what was left in the air but he was under the impression that it was very effective scavenging.

RAPP: Well, if you looked at this steep radiant on the curve you would note that scavenging will rapidly remove all the large stuff and the small stuff would get down much more slowly.

HOLZMAN: But, to continue on Eisenbud's theme, there are many, many documented cases of rainfall, muddy rainfall and brown snows appearing with a whole host of particle sizes all the way down to very small amounts, and gives one, at least from the meteorological point of view, a feeling that it must be a very effective

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ZMAN: (Continued) scavenging agent no matter what you say about the particle distribution.

LIBBY: I hope that the impression has not been given that this Part I is purely theoretical. On Thursday morning we will have the presentation of experiments that are to be proposed to cinch up on some of these points. Did you have a remark, Dr. Holden?

HOLDEN: I just wanted to mention that the effect that Merril Eisenbud described is also noticed with radar and fluron decay products in the atmosphere. If you measure these products before and after a rain-storm you find that the scavenging effect is very complete.

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LIBBY: (continued)

We think that it would be a good idea since there was so much interest in the subject in the Part I report that we organize an additional seminar this afternoon on particle size distribution. We will hold that in the Nuclear Energy conference room on the second floor and invite anyone to attend who wishes, who is assigned to any of the other panels. We'll also move the seminar on scavenging transport, and diffusion into this room for we have a feeling that this will be the largest of the various meetings. At the end of the hour Mr. Turner will give us directions as to where these other meeting rooms are. In the next forty minutes I shall try to tell you what we intend to recommend on the worldwide problem. As I indicated earlier this is primarily an outline of experiments to be done. The problem is when will people start dying as a result of an atomic war, and we focus our attention on the radioactivity. It isn't that we haven't paid some attention to other physical effects but that we are rather convinced that the radioactivity is the most likely to be the most serious. We organized the project on the assumption that biology is so impossibly difficult that we do best to segregate biology and to pay attention to those things we can do something about on a short range and so we asked the Division of Biology and Medicine to tell us what the most important and most dangerous fission products were, or the most dangerous radio-

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LIBBY: (continued) active products given by the atomic bomb. We expected a list of some length ranking the most important on down, but the list was quite short and consisted of one isotope, and this is Strontium^{90}. Now I've given a slightly incorrect impression; there're are interest in other isotopes, but the interest in Sr^{90}, as I understand it, far exceeds that interest in any other isotope. With some justification we can say that our problem is reduced to the study of Sr^{90}. Now this is in many respects a fortunate development, for Sr^{90} is a beautiful thing as far as measurement is concerned. It would be very difficult to divise an isotope easier to detect in low quantities than Sr^{90}. This is the result of the fact that it has an yttrium daughter, the Sr^{90} decay is a form of yttrium 90, this is a 20 year half-life. The yttrium decays with a 61 hour half-life and emits a 2 million volt beta ray. This beta ray is about 1/2 million volt. This is the chemistry of the tri-valence elements. This, of course, is an alkaline earth so the chemical separation of yttrium and strontium is quite a simple matter in principle. Now one sees immediately that processes utilized could consist of two things. You can use the separation of strontium by itself as an element separated from the sample and place it in the counter in equilibrium with its yttrium daughter, its yttrium daughter being an equilibrium in a few days.

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LIBBY: (continued)

In that case you get not only this beta ray, but that beta ray. Two for each disintegration. That's one way of doing it, the other procedure is to not bother to separate the strontium at all, but just to take the yttrium daughter out of the sample. I sort of personnally favor the second, it being a little bit more easy. However, which ever way you do it, it is possible to take a ton of sample and consider detecting the strontium in it with something like 50% efficiency, because of this daughter relationship. For example, we can take the yield of the material from a 20 kiloton bomb. 20 kiloton bomb equals 1 kilogram of fissionable material. That's about four moles. The strontium comes in about at 5% fission yield, so we have about 4 over 20 and about .2 or 1/5 moles of strontium^{90}. That is about 10^{23} Sr^{90} atoms yield. If we get a uniform distribution of a 20 kiloton bomb over the earth's surface, we therefore get about 20,000 Sr^{90} atoms per square centimeter for a 20 kiloton bomb. Now the 20 year life means we have to have 15 million Sr^{90} atoms -- means that you have one disintegration per minute. As the average lifetime of Sr^{90} is 15 million minutes. We will have from this 20,000 over 15 million as the disintegrations per minute per square centimeter. Since we can detect a minimum something like 1 dpm without working too hard, I mean one disintegration per minute, it means that we're

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LIBBY: (continued)

going to have to take perhaps a factor of 1,000, we need 1,000 square centimeters of area, so if we take a 1,000 square centimeters then we get an order of one disintegration per minute. This can be measured. We conclude that in theory, at least, if there's nothing wrong with this reasoning that the Alamagordo bomb, if the fission products were distributed uniformly over the earth's surface, is detectable now anyplace on the earth's surface by taking the fallout on an area so. Of course, we've had a great many bombs since then, one notices for example though, that the radioactivity hasn't gone a 20 year lifetime; means its all around, or essentially all of it is still here from all the bombs that have ever been shot. This is an interesting point for the reason that, referring to our earlier discussion, we have the correlation between the strontium assay and the particular bombs utilized would be very poor probably. With the number of bombs that have been shot we perhaps can remove that problem. It looks, in theory, to be quite practical, now the procedure would be something like this, take your process of your sample of soil or of seawater or of vegetable matter, human material, animal matter, calcine it or extract the alkaline earth from it. The appropriate chemical procedures, trusting that the strontium will follow this, have been to let it stand for a time

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LIBBY:
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for the 61 hour daughter to grow in, separate this out with the minimum of chemical impurity. As you first purify your sample for the tri-valence elements which would follow this, then let it set for a week and then add a tiny bit of carrier and purify again. There seems to be no reason why you can't handle a ton of sample in this manner. So I personally believe that though no one to date has ever measured Sr^{90} (isn't this an extraordinary situation, this most dangerous of all the isotopes, for some queer reason, has never been measured - it just hasn't been done) it will not be at all difficult to measure the Sr^{90} that is due to the atomic bombs any place in the world. Well, I say not at all difficult, you must get the samples and you must process them, but it is not a major operation. The first conclusion is that the worldwide contamination due to atomic weapons will be concerned primarily with the Sr^{90} distribution and so the RAND-GABRIEL proposes that the world be assayed for Sr^{90}, that this thing be done. It seems that there's no way of guessing what will happen. Just consider the various doubtful points. We know, at the present time, something about, at least Mr. Eisenbud tells us, the distribution of ordinary fission products. Sr^{90} is not an ordinary fission, it has a krypton precursor. It's borne in this fashion: This is a 33 second half-life and then it goes to rubidium 90 which is two or three minutes, I've forgotten

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LIBBY: (continued) which; 3 minutes I think it is. Then finally it gets born. During the period of a minute here, or four or five minutes here, but particularly this minute here, the fireball may have cooled down to the point where most of the phenomena which decide the particle size distribution of the fallout are over. Now you have the picture which is rather frightening in some respects, that this most dangerous fission product is now leisurely deposited on the outside of all of the dust particles it touches in a nice soluble available form so that it will desolve in water. This would be one thing that distinguishes it from the other fission products and makes the present data - I wouldn't say worthless - but one certainly must check to see whether there isn't an enormous fractionation of Sr^{90} as compared to the ordinary fission product. Mr. Spence was telling me that this was true in the case of Sr^{89}, which has a similar precursor history, though that krypton is a longer life thing.

TELLER: Is the chain that you have put on the board quantitative, in other words does all strontium come from krypton?

LIBBY: As far as we know this is so.

C. E. ADAMS: I'd like to say that we've analyzed some of the GREENHOUSE fallout radiochemically and there was far less strontium

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C. E. ADAMS: (continued) in the samples then would be indicated by a normal distribution curve.

LIBBY: So this is another argument for the assay. We really don't know anything about it. Now it may be that in the bigger bombs the fireball stays hot long enough so that there's some hope of incorporating this stuff in a sort of an insoluable coating. I think one would say there was some evidence in ordinary fission products that the stuff is incorporated in aluminum oxide or silica or something else that is there when the vapor is formed in the early stages of the fireball and as the thing cools this condenses and you have molten droplets which entrain and dissolve the fission product. This is a mechanism, this makes you sort of rubies, where the thing is protected chemically from the action of waters. This would make the strontium less available to the biosphere and would be a happy thing, but it does seem that in low yield bombs that the strontium is just about as available as it could possibly be. Now maybe the ideas won't fit, but this is another point and another reason for it. So we have abundant reasons for doing worldwide assay on Sr^{90}.

VOICE: Is the Sr^{90} content of the atmosphere known?

VOICE: Sr^{90} does not occur in nature except insofar as its produced by the small amounts of radioactivity.

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VOICE: Any assay?

VOICE: There's never been an assay of Sr^{90} of any sort.

EISENBUD: We have started Sr^{90} determination on a large assortment of fallout materials. I have no data now. The strontium's been separated and we're waiting for the yttrium to grow in. The only Sr^{90} that we have is on those bones that I told you about.

VOICE: What is the Sr^{90} yield from thermonuclear weapons?

LIBBY: Well I don't know exactly, it's probably high. 5% in ordinary fission. There is, of course, a question of the type of weapon yield, but I think in all these things we certainly get a lot of it. Now tomorrow we'll have a more thorough go around on this subject, and I hope that one of the medical doctors will speak to us a few minutes about the reasons for strontium being such an important isotope. We're not particularly competent in this field, but I think we're all curious.

TELLER: Would this be the right time as - while you say that strontium is the main isotope - to ask whether something like plutonium, an alpha emitter, might similarly be important as a bone seeker?

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LIBBY: Well I think these things were next on the list as being dangerous. How much lower I don't know.

VOICE: About 10^{-4}.

TELLER: 10^{-3} would correspond to the relative lifetimes. The energy in plutonium is much greater and my general impression was that an alpha-active substance is more dangerous than a beta-active acting substance in a greater proportion and correspond just to the energy; the energy concentration means that its more dangerous. I'm surprised that the ratio is only 10^{-3}.

WESTERN: 10^{-4} includes the half-life, energy and all the other factors.

TELLER: Then, it probably then includes a greater collective absorption of strontium by bone. Because, if it were only the half-life and energy I would expect the ratio to be less.

WESTERN: Talking about the uptake of calcium that has fallen out. We're not talking about inhalation now. There is a very large factor of diffentiation both in the uptake in strontium by plant and from the uptake of strontium from the plant, by the digestive system. Of course, you have an additional factor there at least of the order of 1,000 to 10,000.

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VOICE: As compared to strontium.

WESTERN: Yes.

TELLER: The strontium is more easily taken up?

WESTERN: Much More.

LIBBY: Strontium is like calcium and calcium is the major element in the body and strontium is like calcium except probably less soluble so if it gets into the bone chemically I suppose there's more chance of it staying there than calcium and so like radium we expect that strontium would stay in the bones. Now we sort of guess at this point about the distribution. We don't know anything but we've still got to get it, because otherwise we don't know what samples to take. The main interest of the GABRIEL is, of course, in human lives. It is quite clear that the biosphere is the most important sample realm, that is, it is absolutely necessary to sample living material, food stuffs for example for human beings. First of all, of course, human beings themselves. Then there are foodstuffs and the soil which supplies the food and the atmosphere and the oceans. All these things are to be tested. In the present ignorant state however, it is very difficult to estimate how many samples will be needed, how difficult the measurements will be, because the difficulty, of course, will be

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LIBBY: (continued)

direct function of the assay, that is the abundance. We propose that the experimental problem be divided into two stages. What we call a pilot assay and then later a full scale assay. And so, at the moment, we're more concerned with setting up the pilot assay program. A set of samples may be 3, 4 or 5 dozen maybe as many as a 100, wisely selected in the various realms, that is human material in the biosphere, meat, that is whether you can use pigs as a substitute for human beings, foodstuffs which are likely to contain calcium, such as milk and vegetables; various kinds of vegetables contain calcium. Using the calcium content as a guide to the likelihood of finding strontium; strontium data are quite poor. Then to test the oceans, test the atmosphere, test the soils. We do enough in the first set of 100 samples to get a notion as to what the facts of life are,

It's perfectly obvious that there's the widest type of fluctuation to be expected in certain of these. We would, I think, expect to find that the sea might be the best mixed of all the realms. So seawater is a good place to begin to get a general notion. We have some measurements on seawater, in progress already. The atmosphere will be next best probably. There are very curious questions here that we heard about earlier this morning.

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LIBBY: (continued)

Problem of sampling in the atmosphere is a very difficult one. If we find that there is worldwide dissemination of fission products and particularly strontium, it seems to me that it is extremely likely that this material is transported above the weather layer in what you call the stratosphere. So we have to sample it. We have to test how much is up there or we have to prove that it is up there. We have various ways of sampling this, I think the simplest thing is to take some rainwater samples from various parts of the earth's surface several months after any bomb has been shot. It seems very likely and reasonable to me that any fission products of strontium that get below 30,000 feet where your rain is scavenging the air all the time will have been washed out rather quickly. If you can go to Southern Chile next Thanksgiving and take a bucket of rain and find strontium coming out of the heavens, I think it's very good evidence that you have strontium in the highest layers of the stratosphere and stratospheric transport. But, regardless of what the implications are, if you find strontium coming out in the rain six months after any bomb is shot this is an important fact in itself. So there's no doubt that we need to collect rain samples and to measure them. We need the ocean samples because this is the next best medium. How shall we measure the ocean, what shall we do about it? Well, strontium is

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LIBBY: (continued)

like calcium. Calcium metabolism in the sea leads directly to the deposition of calcium. Strontium will move along in this place. So we should look, I think, (You correct me Mr. Kulp if I make mistakes in this) but it would seem to me that the type of ocean samples we need are the following: We need the top and the bottom, the freshly fallen sediments, to see whether strontium is in the sea-shells that are just formed. We need the dissolved materials in water itself, we need perhaps articulate matter which is not yet settled and I should think these three thingsare the important things to measure.

P HER:

Well don't you need perhaps the seawater and maybe especially for fresh water fish bones, Dr. Libby?

LIBBY:

Oh yes, of course - I was sort of putting that into the biosphere. This is absolutely necessary. Now taking the biosphere and taking the sea as a principal residence of living material we, of course, are greatly interested in the strontium content of life in the sea - most of which of course is plankton. Plankton that can be sampled; it however will perhaps be easier and cleverer to let the fish eat the plankton and then measure the fish, so you get immediately to the problem of measuring fishes and you get immediately to the problem of the lifetime of the fishes you measure. I think I learned a little from Mr. Ravelle

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LIBBY: that some fishes live much longer than others and you don't want fish that are too old; he seemed to think that a tuna fish was very good and then you just take the bones from a tuna fish. So it would seem that you could sample the biosphere of the ocean pretty easily. You take these bones and calcine them and process them and measure the yttrium in them. Its perfectly simple.

VOICE: When you say the ocean is the best mixed medium, do you mean lateral mixing or vertical mixing?

LIBBY: Well, I think I mean principally lateral mixing and vertical mixing to perhaps a depth of a thousand meters, thru the life zone anyhow. Certainly it isn't perfectly mixed in the full depth, but it is mixed in as far as light exists in the sea to any appreciable extent down to about 200 or 300 meters. It is mixed, in other words, we predict that the strontium content of sea water will not vary widely from spot to spot, this, however, it would be wise to test it.

What do you think Mr. Kulp about this? How many samples would you estimate ought to be taken, say of the Atlantic Ocean in this pilot assay?

KULP: I would think I would take about 10.

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LIBBY: You would take that many.

KULP: Actually your shells are such good integrators, the oysters and clams that you can get at the fish markets probably contain a great deal.

LIBBY: I think there is no doubt that the ocean and the biosphere in the sea and the oceanic life can be measured rather easily and rather directly.

VOICE: Is there going to be one of your goals in the pilot study to account for the total strontium?

LIBBY: All that's wanted in this first thing is an idea of how much strontium there is and how much it fluxuates and so that you can figure out how many samples you are going to have to measure and estimate the cost and budget and time and effort required for a real assay that has real validity.

VOICE: Is there a lead to make it easy for yourself at the start by collecting some ground biosphere samples in the Nevada region where it is presumably a good deal higher than here?

LIBBY: Well yes, sure. That is, I think one of the things we will want to do is to take advantage of the higher concentration near the test site, but I don't think we want to bypass the problem of working out the methods of measuring it even at the lowest concentrations, we must do this and it won't be

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LIBBY: (continued) very difficult and after all aren't we really concerned about whether the negros in South Africa are radioactive? I think that is a principal point in GABRIEL. We know that the local problem is one that you have to watch, a question of world wide is a valid, important aspect of the GABRIEL project. So we mustn't slip in just because it is convenient to just stay at home on this. Incidentally, that would make our job much easier if we could just stay at home, but I don't think it answers the problem. Now on the atmospheric sampling other than the rainfall device, I would say that we could take advantage of airplanes and filters and just test out the samples for strontium. This I think ought to be done and I think some of this ought to be done even in the pilot program so if samples are being taken anyhow and could be tested for Sr^{90} it would be a very help-ful thing, and as far as I know they haven't been tested in the past. Now coming to the human problem - a difficult task of figuring out how to get human bodies. Its quite clear I think that one needs young humans, that is children who have spent most of their lives during the atomic period. In that our bones have been largely developed as I understand it, and you doctors correct me, in the pre-atomic times, so I think we need young bodies. I rather believe we need essentially whole bodies because of the amount of material. It may turn out that its quite a bit easier to detect, we shouldn't highly rejoice over this. [redacted]ts hope its very difficult.

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LIBBY: earlier, but part of the question is a lack of information about legal and religious customs and the difficulties we would have getting around these. One brilliant thought, I don't know whether we transmitted to you, Dr. Bugher, - someone had the notion of using miscarriages as something that might — the type of material which might avoid the religious and legal difficulties and might be fairly generally available, and which would be young enough to give us a valid sample.

DR. BUGHER: How about an amputation; is it sufficient?

LIBBY: Well, I don't know whether there is enough in it. It will depend on the assay as to whether a leg bone is enough.

SOLOMON: Children's bones don't get amputated. In good hospitals they put everything back in the child when they get done and they only amputate them in cases where a child has a disease and then you don't know whether the sample is real or not.

BUGHER: Off hand, it would seem to me just about as important to do adults as children. It seems to me you're distinctly interested in the question whether adults do or do not take it up.

LIBBY: Yes, I think we undoubtedly want to; but in the pilot assay I think perhaps we can forego answering that question. I don't know, it would seem to me that if we can show that the cycle is completed and it does get into human bodies. You see, if it is in a miscarriage, it must have been in the mother.

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BUGHER: Again, if the question is, 'does it get into bones of living mammals, it would seem on a pilot study you wouldn't be at all unhappy about using some other animals than humans, that is -

LIBBY: Well, you take a pig -

BUGHER: I don't honestly think there is that tremendous --- it would seem to me you could answer this question, that is, this could be answered - whether you could use animals.

LIBBY: Well it's a question of the food types. For example, it's certainly going to be necessary to test, say, milk. Milk solids will have to be collected from various spots and calcined and assayed for strontium. Now the question is, people who drink milk, does this get fixed. I suppose there is no doubt about it getting fixed, but it might be necessary to prove this. The vegetables, there are several vegetables which are quite high in calcium, which presumably will contain it; eggs have some. There are various types of materials which, particularly favorable eggshells as Dr. Solomon suggests being a particularly useful type of material, readily available. The question of how many samples of flour you are going to want in order to see what the content of wheat is, is a difficult one that I would say in the absence of any other number we've got to take two or three of each of these kinds of food stuffs and measure and select them from various parts of the country.

CADle: I believe statisticians can help a great deal in the consignment of the samples of foods -- to indicate the probable error and

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computation of the results we get.

LIBBY: We have one with us and I hope in the discussions this afternoon that perhaps some of these statistical points can be brought out because you are quite right; perhaps one of the most important things we want to know from this pilot program is how many samples you are going to have to measure to get any reliable information and this means that you've got to take more than one of any given king to get any sort of indication of the scatters.

CADLE: Yes, we've just gone thru the procedure.

LIBBY: Now there will be various outcomes of the pilot program. It may be that strontium comes down right near the site, disseminated worldwide, or it may be that it has practically uniform worldwide distribution. These could be two general results. Now in the first instance we would, of course, concentrate on establishing the gradient of the given test site; and in the second instance we will concentrate on establishing the turnover time and the general build-up in the human body and of things of this sort which should contribute to our knowledge and prediction of the damage it can do. But, as a rough guess, I would say that if either of these extreme results appeared on the pilot program, we would still want to measure several hundred and perhaps several thousands - I am not sure - but certainly several hundreds of samples in order to accomplish an assay of the world which would be of sufficient validity to warrant the high officials placing

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in it in deceiding whether its atomic war is bearable from this angle. There are several hundred or several thousand, and it's a siseable program and the question of how it is going to be done is no minor one. The methods of making measurements at this level are fairly well developed and do not offer any real difficulty. However, the knowledge is not widely disseminated so there is a problem of instructions. It is a question of whether it would be better done for political reasons; if a private company or university did it rather than a government laboratory, is an important one. I can see that some of the difficulties we were debating this morning might be less serious if you had a cover-organization, that is, if some university or some private company went out and did this even though you might be supporting it, it might cause you less embarrassment. Now we have in mind a number of answers to these questions as to how it can be done particularly in the pilot stage - where we think of the order of several dozens of samples; and I think there is no doubt that that can be done so that the results can be obtained for these pilot samples. Then when these are obtained, the question of the magnitude of the effort will be more answerable and be possible to consider various contractors and types of arrangments that could be made. Now look, this is not only just a matter of Gabriel. It seems to me that the Sr^{90} distribution in the world may be a thing of real fundamental interest in various fields such as hydrology, perhaps; certainly in oceanography. I believe perhaps even

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meteorologists might learn something from the distribution of this material. I think there is no doubt that these various fields of geologists and geochemists can learn something. I think that there would be no lack of interest in working on this if it could be kept out so that people could discuss rather freely, these general fundamental applications of the data. Now I hope these somewhat random remarks have not given you the impression that we are indefinite or uncertain about what we mean. What we mean is very simple. We mean to measure the world, assay the world for Sr^{90}. And this, I hope, we will be given permission to do. It's a problem of presenting the report in such a way that it will seem reasonable to give this permission as a serious and important one. I don't think anybody can guess about this. It's got to be measured and only in this way will a real answer to the worldwide Gabriel ever be obtained. This is the sort of trend of thought that we have had here this summer and I think represents a great deal of documentation; some of this will be presented tomorrow morning and in the seminars this afternoon.

Now Mr. Turner, will make some announcements about more practical matters.

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LIBBY: We want to go over the details of the worldwide GABRIEL this morning. We are most anxious that there be free and easy discussion, so don't hesitate to interrupt and ask questions. The first of the offerings is Jerry Hill's attempt to make deductions from the data presently available, these being very largely those obtained by Mr. Eisenbud and his organization. Without anyone's ever having, of course, measured the real isotope. These is a further difficulty as you know we have real doubts that the Sr^{90} follows the main fission products, because of its krypton precursor. We'll see what we can do.

HILL: I notice that the topic of my talk is present knowledge. It is really not as all-inclusive as that, even considering how little we know about the whole project. However, what I have tried to do is take the measurements that Mr. Eisenbud and his NYO group have been making on the fallout. Actually I have only considered the two series of tests, the TUMBLER-SNAPPER test at the Nevada test site, and the Ivy test at Eniwetok.

From these we can draw, I think, some very broad range of the sort of risk in regard to Sr^{90} that we have at present from what has been shot and perhaps get some rough ideas about how far away we are perhaps from the danger point. Mr. Eisenbud's program consists of sending people gum paper which is put out in a special little frame which is supposedly a square foot to the fallout. Then the fallout material sticks to the gum paper, they are folded up and sent in and ashed and measured on a beta ray counter.

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It was Merril's opinion that nearly all the activity measured would be so small that the constitution of the gamma rays from the fallout to these counts would be negligible. With this in mind - well I should say one or two things about the way the data was treated. The samples were taken once every 24 hours and then when measurements were made these were extrapolated to some common date, so that they could be added using the $t^{-1.2}$ law of decay for the fission products. With that in mind let us look at the table. First, the TUMBLER-SNAPPER results in the top part of the table. The data were recorded in such a way that one can conveniently treat the U.S. data and then the area outside the U.S. The U.S. was very well sampled in TUMBLER-SNAPPER; there were some 92 stations. The measurements range from a maximum of 14,000 counts per minute per square foot extrapolated to January 1, 1953 when the test began in April and went through into May in fact into early June. The lowest value was 86. This reading was taken at Elko, Nevada and this one up in Washington. That is the range of values. The average for the whole U.S. was 1750. I might say a word here about getting these averages. I am sure that if independent people took this data and worked up the averages you could get quite a variety of averages, depending on how you do it. It seems most fair to me since the distribution of the stations was by no means uniform that is, giving a certain station per unit area all in the U.S., to take the stations by state and average them. In some cases there were no stations in a state and in other cases one or two. In those cases I took the values around the boundary nearest the boundary and

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and averaged thos'in. Because it was quite clear from the map on which these values were placed that there were general trends, that is, from the test site outward, there was a general trend of falling off of the activity and this seemed a more fair way to treat it.

WEXLER: Would this measure any activity brought down by rain?

HILL: This is a good question which I want to talk more about later and that's the efficiency of collection. I plan to discuss this later. Since Merril is here I hope he will break in at any time if I say anything that does injustice to what goes on, or if you have anything to add. In fact I think perhaps when I am finished I'd like to ask you to sort of bring us up to date on what has happened since these reports were published if you don't mind. Well, let's refer to that for just a few moments. On that basis the average, well, to complete this business about the averaging. If you just take a straight numerical average, then take the sum of the activities from all the stations and divide them by the number of stations, you get a slightly lower value, but it checks farily well so this is probably a fairly good value. It is about 1500 as I remember. On this basis then, you get about $2x10^{-2}$ curies per square mile on the average over the U.S. as of January 1, 1953.

LIBBY: These are β curies?

HILL: Yes. Now the total value for the United States takes a little analysis. The actual value you take by taking this concentration per

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square mile, multiplying by a few million miles, square miles, is 6.6×10^{-2} megacuries. However, Col. Lulejian has reported that from the measurements close in to the test site in Nevada on TUMBLER-SNAPPER, that for the first four shots there was relatively little fallout around the test site; but for the last four shots, ~~[illegible]~~ quite large percentages of the total activity put up could be accounted for by the ground surveys of the activity and taking his figures where he gave a range of values for the estimate of the percentage of that particular bomb which came down within 200 miles of the test site, I just took the middle value of those, and out of those values I obtained this about a tenth of a megacure of the total was accounted for from that. This is more, you see, that was accounted for in the total fallout outside the 200 mile range radius. On this basis taking the total β-curies that would have been produced, and extrapolating by the $t^{-1.2}$ law to the January 1, 1953, this amount of stuff accounts for about 8%, or 90 of 9% of the total β-curbs put out in that TUMBLER-SNAPPER series. That is just on the United States. Now the worldwide situation in the TUMBLER-SNAPPER --

LIBBY: Let's see, what percentage of the area of the world is the United States?

HILL: It's 3/200.

Libby: About 6%. Very interesting.

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HILL: Yes, but you'll see that this is...uh... you're making an inference which is unjustified, when you look at the worldwide data I'm afraid. You didn't account for all of it by a long way. The worldwide regiment, there were only 16 stations activated, and of these only 6 made measurements all during the period, which was from April 1st to past the middle of June. The other nine were not activated until about the time of the 4th shot, and they reported for the period after that. The maximum fallout observed was in Scotland, about 610 disintegrations per minute per square foot, again extrapolated to January 1, 1953; minimum is some question - one station in this nine which didn't report for the whole series, reported nothing. The lowest value from those stations was 6 and, making a correction for the fraction of material which had been put up in the last 5 explosions, one has to up these values to.... hm, so that... I can't up zero very well....8-1/2 gives you some idea. The same thing was done.....these were averaged in....with the average of these nine stations, by increasing proportionately the amount of stuff that went in. Now on that basis, the worldwide, well I should say further, these stations did not cover anywhere near the whole surface of the earth, even as sparsely as you can imagine in number. Actually they covered a belt beginning roughly on the eastern boundary of the United States, extending eastward to about Germany and Tripoli in Africa, and the latitude variation was from 60° north to 35° south latitude. This accounts for about 40,000,000 square miles, that is the total of the United

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States plus this. That's about a fifth of the earth's surface. The totals then for the extra United States then gave this value for the concentration about $.27 \times 10^{-2}$ curies per square mile and the total was oh, say, roughly 10^{-1} megacuries. Now this accounts for about 5% of the total activity put up, or in other words a total accounted for about 13.7% of the activity put up was accounted for in this 40,000,000 square mile or a fifth of the area of the earth. From here on, as far as summarizing TUMBLER-SNAPPER, it's a matter of playing around with numbers a little bit; I think it's frankly constructive. May I erase this?

LIBBY: Will people notice that the seminar is this afternoon. We have collapsed three of the -----

HILL: What I did was the following, to get some idea of whether this in reality really accounted for a large percentage of the total activity put up worldwide. I assumed first one could take the remaining area and divide by the quantity of activity that is not accounted for here, and you can work backwards and find what number of counts you would have had to have on a sample on an average for the rest of the world to account for the total activity, and this comes out to be, I think, a completely unbelievable number, 852 disintegrations per minute per square foot. Do you agree with this, Merril? That compares, you see, with the average in the United States; it is about half of that and it is 4 or 5 times that for the average for the area outside the United States. Furthermore, this area, being just to the east of the U.S. as far as the

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world is concerned, and in the northern hemisphere primarily, the prevailing winds are largely from west to east, so that this would be the area which would get, one would expect, the next biggest fallout if you were to say, divide the earth into fourths, b y great circles owing to the meridian, so that it's completely unbelievable that one would have found that much for the average of the unsampled part of the world. A more reasonable value, I guess, from looking at the distribution of the smaller samples of the worldwide, as apart from the southern hemisphere which is largely neglected, and the polar regions, would be something like 20 disintegrations per minute/per square foot from the rest of the world. If you use this value, then you wind up accounting for all the activity...I'm sorry, only about 13 or so percent of the activity - 15%......
Now if you take the, just take a guess, say well, suppose the paper were 50% efficient. You still come up for the necessary to balance up the books, of 350 for the rest of the world.

LIBBY: Let me see whether I understand your statement. You were telling us that there is stratospheric storage, that the activity doesn't fall out.

HILL: I don't think one can safely draw that conclusion from this value. One can say that either of the order of 80% of this stuff stayed up in periods longer that the period of measurement, or the efficiencies of the collection of the paper are the order of 10 to 20% ---

SCOVILLE: Or the third possibility is that rain is the major aspect of the

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S. ILLE: (Continued) fallout and that isn't in the picture at all here.

HILL: Well, how about that Merrill, I get the impression that you feel that in rainout you really collect _________ on your gummed paper.

EISENBUD: I think we have good evidence for that.

HILL: There certainly would be a question though as to whether the efficiency of collection would be the same during rainout as during dry fallout.

EISENBUD: Well, you know in our earlier report, I think it was the original BUSTER-JANGLE report.

Y ZE: Yes, I remember that.

EISENBUD: In 7 square foot tray we collected rain water in which things washed into it. We ashed the whole sample and counted it. So presumably we got everything that came down. Incidentally these collections were made by Dr. Wexler's group. But, at that time we were looking for something simpler than a total collection because of the difficulty in this number of places that we were considering in this network. We had a little difficulty in collecting quarts of water, going through the evaporating procedure. Then we hit on the idea of gummed paper and we ran parallel collections. We found that the gummed paper results were more reproducible for one thing. This we attributed to the fact that when the tray was dry, of course, dust might blow off and, although the instructions were to always keep a

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half inch of water in the tray, in some places this wasn't done. The variability was in the dry parts of the country. Not only was the gummed paper more reproducible, but also yielded higher collections, even when it rained. This we couldn't account for. Water was being spilled in the course of transfer. These are things we have no control over. Over 100 different people, all using their own techniques in transferring from trays to jars.

LIBBY: Could we have a little description of this gummed paper.

EISENBUD: Gummed paper uses the same adhesive which is on the Kum Kleen label, which you can stick on laboratory glassware and pull right off. It has very nice properties. It retains its adhesiveness over a wide range of temperatures even when wet. Our interpretation of the fact that we get a highly efficient collection when it rains, is that the particulates are impacted on to a gummed surface and stick, letting the water run off. Now, we do know that 50% of it is running off. Of course, our interpretation of that is that this 50% is in solution. It is made up of solids in the forms of very fine particles. But the percentage that runs off is fairly uniform - a lower limit about 30%, upper limit about 80%.

LIBBY: Then you would not necessarily disagree with Mr. Long's statement. Mr. Long suggested the missing 80% would be rain which you didn't catch on gummed paper.

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HILL: This gets a little tricky now, doesn't it Merrill? That is any given sampling station, the fraction of the fallout falling out depends on precipitation and will vary from station to station, so the actual collection efficiency for that station during the whole period might be quite different for the different stations.

EISENBUD: I think you will find from an analysis of the data, that once you get beyond 200 miles most of what falls out is carried down by precipitation. In a station where it rains on the average, fallout would be about ten times as high as the fallout in the case of a station where it didn't rain.

HILL: Then, you would say the collection efficiency for a practical point of view would be what - about 50%. Well, on that basis, then ..... . That still would leave you, if you use 50% for the efficiency, with a considerable unaccounted for. I haven't worked it out on that basis, but it must be on the order of 70 to 80%.

LULEJIAN: Do we take any ocean samples here?

EISENBUD: No

LULEJIAN: You don't have the fallout on the surface of the water, then?

EISENBUD: I understand that in the TUMBLER-SNAPPER analysis, the extrapolations, on the basis of data from fifteen stations of which only six operation, continiously, we were sampling only

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E[illegible]BUD: (Continued

six square feet, one square foot per station......

LULEJIAN:

Do you really think that the data you have, the number of samples you have, clearly is minimal to the analysis of the...

EISENBUD:

One of the striking things about these data in the U.S. is the uniformity of the way in which data can be related to distance from the test site.

LELEJIAN:

Can you then relate this to the rest of the world, is my question. Why are you worried when you can't?

EISENBUD:

Well, I think there are ways in which this can be analyzed. I don't think we ought to take the full group. I doubt if this comes to an answer too different from that.

HILL:

There certainly are different ways of going about it. However, I think one has to say to be fair, that as far as the worldwide situation is concerned, TUMBLER-SNAPPER, it's only the barest indication. It looks as though maybe even in this where four out of the eight shots were tower shots, you still perhaps have not accounted for quite a bit of the activity put up during the time period of measurement.

BETHE:

The question of the ocean was raised. The area of the North Atlantic Ocean, I would imagine, would have an activity intermediate between the U.S. and Europe and, therefore, very high. Was this taken into account?

L[illegible]:

This, essentially, was what was taken into account, particularly area in weighting the values.

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LIBBY: Well, it seems to me that though this would be more profitably pursued in the panel this afternoon on particle size and scavenging, I would like to hear your answer to this question: Do you believe your data indicate atmospheric storage? You do believe so?

HILL: I think if you wait, Dr. Libby, until the Ivy it will become quite clear that......

LIBBY: No, I want these TUMBLER-SNAPPERS.

HILL: I see.

LIBBY: These low levels.

EISENBUD: The trouble with all of this is that so many of these questions are raised after the fact. Hindsight is always so much better. I'll show you what the basic problem is. I don't know whether we're going to be able to get around to it or not. If we plot as a function of time, taking observations every day, the data activity per square foot at these places, we find that before the test series - and this isn't true any more, as we'll discuss later, I'm sure, because of the marked effect on Ivy tests - prior to Ivy, we get essentially zero values in the fluctuation above and below zero reflection of the fluctuation of background counting. Then, all of a sudden the activity was spiked up to a very high value, which might be 10^2 or 10^3, sometimes as high as 10^7. Then it would drop very rapidly on the following day and it would remain above background for a

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EISENBUD: (cont'd) while, but the integration of this curve that we would get would be such that for all practical purposes all of the activity would fall out in one or two days when the cloud was passing over this area. When we went to pick it up on the second time around, the contribution into this total was so minor that we neglected it. In the laboratory we had the problem of counting 300 to 600 samples a day. We decided arbitrarily on the basis of our experience with this sort of thing that we would neglect counts of less than about 30 disintegrations per minute per square foot. Remember now the things that were contributing to the totals for the test period were counts of the order of thousands to millions. Now this was all right, except for the fact that after Ivy we observed a continuous drip - the only way I can describe it - which went on until the beginning of the UPSHOT-KNOTHOLD series and now we're picking it up again as UPSHOT-KNOTHOLE disappears. We find again that Ivy is back in the picture. This drip is in the order of only a few counts per day, 5 or 10.

HILL: Is that extrapolated, or actual?

EISENBUD: Actual. Remember now that this is six months after the fact, after the detonation, so we're dealing with long lived isotopes. This is only a few days after the detonation. These 3 or 4 or 5 d per m per day we record at the end of the month represent a very significant fallout, and since this drip is not perceptibly diminishing we expect it to go on for a long

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EISENBUD: (Continued) time. We may never know how long it will go on because there's another set of big shots coming up that's going to mask it. But there is naturally a question. For Ivy there is definite evidence of atmospheric storage and agin in hindsight, going back to what we thought were background counts, we do find drift from the early test series which we ignored at that time.

VOICE: What was your average background before Ivy?

EISENBUD: Well, the instrumental background in our laboratories was on the order of 6 counts per minute. The actual background of the fallout papers prior to Ivy was probably one count a minute. I mean there is an indication that there was a --.

LIBBY: Plus or minus?

EISENBUD: No ...

LIBBY: You get some stuff from radon, do you?

EISENBUD: Well, this doesn't bother us, because these counts are made several days after collection.

LIBBY: I see.

EISENBUD: And the natural decay products are eliminated.

LIBBY: You take your square foot of fly paper and combust it and put the residue - this is the way you count, is this right?

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EISENBUD: Yes.

LIBBY: You have a pretty good counting geometry, maybe 30%?

EISENBUD: It's a little better than that, we have a very good system which gives, well, 45%. We count to 640 count now, but we usdd' to count to 20 minutes, but we didn't get 640 counts in 20 minutes.

SCOVILLE: Merrill, how do you calculate your total beta megacuries from your disintegrations per minute per square foot. That might lead to considerable error.

HILL: He didn't do this, I did.

EISENBUD: Well this is a good point, because we are counting in order to expedite the large number of samples we handle each day. We seal these samples into polyvinyl strips on tapes, and we have 100 samples per roll, and count them automatically. The total absorption is around 15 miligrams per square centimeter.

LIBBY: And your window?

EISENBUD: The window is negligible, it's of the order of 1.5 negligible or 1.7. So we are getting some absorption of soft betas.

LIBBY: This is particularly serious on the Ivy drift as you call it there is a longer life involved. These are softer.

EISENBUD: What we had hoped was that our accounting methods which involve systems which were selected on the basis of expediency because

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EISENBUD: of the large number of samples involved could be re-evaluated in terms of more absolute methods which other people have used for example .

LIBBY: Yes, I think there is a good possibility of that. You did keep good notebooks, then you can always go back and, by standardizing, this would be very helpful if you could do it fairly soon, it isn't a very big job, and write down as a function of energy what your counting efficiency would be and then convert back to betacuries per square mile as a result of this. That would give us a number which might differ appreciably and might be more valuable.

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LIBBY: was that it could account for 1% of the activity put up at Ivy from the worldwide sampling and here while a number of stations still leave something to be desired as far as covering the area of the world, you still have a much larger number and they were much more widely distributed. In this case the areas actually used in determining the average from these 49 stations plus the U.S. stations were a 111 million square miles, say roughly 200 million for the world. This is an indication that probably there were large quantities of material that stayed in the upper atmosphere for a period longer than the 61 days which Merrill says he is now finding. Clearly, if you compare these two, even if you assume that this sort of thing is indicative of the real efficiency of the gum papers, you still have 90% or so of the Ivy which is unaccounted for.

LIBBY: Do you agree with these numbers, Mr. Spence?

SPENCE: I neither agree nor disagree, I am interested.

LIBBY: It doesn't shock you? That is, that 1% number - does that shock you?

SPENCE: I have no way of knowing whether 99% straight up or not.

EISENBUD: May I ask how much of these percentages were arrived at. It was agreed that the question of efficiency was involved and you think it is about 50%. Does that require that you multiply the 8.6 by 2?

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I [illegible]: Actually taking them at face value the efficiency turns out to be about 11%.

EISENBUD: Well no, I am going at it the other way. What happens here is that if you assume that half of the fallout is within 200 miles the defficiency is 50% and then it comes about 35% in the case of TUMBLER-SNAPPER U.S. which I don't think is a bad estimate of the fallout.

HILL: But, according to the fallout within 200 miles that I calculated, there was only about 5% of the total within 200 miles.

VOICE: I was told that few if any of the collection stations for Ivy at the test site were in the line of fallout.

[illegible]: Well, it is my understanding, and you people can correct me, Bob Rapp - isn't it true that the activity from Ivy spread out all over the northeast and northwest Pacific pretty thoroughly?

RAPP: Yes, it did spread out, but the point is that is there were some secondary fallout matter, such as you observed from Nevada, as Colonel Lulejian has measured, you would miss this completely in Ivy because this would be in a large section of ocean.

HILL: Certainly one must take this into account. A very informative opinion here is that taking a total number of stations and even considering that they are heavily raided on the U.S., you

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I [illegible]: (continued) still only have about one station for every 4 million square miles, and this means that you never can be sure of this sort of thing - that there wasn't considerable amount of activity coming from the areas that you just don't know anything about.

BETHE: There must have been a very stron maximum observed, wasn't there?

HILL: Yes. I think that it is worth mentioning. This 330,000 was the total for Iwo Jima over the 61 days, extrapolated from January 1, 1953 and actually most of this is accounted for by the fallout on one day, I think it was M+4, where the count was over 2,600,000 or that order.

VOICE: Was it raining on that day?

HILL: Yes, it was.

EISENBUD: It rains every day in that part of the world, I guess.

VOICE: Have you added in the close fallout in Ivy at all?

HILL: I have no information on it.

LIBBY: That is why I was asking you, Dr. Spence, about this.

SPENCE: Well, I have a very difficult time relating the close-in fallout ____________ quantitatively to this data. However, it might be possible.

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B[illegible]: I think that, while there was fallout, that even on the islands in the Atoll, you were able to go in fairly soon afterward so that it wasn't an extreme problem. Is that right?

SCOVILLE: NRDL made some measurements. The fallout was quite appreciable, I suspect it is a much bigger faction than what you are talking about here. Certainly more than 1% of the total. This would dominate all these other matters.

ADAMS: The station layout was not directly in the line of the clouds. It was pretty much upward, rather than downward as I remember.

CADLE: Well, the stations at Ivy were layed out within the Atoll, whereas the fallout pattern then went to the northwest so that they got crosswind fallout which amounted, as I remember, within a few hours after the event to perhaps 8 to 10 r per hour out to distances up to 10 miles or so.

HILL: I would like to get hold of this data and include it if it is possible. However, again it might be fairly misleading. I suspect that is is a much bigger fraction of the total amount of fallout than what shows up here. Well, you have to be a little careful there. The earth is a lot of square miles and if you, depending on how much area you consider and what fallout you consider a representative part. You see, in the case of the Nevada test for the four tower shots you would say, this is a lot of activity coming down in this area within, say, 200

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HILL: (Continued) miles of the test site. Actually, the intense part of it didn't cover this whole area by any means. There was a strip under the downwind direction of the projection of the cloud and, on the basis of Colonel Lulejian's values, this only accounted - for more than the total fallout in the U. S., it is true, but it still didn't account for a large fraction of the total produced.

SCOVILLE: This is despite the fact that you added in four air burst shots of which almost nothing landed close in, and the Ivy shot, the big one, was a surface shot.

HILL: On the other hand it went awfully high.

SCOVILLE: That is true. On the other hand the radiation contours scaled not too badly from JANGLE.

HILL: Did you actually get contours from this?

VOICE: Yes, the data is very poor, which everyone will admit, but they are not layoff by orders of magnitude from what you would scale from JANGLE.

HILL: Have you tried to analyze to find out what percent of the activity?

ADAMS: I don't know whether NRDL has or not.

VOICE: Have you analyzed the contours at Ivy?

ADAMS: You mean the integrated fallout? No, I didn't work on that particular project so I don't know, I just repeat these figures

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[illegible]MS: (Continued) from memory from reading the report. I might say though that the difference - I wonder if anybody here has observed the fact that at the surface shots the larger amount of the activity becomes incorporated in large pieces which fallout more rapidly and closer in than the tower or in airburst so that at Ivy you might expect a much larger percentage of fallout.

LIBBY: Yes, that is what I was hinting at - that if all that coral went up .

SPENCE: Well, I don't believe that would change any of your conclusions. I am sure that it was larger perhaps than fallout and perhaps even surface JANGLE, but I don't believe that it is more than - well suppose you took a figure like 10% or even 20%, it isn't going to change your conclusions any, is it? I don't believe that it was way high like 50%.

HILL: Furthermore, you have Eisenbud's evidence that it is still coming down - it must be up there, a lot of it.

EISENBUD: Another interesting thing about that. You take a U.S. statement over a two week period, which we are looking at at the present time, and find that the fallout during that two week period, extrapolated now to 1 January 1954, will vary from say 10 d per m per square foot to say perhaps 100. It seems to be quite a spread in view of the diffuse nature of the fallout. It may be attributable to rainfall at these collection stations.

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H :   Do you think that if you were to take this data you were getting now, and you got before at UPSHOT-KNOTHOLE, and sort of fit it in between, by guess, and extrapolated back to January 1, 1953, would this be a dangerous thing to do? It probably would, wouldn't it?

EISENBUD: I would make one statement on the basis of what I have seen from the data so far. That is if more than 1% can be accounted for in the period M+61, I guess this is up through M+90 something which was the start of UPSHOT-KNOTHOLE.

HILL: I would appreciate it if you could get me any of this data that I can analyze.

EISENBUD: Well, we are rushing to put it together as fast as we can.

GRIGGS: I don't quite understand this. Do you mean to say that more fell during the time between 60 and 90 days than fell in the first 60 days?

EISENBUD: No, that is what I said but that is not what I meant. To exclude the initial fallout which occurred on the first pass around the world, which I think took 18 days. So the comparison of about 30 days against some 45 just doesn't seem right. I would say they are the same order of magnitude.

GRIGGS: You have no indication of a fallout or rate of fallout?

HILL: It would be very interesting to try to keep track of this,

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HILL: (continued) because if we once get some idea of how long a period of time this took to come down, or a reasonable fraction of that time, this would be important from a biological hazard point of view. If it stays up there several years it certainly means that there is a stronger tendency to get more uniform fallout over the world. Also its decaying some and if it should happen to stay up, even a reasonable fraction of it, for a half life, well then you would really get a demunition and a hazard. There are a few other things in the Ivy test that are quite interesting. The time of first fallout observed in the U. S. occurred here in Los Angeles and it occurred about six days after the shot. The station receiving it last in the U. S. was Atlanta, Georgia, and that was some 23 days after the shot. The stuff was diffusing, by this time fairly slowly. Furthermore, the stations up in Main, the extreme corner of the U. S. got it earlier than did Atlanta, Georgia. It must of gone over up there and sort of either curved down or a tail of a bulge came in later down in the sourthern part of the U. S.

BETHE: This couldn't have been the second way around?

HILL: How about that?

EISENBUD: No, I think that it is two cloud. One went east and one went west, and I think the cloud that passed California was a very small component of the total that it also picked up at Hawaii and Wake Island, I believe, and possibly the west coast of the U. S. The bulk of it headed east and I think probably hit

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EISENBUD: (Continued) Main first because it sticks out a little bit.

HILL: It is interesting in that connection, that the minimum fallout observed in the U. S. from Ivy was 420 and this occurred both at Los Angeles and San Francisco. The maximum value was in Fort Worth, Texas. I would think that this would bear out your statement that they were different clouds representing one of smaller fragments.

SCOVILLE: Has any attempt been made to correlate the rainfall which occurred at the collecting stations with the average rainfall in these areas.

HILL: In the reports I have, Eisenbud's report on Ivy, there are only a few stations. I guess it was the TUMBLER-SNAPPER, you had a few stations bar graphs plotted with the precipitation underneath. Looking at these I couldn't see for sure that there was a correlation. Some days when it rains you would get a peak and other days you would get rain and no peak. I think that it all depends on whether the rain occurs when the intense part of the cloud is there or not.

LIBBY: Or the level from which the rain falls.

SCOVILLE: There has been no attempt to study this?

HILL: Not as far as I am concerned. I don't know whether Merrill has done much of this or not.

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EISENBUD: Dr. Westen's group is doing this and is going to write in Part II of the report.

BELL: The biggest fallout was in the last two Nevada tests and also that big one in the Pacific. Rained and water samples taken throughout Massachusetts came from the Able-Six blast of this year and the thing I would like to ask - what appears to me to be a fair example of rain in east Massachusetts when these clouds came over and in eastern Massachusetts from about 10 stations. We got a variation in the activity of the rain of about 65 times. You can't put too much basis on a station in one small area.

HILL: Well, there is some indication of that in Eisenbud's report. For example, there were two stations in Los Angeles, and I think that one of them was at the airport and one somewhere else. The low value given there is 480, the other one was in the neighborhood of 1,000 or 1,100 as I remember it. You do get great sharp variations in small distances, this is for sure. I don't know whether any attempts have been made, in this case there probably was no rain. The probability that there was rain getting this was pretty small. Do you know about this?

BELL: I don't know.

HILL: It might be accounted for by differences in rainfall. They are very sharp out in this area, for example.

BETHE: If there are such big fluctuations couldn't it be that you have missed one place where there is one hundred times the intensity and which contributes?

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LL: This is always the possibility that you can't avoid completely. You have got to consider it as a possibility. I think it is rather unlikely. The great difference between the Ivy shot and the Tumbler-Snapper shot, using the same methods for collection and so on, and the fact that most of the conditions of the Ivy shot, the fact that the cloud went so high and so on, would tend to produce this longer duration of the material. The fact that Mr. Eisenbud is still getting it, indicates that this probably isn't in the wrong ball park, but you never can be completely sure that you haven't missed the hot spot.

BETHE: Eisenbud's present findings, of course, show that there are a lot of activities, still up, whether that is higher than 90% or 10% they don't tell you.

HILL: And I don't know whether you would ever be able to sample densely enough to find out in a practical way. How do you feel about that, Merrill?

EISENBUD: Well, if you could hold up Castle for about two years you could get the answer, but really I think that the prospects of Castle coming on early next spring or late in the winter. It is not going to be possible

HILL: Well, one thing is clear; the large areas of the ocean where it is rather impractical unless you park a ship out there and let it coast around and use it for a fallout station to get the

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LIBBY: (Continued) data. Also the polar regions are rather inaccessable. Also the whole area behind the iron curtain which is a very large fraction, even of the total of the earth's surface is not available to you, so you have problems.

WEXLER: How about nature's integration of watershed collection, runoff in rivers. Would there be any runoff of this material, Merrill?

LIBBY: Well, Mr. Bell has some information and I think Mr. Theis can make a remark. Mr. Bell.

BELL: Ours is very preliminary, but it looks to us from this Able-Six blast that we tried to get some idea of a runoff coefficient that we could use in these activities, and we came to the conclusion that the stuff, as far as we could measure, was not runoff. We only had to worry about that which hit on the water. That was very preliminary.

LIBBY: Does this make sense to you, Mr. Theis?

THEIS: I want to point out that the eastern Massachusetts area is an area in which there would be less rapid erosion than in other parts of the country and so that the lack of erosion of the fallout material of the soil, which might carry the fallout, might be correlated, but that I don't know.

LIBBY: You are saying that muddy rivers might have stuff in them because it has gone along with the mud.

THEIS: I think so.

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BY: It seems highly probable.

HILL: This would make it interesting to look at the Lake Mead situation, wouldn't it?

THEIS: Or some of the samples taken in the Grand Canyon silt stations.

GRIGGS: Aren't there two factors in this problem; one, that of the errosion and transport of material and second, the time lag between rainfall and appearance in a stream down below. In Massachusetts isn't the second factor also important?

THEIS: I don't quite understand the second point.

GRIGGS: As I understand it, in glaciers we expect water to fall, it is stored and later appears down water with a time lag of many months.

VOICE: Yes, that would be a possibility. Much of the water would be evaporated.

GRIGGS: It is not necessary, for your criterion, that the material be physically eroded. Like in a place where you have rapid runoff with or without erosion you might get more concentration.

THEIS: I think that is so and I think that the nature of this material would be very important. If it is ionic in form it will probably be absorbed. If it is some of this fine ultra-small particulate matter, it might go into the ground water and be carried.

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[illegible]P: Well, a solar action would probably be at least as important, if not more important, than the time delay on that.

THEIS: I think it is very important and also the ion exchange characteristics.

HILL: Well, a lot would depend too on whether the stuff was soluable or not.

LIBBY: Well, from what Mr. Theis has been saying about the ion exchange property to average soil it can be soluable and still go out on the soil. Of course, alkalis and things like that but if it is any polyvalent ion from what you say it would just go out on the soil, apparently.

HILL: Would this be likely to be in the very surface layers that are important agriculturally or would it have to be spread pretty deeply.

THEIS: Well, if it is dissolved I think it would be in the surface layers and that has been the indication of most waste products of reactors, but if we have some of this new form of matter some of these little corrundum beads I think it will go down deeper in the soil.

LIBBY: We are pretty well convinced now, Mr. Theis, that our most dangerous isotope is not going to be in the form of a ruby. The ordinary fission products may be but not this stuff. This stuff is likely to be soluble so it will probably be in the surface soil, from what you say. It looks as though we have to

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BY: (Continued) sample soils. This is chemically a very difficult problem. Take a barrel of dirt and treat it.

HILL: I have just a few more remarks. Time is getting short and maybe I had better put them on. On the basis of this it is important to ask: are there any lessons here as far as a present estimate of the worldwide hazard? If you go on the assumption that Strontium 90 is the dangerous character, it is of interest to calculate the total amount which has been produced in all the bombs that have been shot. Actually, I did adjust for the 44 that have been shot in the U.S. This gives approximately 9 kilograms of Strontium 90 that has been produced since the Almagordo Shot. If you calculate the concentrations per square mile you get, say, 40 to 50 micrograms per square mile. From looking at the results here on the worldwide Ivy you see that the ratio here can be rather large from the maximum, say, to the average which is almost 100. Also, you will have areas which will have almost none as is indicated by some of the small fall-out observed. So that this could be a wide range locally. However, Dr. Mitchell has made an estimate and he will talk more about this later, I just put it out for comparison that 150,000 micrograms of Strontium per square mile gotten into the biosphere and into man and under equilibrium conditions over a 20 year period would be about tolerance level on the basis of - what was it, about $6x10^{-3}$ microcuries fixed in the skeleton?

MITCHELL: Micrograms, in other words.

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R .: That is right, so that it doesn't looky on the basis of this, as though we need to worry for the moment, but there are large ranges of uncertainty. I can't help feeling, in spite of what has been said about the effects of errosion and weathering, that there are a lot of factors in nature which will tend to put this stuff where it is not dangerous as if it were put down uniformly and just left there. At the bottom of the oceans and probably even to some extent in the upper atmosphere.

LIBBY: I can't believe it would stay up there so long as to decay.

HILL: Well, we don't know.

LIBBY: Well, perhaps we have learned one lesson from this look into the data available at the present and that is that there is stuff in the atmosphere and it does stay there for quite a while so world-wide dissemination is extremely likely. This is not a minor conclusions.

HILL: Oh, another thing that is involved in this is the effect of fractionation which I think it would be very interesting if you could, in your samples, get some indication of the fraction of the total activity there that is due to Strontium 90 as a function of time after the shot or distance away from the shot.

LIBBY: Do you still have those samples, Mr. Eisenbud?

EISENBUD: We have only a few of them. They are being processed.

L BY: Oh, excellent.

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.L: Do you have anything to say on those yet?

EISENBUD: Well, I called yesterday when that question came up and they still don't have the numbers. I hope to have them by tomorrow.

LIBBY: Well, that will be very interesting.

BETHE: In the Ivy outside U.S. you wrote two rows of numbers, what do they refer to?

HILL: Oh, well the situation there is that there was some 86 million square miles in which there were no stations at all in and what I did was take the stations nearest, say the ones down below the equator and above latitude 64, and average all those and maybe this is a good representative value for the rest of the world. I don't think that it is but this is one thing that you can do. In any event it is probably high, I would say, because all the indications are that the value of the fallout, as you go to the north and south latitudes, falls off rapidly and a large fraction of this is in the polar regions, of course, and particularly the region below the equator which also indicates that certainly when you get away from the longitude of Eniwetok the southern stations are much smaller in average. It still accounts for less than 10%.

BETHE: Can you tell us once more how the megacuries are normalized and time?

HILL: Yes, what you do there is take the expression out of the Weapons Effects Handbook which gives you the number of beta curies per KT.

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L: (Continued) It is 13.? megacuries per KT times T to the minus 1.2, is the relationship.

LIBBY: inaudible

BETHE: And so this is really normalised at one day.

HILL: Well, what I did was to extrapolate to this January, 1953 date the same as the samples were.

BETHE: These numbers are the coefficients.

HILL: They are normalized to the numbers given in the Weapons Effects Handbook for one day.

EISENBUD: Does anyone have any data on the rate at which volcanic explosions are eliminated from the upper atmosphere. It seems to me that there is a possibility of getting that data from solar constant observations.

WEXLER: At Krakatoa there was one solar observatory taking readings in France. It took the dust at Krakatoa which is 6° south about three months to effect the radiation values at Mount Pelier, France and the values stayed below the normal for about three years. That, of course, would mean considerable quantities of dust, enough to interfere with radiation and it still might have been quantities for many years afterward which would not show up by such crude measurements.

EISENBUD: Is the data good enough so that we could plot a curve which would indicate the rate at which the dust has been eliminated?

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. .IFR: That would be very hard to do from just one point station.

GRIGGS: I thought that there was an observatory in the Sahara at the time of Krakatoa.

WEXLER: No, that was Katmai, the Smithsonian station in Katmai, 1912. That was effected by Katmai.

GRIGGS: Well, this showed peaks as the thing went around the world several times.

WEXLER: It may have, I don't know. As a matter of fact Mount Pelier shows the same thing. It wasn't a uniform depression, there are ups and downs. The average was about 10% decrease in normal for a period of three years.

HILL: Would it be detectable for periods more than three years?

WEXLER: Not by such crude measurements as pointing a tube at the sun. With one of the spectral measurements, probably yes.

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VOICE: We'll speak now about the sampling problem. It has many aspects, so we'll have three or four speakers. The first of these is Dr. Mitchell, who'll speak on Sr^{90}.

MITCHELL: I just thought I would give you a few brief remarks about the relationship of Sr^{90} and calcium-strontium metabolism in humans as we have put it together for purposes of this project. Sr^{90} is the critical element involved in the long-term hazard, primarily because of its bone seeking properties. Since we know that bone is one of the least active of the structures of the human body so far as dynamic equilibrium is concerned, we're able to state that these elements which are bone seeking tend to get into the bone and stay there. If we follow calcium or inert strontium or also radium we have figures which enable us to indicate that after an initial uptake of the element that the material becomes mineralized, and that after it becomes mineralized, its rate of release from the body is quite slow. Whatever radioactive material is present will exert its effects over a long biological half-life. I think for most of our considerations here we have essentially considered strontium 90 as being deposited in bone and staying there. Calcium is a very important element in the body, and 99 percent of it gets into the bone. The other 1 percent which is concerned with nervous reactions, and various membrane reactions, etc., we can neglect for purposes of this project. In order to relate the

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MITCHELL (continued) strontium problem to the Sr^{90}, in other words the inert strontium which exists in the environment, to the problem of the Sr^{90} contamination, we have run up against the fact that not very much is known about strontium metabolism. It is presumably just a trace element without any metabolic functions and has not been studied. So what I have tried to do is to take some of the known values with regard to calcium and with regard to the analyses on inert strontium and give some idea of the quantitative aspects with regard to the deposition of radioactive strontium.

One of our figures, using the standard man as having 7,000 grams of bone with 1,000 grams of calcium is an important one to remember. During the process of growth of the individual that would be about the figure you would get. Calling this calcium in this age during the growth years 20 years, and this being 1,000 grams calcium. So that a growing person would accumulate calcium, starting at birth with about 25 grams of calcium, and going up to 1,000 grams of calcium by the time he reaches 20 years.

LIBBY: What percentage of the total calcium he ate is that 1,000 grams?

MITCHELL: That would vary with the diet. He's accumulated it at about 100 milligrams a day, on the average. The body will tend to keep calcium in equilibrium. That is, if you take in an

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MITCHELL: (continued) excess amount of calcium more will be excreted.

LIBBY: So what is normal calcium content of the food per day - do you happen to know that?

MITCHELL: No, but about .45 grams would be the minimal intake to keep a person (adult) in calcium equilibrium.

LIBBY: So it's about 20 percent as a minimum efficiency.

MITCHELL: Yes, the bulk of calcium is excreted in the feces. This will vary considerably with age. For instance a growing boy in this part of the curve will need about 1 1/2 grams as a minimum intake to keep his growth rate going. That is the recommended intake and that has a margin of safety that the nutrition people put in. The bulk of the calcium gets excreted in the feces if you have a high calcium diet. If you get on a lower calcium diet down to the point where a good deal of the calcium is absorbed, then the bulk of the excretion in negative calcium balance will be in the urine.

LIBBY: Its still about 80 percent excreted even in the negative balance. That is, you said 450 is a minimal and you take in 100. Is that right?

MITCHELL: Well, for these people let us say you had a person taking in no calcium at all, a certain amount of calcium will be excreted and that we'll attempt to reach a minimal figure and

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MITCHELL (continued)

I don't know what that figure is, but you would certainly interfere with the growth of the person and growth of the bones. But at least you can say that 100 milligrams per day, on the average, will get the person up to his 1000 grams in 20 years and the actual rates vary. I have these curves if anyone wants to look at them, incidentally. Starting for instance at the age of one year, the person is accumulating calcium at the rate of 150 milligrams and that goes down to the age of 4 to 5 - to where he needs 50 milligrams of calcium per day laid down to maintain a normal growth rate. During the adolescent years it goes up as high as 250 milligrams of calcium. So if you are interested in any spot checking or any immediate effect of a short-term, you would have to take the age of the person into account. But on the average of 100 milligrams per day would get you up to that 1000 gram calcium ceiling in 20 years (the growth period). The reason I have used that figure is that our analyses show that approximately 700 or 7/10 of a gram, 700 milligrams, is the amount of strontium which is found on analyses of human bone, so that in the absence of any further data we just assumed that strontium is being laid down in the same relative proportion to calcium, and so using the calcium curve you can then make an estimate as to the amount of strontium which the body is depositing in the American environment. That is, these are analyses

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MITCHELL (continued) that come from bones of people who died in 1914, who were preserved as cadavers, and then various people who died in 1949 at various ages, were analyzed, and the figures are all of the same order of magnitude, approximately an average of .02 percent of strontium, so that whatever variations there were in the strontium intake and the diet, as opposed to the people who died in 1914, who were all adults which would indicate a dietary regime in the 1890's etc., etc., and the current people who died in 1949, covering everything from foetuses up to adults, but there was no really significant difference in the amount of strontium found in the bones of those people. The data actually stems from some work that was done at UCLA by a worker by the name of Hodges, and it is quite interesting that this thing was of such a small order of magnitude, and the total range was rather small, that is, leaving out one unusual case of hydrocephalic infant, the range was .017 to .029.

LIBBY: Is this percent of body weight?

MITCHELL: No, this is the percent of strontium in bone ash.

BETHE: And that averages out to .Oy for strontium?

MITCHELL: No, .017

BETHE: But .07 gm

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MITCHELL: Oh, that will come out to 7/10 of a gram per 1000 grams of bone, we rounded it off at .02 percent. You see, in 7000 grams of bone we just roughly said approximately 50 percent of that was bone ash and that was the way we got the figures. In other words, .02 percent of 7000 grams of bone, saying that approximately half of it is bone ash would give us .7 grams per thousand. Actually, the study analyzed parietal bone, vertebra, rib and femur. The values are all quite close together, so that the metabolism of inert strontium in the body there is pretty uniform distribution of the material. In the laying down of calcium and also strontium in the bones, there is a good deal of dynamic equilibrium, and a good deal of variation in the parts of the bone in terms of growth. In other words, the medical people will know that the primary deposition of calcium will occur for instance at the region of the diaphysis and the epiphisis, or in the metaphysis, so that as the long bone grows, the bulk of the growth is at one very narrow portion at the ends of the bone. Then there will tend to be some bone laid down right under the periosteal and endosteal membrane in order to make the bone grow wider, so that there apparently are mechanisms for rather evenly distributing this material. It has been shown, for instance, that if you laid down some marked thing like a red dye at a certain time of bone growth,

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that that material will deposit and remain fixed in the bone, but its relative location in the bone will change. In other words, as more bone is laid down this material will tend to be put in toward the center because the bone is growing out, so if you were to fix radioactive strontium, for instance, at a particular point in the bone, the bone may grow around it, and this might be located in one portion of the bone, whereas if the person were subjected to a steady state of radioactive material going into the body over a period of years, it would appear to be pretty uniformly distributed.

VOICE: Dr. Mitchell, are you making a point that if a younger person incorporated it within his bone that is more damaging?

MITCHELL: Would you repeat the question please?

LIBBY: He asked, is it more dangerous in a young person because being on the inside its closer to the blood forming system?

MITCHELL: No, I wasn't particularly implying that, although I think it is more dangerous in a young person because the insult to a young person is going to stay there more than in an old person. In other words, there is going to be more of a turnover in the older person who gets the same amount of material than in the younger person. It is the younger

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MITCHELL (continued)

person that deposits and mineralizes a greater amount of it and less of it is going to be available for exchange which takes place. In other words, you can look at it in this sense, that the last amount of calcium in will be the first amount that comes out in any dynamic equilibrium which would exist, say for instance, in the demands of pregnancy, the woman will mobilise calcium. Well, she will mobilise the last part that was deposited not the first part, or not an equal part so that if that woman, for instance, has radio-active material which was deposited years ago, that is not the part that is going to be mobilized in order to provide material for the fetal development and also it won't be the part which will go into the milk. Now, there are animal experiments to show this. Rather large amounts of radio-active strontium which were put in and the amount which appears in the milk and the amount which appears in the fetus relates very much to at what time the mother was given the dose, not the amount she has to have. But the time at which it was in. So that if the material is really mineral-ised and a considerable period of time passes, even though she has a considerable amount of radioactive material there, it will not be passed in any great quantity to the fetus of the milk, whereas she might have a smaller amount which was put in say 10 days before she starts to lactate a large amount

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MITCHELL (continued) of that will appear in the milk so that the mobilizable calcium, and presumably other radioactive bone seeking elements are the ones which are more recently laid down.

LIBBY: This looks like an idea which we had for human sampling may not be so bad, this was to take miscarriage - in order to avoid legal and religious difficulty. Are you saying that this material would reflect the diet of the mother pretty faithfully.

MITCHELL: To some extent, yes. Now when you deal with miscarriages you have to be aware of the way calcium is laid down in the foetes. Now there is practically no calcium laid down in the fetes the first 4 months and then the fetel curve goes something like this (see chart). With this being 4 months, and this being 9 months, and this being say! 28 grams of calcium in the fetes, and about 60 percent of that is in the last 2 months.

LIBBY: Most miscarriages are under 7 months, aren't they?

MITCHELL: Well, I wouldn't know the figures on that.

LIBBY: Do you know that, Dr. Bugher? Does anyone know that? (chatter)

MITCHELL: Actually, that is one weight to weight basis. If you could get human milk from wet nurses, you would be better off.

VOICE: You would?

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MITCHELL: Yes, because the calcium demands of the fetus are 28 grams, where in a lactating period you get 80 grams of calcium going into the milk.

LIBBY: Well, that's very helpful.

MITCHELL: Well, now I don't know whether the amount of lactating milk is greater than the size of the fetus of not but I imagine it would be smaller.

VOICE: How are you going to do that on a worldwide basis?

MITCHELL: You'll have to ask the anthropologist that.

COLMAR: There are a couple of points about that - in your miscarriage material you have to realize that it has been indicated that the accumulation of strontium would represent only that which was there a short time before so that you would really be making a sampling over maybe a 2 or 3 months period.

LIBBY: Well, that's very good, the adult structure being a thing that is build up over 20 years, the pre-atomic period, we're not interested in that, we want something that is atomic in response, that does apply to recent times.

COLMAR: This would be alright, but your looking at a 3 months period but it depens on whether your environment has a continual

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COLMAR: (continued) exposure or not. Suppose you have a high exposure here for a few months then it drops down, you might miss the high exposure entirely.

LIBBY: Oh, of course, oh, certainly.

MITCHELL: One assumption a lot of people have been making and that is that at the peak of fallout, that time is important to human, it might not be, in other words, we don't know how long it takes for that material to travel thru the cycle before it finally gets into the human. That apparently is still pretty unknown. In fact, it conceivable that it might be piling up in certain places and not be getting to the human at all until it reaches a certain threshhold and then it might spill over in something like fertilizer or something like that. Those are just part of the unknowns that exist in this thing. Mainly, one other thing that we did was to take the assumption that the average American man going through life will accumulate this 7/10 of a gram of strontium. We tried to calculate what the ratio of radioactive strontium to inert strontium would have to be to bring a person to the tolerance level over the course of his lifetime of growth. We took the tolerance level as being one microcurie or 6.2×10^{-3} micrograms which is given by the International Commission on Radiological Protection, and we assumed the average daily deposition of 1/10 of a milligram and that works out to the fact that you would need a hundred million to one as the ratio

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MITCHELL (continued) of strontium to Sr^{90}, in the dietary intake of the person to bring that person to tolerance in 20 years. Now, that enables you to neglect all the unknowns with regard to rates of excretion and amounts of retention and things like that if you accept the figure of gradual accumulation of 7/10 of a gram.

BACHER: Would you repeat that figure again, sir?

MITCHELL: Pardon?

LIBBY: This is Sr^{90} to strontium, 1 in a hundred million if all the body strontium has that composition. This is tolerance. Is that correct?

MITCHELL: That would be a tolerance dose accumulated over a period of 20 years. Now if the person, let us say, were taking a diet like that at all times, then by the time he reaches his adult period, the amount of additional material he would be taking in would equal the amount he was taking out, in other words, the equilibrium there presumably would not cause any further accumulation of the radiation, presuming that it was a constant amount throughout the whole growth period and into the period of adult life. That would be one way of looking at this thing.

VOICE: What is the bio-half life of strontium?

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MITCHELL: No body knows - at least I don't. That is the biological half life, then you would have to get into the problem of retention and excretion rates, and there is a lot of information on that coming out now with regard to short-term effects with rather large doses and whether you can say that is the same thing which is going to apply with minute amounts over a period of 20 years, I don't know, but I would be inclined to say that you cannot extrapolate from that kind of data.

HOLMAR: I would think that best estimates would be similar up around 40 or 50 years. This is really just a guess based on calcium-45 elimination.

VOICE: The National Bureau of Standards uses 9×10^3 days, which is roughly 30 years.

LIBBY: Taking your figure, I calculate this as a 1 microcurie per individual as the tolerance, is this correct?

MITCHELL: Yes. We took one other set of figures which might be of interest to the people here, and that is we got some agricultural figures which indicated that there were 27,000 grams of strontium available per acre of land. Now, if you put 270 micrograms of radioactive strontium mixed into that, a person eating off that type of land, in other words getting

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MITCHELL: (continued) all his dietary intake from that, or you could put it the other way, that acre would contribute the tolerance amount to the diet, so that if all the land that a person were eating off of .....

LIBBY: 270 microcuries per acre?

MITCHELL: 270 micrograms of Sr^{90} and then assuming path way will be the same as this here.

LOVE: Is that the previous figure on the available strontium per acre - was what?

MITCHELL: 60 pounds per acre and it came out about 27000 grams.

VOICE: Available strontium (chatter)

LONG: That's the question, is that available strontium? Because there tends to be, at least I've got very casually the notes from an agronomist that there is very commonly quite a major difference between analyzable material and available material, you see if you've got ....

MITCHELL: Well, these figures would apply only if the same ratio held in radioactive material.

LONG: But there is no reason why one wouldn't think it ought to, that is to say if you've got a biosphere drift technique feeding radioactive strontium down on the ground, one might

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LONG: (continued) have a sizeable factor between the availability of this radio-active strontium.

MITCHELL: Well, that would have to be investigated if you wanted to use this type of stand. In other words, the assumption that I made in this stuff is that the thing is full of unknowns as to what the path way, particularly the quantatative path way of strontium is, and so I set up this model to take into the things we do know, which is that we accumulate 7/10 of the grams of strontium over the growth period and then look at it as a tracer experiment and say how much of the same kind of biologically available strontium you have to have, for instance, if Dr. Libby's idea about the ruby were correct then the whole thing would just be no dice, so that the assumption is that whatever amount you have times the factor of availability, that these figures would set up an accurate model.

VOICE: The experimental data ought to be available so that, well, because the soil people have done a great deal of base exchange work. Presumably only base exchangeable strontium is available and this would greatly reduce the amount of strontium 90 you need to put on per acre. It is very likely that 1 part available per hundred is available, something like that. The other one hundred becomes available through

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VOICE (continued): normal process of weathering, but this is a 20 to one thousand year time scale.

SOLOMON: Do you know the amount of calcium in the top soil? The total amount of calcium? I have a figure of 1000 parts per million, as the available calcium content in soil, and if you knew the total calcium then perhaps you could find the fraction of available strontium by using the same figure, but I don't know that.

MITCHELL: I don't know that either.

KRIEGER: The figure is 365,000 parts per million.

SOLOMON: Of total calcium, and if one figure of 1,000 parts of available calcium per million is true, then one part in 365 is available.

VOICE: In our laboratory we've done some base exchange work on strontium 90 using normal clay, normal agricultural clay soils, and we find that up as high as 30 percent can be fixed, unavailable.

VOICE: Did you say strontium nitrate solutions?

VOICE: No, I mean what .. (salt)

LIBBY: It is just a - just dilute aqueous solution.

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MITCHELL: And that would be made available.

VOICE: That's right.

MITCHELL: That would be the type of information which we would have to reduce the factor of availability.

EISENBUD: I have some different soils taken in different parts of the country shows that the exchangeable calcium varies over about two orders of magnitude. These units are in milli-equivalents per gram .8 to about 38.

MITCHELL: As far as I know this strontium thing is just a general average.

LIBBY: Do you have any comment Mr. Theis?

THEIS: I don't know a thing about chemistry.

SOLOMON: The available calcium varies very widely and I had to beat my guy over the head before he even would give me any kind of a figure, and he just said a thousand seemed reasonable for top soil, but it certainly could be off in the order of magnitude in either direction.

VOICE: Could we obtain variable information if we were to doperiodic analyses of fertilizer from sewage disposal plant such as the Milwaukee plant producing milorganite?

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LIBBY: He asked whether milorganite is a good sample?

MITCHELL: Well, I think it would be a pretty good reflection of the diet.

VOICE: I should think it would integrate rather large in one area of the country.

MITCHELL: In other words, what you would be getting would be an indirect estimate of the amount that is excreted, and the excretion is not going to give you too much indication of the metabolism of the body but it will give you a fairly good indication of the integrated amounts in the diet itself.

Before I get lost in the questions I would like to bring up one other thing that Dr. Libby suggested when he was out here the first time. That was the problem of inhalation of aluminum oxide. Now, though, you seem to think that is not too much of a problem, I'll at least mention it. Aluminum oxide would be the ruby type of thing with the strontium embedded in it and on inhalation, the possibility of a retention in the lung came up. I investigated the literature on that and there is a disease known as Shavers disease, Dr. Shaver being the original describer, in which material is actually set and fixed in a sort of fibrotic process in the lung, and so that possibility does exist then that aluminum oxide was set up but similar to a silicosis say, which would then fix the material. Then the strontium would be acting as a radioactive source for lung tissue rather than bone. There is absolutely no quantitative information

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MITCHELL (continued) to tell you at what level the aluminum oxide has to reach before the lung will respond that way. The Aluminum Corporation of America, in response to an inquiry of mine, has raised the question that maybe it is not aluminum but maybe its contaminated silican. Apparently there is the possibility that the aluminum oxide is not the agent with which we'd be concerned, I don't know. If you do want to accept the fact that aluminum oxide when inhaled will be fixed you can make certain calculations as to the amount in air which would be a hazard. Taking the original tolerance level of 6.2×10^{-3} micrograms, I took 1/7 of that as the tolerance level, because of the fact that the lung has 1000 grams, as opposed to 7000 grams of bone, and neglecting any difference in tissue response, the amount of air breathed in 24 hours is 20 million cc and over a period of 20 years that comes out to 1.5×10^{11} cc. A tolerance level would be accumulated in the lung if the air had a concentration 6×10^{-15} micrograms per cc. There again that's an assumption of complete retention.

PLESSET: Wouldn't you infer that for the particles to be retained they would have to fall within a narrow range of sizes, like 2 to 5 microns - found by the BW people.

MITCHELL: There are apparently all sorts of you might say arbitrary ways of handling that and I think the biological commissions just roughly state that 25 percent of inhaled material is

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MITCHELL: retained. Am I right on that Dr. Western? That for the aerosols study thing ---- just state some percent.

WESTERN: Mr. Eisenbud is our expert on that, in that field.

MITCHELL: Whatever the factor of retention is then you would have to reduce this number, since it is worked out on the assumption of 100 percent retention.

LIBBY: May I ask you a small question, Dr. Mitchell? Why Sr^{90}?

MITCHELL: You mean as the hazard?

LIBBY: Why is it the important isotope? I'd like for you and Dr. Western to develop this in the next 5 minutes if you can. Why is it more important than other radioactive materials?

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MITCHELL: Well, my impression of the problem resolves itself in the fact that as far as the insult, the smallest possible thing which we might be concerned with, that is the greatest hazard from the smallest amount, would be the carcinogenic effect of radiation.

VOICE: What does this term "insult" mean?

MITCHELL: Well, it means damage to tissues -- damage of an undescribed sort. We are seeking then the threshold, in other words, the point at which we are concerned on the lowest possible level of radioactive or any kind of material. You very rapidly get down to the fact, for instance, in the bomb condition that it's going to be radioactivity, since dust and climatic changes and all that apparently are several orders of magnitude away from constituting a hazard. And then you begin to concern yourself with the biological effects of radioactivity. Well, we know for instance, that radioactivity can cause necrosis of tissue. But the amount necessary to cause necrosis is probably a hundred thousand times the amount necessary to cause a fairly significant number of carcinomas over a period of time. So that all the gross effects on tissue, by the time you are getting that, predicting what would happen to a species of animal over his lifetime, you would be killing all of them, say by tumors, if you ever got to the point where you were causing real severe damage; if not all of them, at least a significant amount. For instance, when you take the radium experience -- now my figures here are pretty general, but of the people who had necrosis of bone due to radium, the incidence of bone sarcomas was about

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twenty-five percent, and the incidence of bone sarcoma in the general population is probably on the order of .01% or something like that -- maybe one in a thousand. So that you would then not be concerned primarily with necrosis of bone because by the time you got that you were killing so many people over the long term due to carcinogenesis. Now whether genetic effects would occur in even smaller amounts than carcinoma, I don't know. And, I haven't seen anyone who is willing to express an opinion on that. So that would be one type of possibility. But all opinion seems to center on carcinogenic effect, and radiation and carcinogenesis will tie in at least to this extent, and that is for a given unit of radiation, the longer the period of time in which the animal lives, the greater the probability of the carcinoma developing. So, that a certain amount of radiant energy in a human would be much more potentially damaging, say than in a mouse. I think some of the original work that was done, for instance, when they tried to extrapolate from the animal data to human, that on a sheer physical, mathematical basis, the animal data was all 150 times the amount that is extrapolated, and they came out with answers that were, say, on the order of 100 to 200 times greater than they actually found they were able to put in a human being. Part of that relates to the mathematics of probability of the occurrence of events over long periods of time. I think we see it generally in population with increase in carcinoma generally. That, as the population stays in the

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area of life longer, in other words as the life span gets longer the incidence of carcinoma is going up. Simply because whatever stimulii are accumulating over the years have a greater period of time in which to act. So that this brings us into the realm of the carcinogenic effect of radiation as being a critical thing. Now with regard to strontium as opposed to others, it relates to the fact that the material stays fixed, and if the biological half life is so very long (the turnover of isotopic carbon is relatively short) I think it's measured in days on a biological experiment -- it just goes out.) Whereas we are measuring this thing in terms of the lifetime of the individual, so that the amount that gets fixed in bone, it becomes the physical half life which is of significance rather than the biological half life.

BETHE: But couldn't this change if really the whole world were full of radioactivity, if the new carbon which you get is equally radioactive as one which you eliminate, what good does it do you that you eliminate the first carbon?

MITCHELL: Well, if you keep increasing the amount of radioactivity from any source, you undoubtedly are going to increase the probability of getting carcinomas developed. So that whatever the source is, strontium happens to be the type of thing, in other words why is radium so important? For that same reason, that the amount of radium you take in is very small compared

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— well, in fact, the amount of radium is probably smaller in its carcinogenic effects than perhaps even the amount of radioactive potassium. And yet radioactive potassium doesn't constitute, to my knowledge at least, a carcinogenic problem and radium does. In other words you can't accumulate enough of the potassium if you reach an equilibrium stage you have another problem. In other words if you've got an insult up to a certain level, then you would have a certain increased probability of carcinoma occurring from radioactivity.

VOICE: So among other things the fact that the strontium is concentrated in the bones and not distributed equally all through the body makes it worse?

MITCHELL: Well, yes, in other words the amount then is related to a smaller total mass of tissue. That's correct.

VOICE: Isn't it also a function of the fact that the strontium has got really — it's important for four counts — I don't know how much carbon is produced, but its produced in about 5% in fission yield. So that this means that it is one of the largest elements. It's easily absorbed because calcium is normally absorbed into the body; it stays in the body for a period which is long compared to its own half life. And these three things combined, it seems to me to make it an extremely dangerous element. Others that may be potentially more dangerous because of their radiation, don't compete with strontium in these other levels.

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BY: There's only one possible weakness, in that chain I've been able to discover, and that is most of your evidence for carcinoma of the bone is from alpha radiation. And, is it clear that the same lethality will be characteristic of beta rays?

No, but there's gamma radiation in this stuff, too. I remember reading a very grisly story of a dozen or two dozen people that had a lot of X-radiation for I think for rather surface effects on their limbs, and they had doses of a hundred thousand R and twenty years later out of groups of twenty (these figures I am making up but they are approximately right) I think three were still alive, and one or two of these had had their legs amputated.

MITCHELL: Well, there is enough direct experimental evidence in animals to show that strontium 90 is a bone tumor producer par excellence.

LIBBY: Okay. Do you agree, Dr. Lauritsen? Does everybody here agree with that statement?

LONG: Is the question, "Is the beta ray as bad?"

VOICE: Well, I didn't even ask that, but are they really dangerous in this. . . . .

BUGHER: I think the answer to the strontium thing is that we've been speaking of -- that's an experimental point and there's no question about that. We have not had the comparable experiment in man, so that the incidence or the predictions of incidence of

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malignancies in bone in man derive partly from the animal demonstrations of sarcomas of bone produced in rather heavy doses of strontium, plus the historical experience of radium in bones in man, which is quite clear. So that there is an inference here as far as the human situation is concerned. I think the uncertainty is not the fact of the incitement of sarcoma to bone from strontium 90 deposition, but only about the magnitude of the probability, the actual numerical quantity.

LIBBY: I wonder, Dr. Bugher, since you are on your feet if you will take a few minutes to tell us about the Division of Biology and Medicine's overall plans on Gabriel. You see we are contracted out of the Division of Biology and Medicone with also joint interest of the Air Force, and as I understand it, you have some other studies in progress which will probably supplement this one. I think it would be interesting to the group if we had a rough idea of what sort of things you have in mind, if you want to tell us.

BUGHER: I'll make it very brief, because it does fit in with what has been under discussion here this morning. I believe it was brought out yesterday morning after the preliminary studies which Dr. Nicholas Smith conducted on this problem, it was quite evident that he had exhausted all of the available information and nothing to be gained therefore by continued thinking along those lines. We needed qualitative data from which them to work. Consequently, two years ago the program which had

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been working along some of the biological lines which have been mentioned was somewhat re-oriented to give greater emphasis to some of the specific problems. I think in just a few minutes I can give you a general idea of what is going on now, with the idea of deriving reliable data which Projects such as this we are discussing can use in order to do the thing that Dr. Libby mentioned yesterday -- to give experimental checks on conclusions which may be drawn from somewhat indeterminant theoretical grounds. These studies, if you measure them in dollars, will amount to about a million and a half dollars a year, which you can use any factor you wish to convert to scientific man-years. They fall into a number of rather broad categories. One is the performance of the weapon itself, in terms of distribution of fallout over given land masses. That program Mr. Eisenbud has had in hand and you are already familiar with that. A second phase concerns itself with what is the fate of some of this material as it falls, or after it has fallen, in the one case to the ground, and in the other case into water bodies both fresh and salt. The question of the fate in the soil has been under investigation by the group at UCLA. Mr. Larson has I think been giving us some of the results of that, and we have been working with the Department of Agriculture in regard to the distribution of some of this material in soils, and to a certain extent of the movement of mixed fission products in soils.

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There are a number of additional research projects in various parts of the country which deal with one or the other of these special phases of the soil problem. Now the work which has been done by UCLA began with the study of the original Alamagordo detonation site and concerned itself not only with the pick-up of mixed fission products in plants and animals, but likewise with the fate of the appriciable amount of plutonium which was residual in the fusion material of that site. The type of study which was developed there is now being extended to areas of high contamination around the Nevada site with techniques which have grown, you might say, out of this earlier experience. The present time finds quite a lot of emphasis upon the particle size and character of the material that is falling out within two hundred miles of a test site and with the way that material becomes incorporated in the small animals, and in the soil as reflected in plants which subsequently may be grown under greenhouse conditions from soil contaminated with this material. Somewhat similar, but definitely different experiments have been conducted at Beltsville in Maryland, the Department of Agriculture station, and have revolved around the effect on the pick-up of various isotopes, but particularly strontium, of other constituents in the soil. That has led to the sort of thing which has been mentioned, the relationship of the replaceable calcium of the soil to the pick-up of strontium by

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the plant itself. The relationship is exponential and inverse. Now, with regard to the water situation the studies which are under way are really just preliminary. You've heard something about the work being done in Massachusetts, that is being supplemented by a study of the reservoir at Troy, New York. In the past rather sketchy studies of Lake Mead have been conducted (not adequate, I think) and a number of streams have been involved in studies over a number of years particularly at Oak Ridge and of course the project at Hanford has been very much interested in the modification of the Columbia River fauna. We have likewise anticipated these things at the Savannah River area and a general survey of the biological balance there has just been completed to give a line for future comparisons. Now continuing in the water area, one is concerned very much with the factor which have been mentioned. The fact that so many of the food cycles which are important to man have their origin in the sea. There again we immediately become concerned with the isotopes of long half life which also may be picked up particularly emphatically by the plankton. We find that strontium again becomes important because the tendency to concentrate is perhaps more marked in the strontium-calcium complex than it is with respect to any other of the radioactive isotopes of significant half-life. This is not limited to the plankton, but one finds the same thing occurring in various forms of life, for example, the mosquito larva itself, even though the water itself may not be high in strontium if the sediment in a case of a bottom burrowing larva is rather high in bound strontium you find that the amount picked-up by the mosquito larva is quite surprisingly high. Such

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forms of life in themselves become the beginning of the food cycle for small minnows, fish and they in turn become food for larger fish. Eventually we find that we may have an appreciable concentration in the bones of such fish. It is true that some fish bones are consumed as food. Now, the marine life is a fascinating problem in itself, and we do not feel that one should restrict his view to any one isotppe but rather attempt to make a fairly complete study that had been going on in connection with the salmon of the Columbia River for a number of years now and the pick-up of strontium and other isotopes by the salmon has been measured. That ties in with studies made at Bikini and at Eniwetok lagoon with reference to the distribution of fission products in the fish in those areas. The work at Hanford has been associated with that at the University of Washington under Dr. Donaldson and is leading, I think, to some very interesting new techniques in Biology. These little salmon fingerlings have been turned loose from the laboratory, have gone out to Puget Sound to sea, have been captured (they are marked, of course) from Alaska to the Southern California Peninsula and then they are now returning to their birth place which is the laboratory, and are returning of their own free will and a certain fraction of those salmon which were released as infants now swim in through the gate and are available for further study of a nice integrated procedure. It is, I think also possible that we

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will have significant information from the genetics of these fish as time goes on. If one turns to the land animals, the original cattle which were beta burned at Alamagordo were taken in charge by Dr. Comar and he can give you any information you desire on those, and in addition, that project was simply a part of the broader study which he has conducted at the University of Tennessee on the fate and effect of the various radiation factors, particularly fission product, notably strontium, in large animals. A great deal of the knowledge we have concerning the distribution of strontium in the bone and its relation to the growing portion of the bone, it really has been derived from that work. That is the large animal story. Now, from the standpoint of what happend in the whole system we obviously are confronted with the problem of measuring the cycles introduced into the growing plant life and the effects of those things in animals with the objective of getting some sort of prediction factors which can apply to the human situation. You can, I think sense from the discussions yesterday and today how much is missing in the way of hard information and the matter of prediction of what will happen. The question of insitement of tumors is only one of those. So that in man we have been conducting rather broad studies. In one instance analyses looking towards the determination of the normal composition of the body with reference to a good many trace substances including, in this case, also the normal content of radium in the bodies of individuals in different parts of the country. That is a long and rather arduous analy-

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tical procedure. It will have information of value for various things. In the field of general concern with end results we have, of course, the continued interest in the radium patients some of whom are the survivors of the original watch-dial painters in the New Jersey factories and another group composed of those who in the thirties were given actual injections of radium salt in the hope that it would do something good for them. It was a misguided and entirely, I think perhaps you would say unethical type of medical practice which was limited to a very small area of the country, but we are indebted to that unfortunate episode for some extremely valuable information that is now developing. The difficulty in all this area is that the period of induction of the anatomical results, whether its a tumor or severe anemia, may be rather long. In the case of the Czechoslovakian miners, for example, it was an average of 17 years following the beginning of exposure of the mines before cancer of the lungs was finally recognized. In the case of the nickel-carbonyl workers in England it has been about 13 years, so that the time span here becomes a very important sort of thing. We therefore attempt to acquire as much of this specific information as we can and also we would certainly like to make it clear that the conclusions of conferences such as these also serve to orient the emphasis that we've placed on the research program for the future so this is a feedback sort

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of system in which the ideas and needs, as they develop also result in more emphatic treatment of particular problems in the future.

Are there any questions?

VOICE: I heard you have a speech the other day telling the Colorado miners to look out for cancer. Was that misquoted?

BUCHER: It wasn't a misquote but it sounded like one when I saw it later. It's one of those things that is hard to present in a balanced way because the situation exists where one recognizes that there may be hazard. If there may be a hazard then attention should be paid to it. In the minds of many people as soon as you pay attention to something it proves that there is a hazard. Now in the connection with the Colorado mining problem, we have now developing in the United States for the first time an extended industry in the mining of low-grade ores. The mines are small, and for the most part not equipped with the type of machinery and handling equipment that ordinarily is attached to larger rock mining operations. They're dusty, therefore. The measurement of radon concentration in these mines, in many cases, has shown a concentration which is really quite high. It is comparable, in some instances exceeds, the recorded radon levels in the German's mines which I mentioned a few minutes ago. We do know that cancer of the lung was the most common cause of death among

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the German miners many years ago, those who actually worked below ground. However, the mines were not mined for uranium, that was only something that got in the way. They were deposited nickel and cobalt with considerable amount of arsenic and a host of other things present in small amounts. They were highly silicious. The miners all developed silicosis. They were doing dry mining and the amount of dust was fantastic. The early reports were that a miner working at his drilling, at the end of his shift often times would have the nasal passage completely blocked with dust, and they would have to wash the material out. Silicosis, the, was a very common picture and along with that, tuberculosis which always accompanies, played a part. After years of preliminary pathology which I think it would be an understatement to call an insult, carcinoma developed. In that complex, how much was due to radon nobody yet knows. The present thinking favors the emphasis upon the radon or the daughter products of radon as being important factors. Hence, the concern with the Colorado area. It's a question of thinking well this may be a hazard. If it is a hazard, we ought to keep our eye on it and meanwhile on the chance it may be a significant hazard, see what we can do about reducing it. But we actually do not have any proof that the mining operations will in fact lead to an increase in lung cancer. We have the further fact that people have lived in Colorado on the plateau for quite a good many years and it is not noted as an area of lung cancer at the present time.

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SOLOMON: ?? I wondered if it was possible to show a graphical relation between a shortening of the life span and the amount of strontium in the body, or something of this sort.

BUGHER: This tolerance figure is the one which people are welling to agree that there is either no damage or damage so little that it could not be detected as significant. The factor which would lead to really serious injury is a thousand or more, in all probability.

__________: Well, if there are no more questions, Mr. Solomon (Harvard) --

WESTERN: May I have just one general interruption -- I got a pretty good notion from Dr. Mitchell of why it looks like strontium is a bad actor. I have a much less clear picture as to why nothing else is a bad actor. Of the various products from an atomic bomb, one might consider plutonium and iodine, and perhaps some of the rare earths as being materials which are produced in sufficient quantities to be of interest. The iodine might be very bad if, on the short term basis, people were directly exposed to it/ Because of the short half-life on a long term basis, it is no consequence. I mean, if you are not around it. It has a half-life of about 8 days. The reason it is bad compared to strontium is that while you have about the same number of atoms as iodine formed as you do of one of the strontiums, it would be concentrated in a much smaller organ, the throid over and against a factor of about 30 for relative mass of skelton to thyroid tissue. You have perhaps a factor of almost 30 in the other direction for relative sensitivity to radiation, and that is a very rough figure. The thyroid is notably insensitive and the skeleton is usually considered more sensitive than normal tissue. So that I think we can mark the iodine off very readily,

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WARREN (continued)

because most of it gets away before man is directly exposed to it. The question of plutonium came up yesterday, and there are a number of factors which were discussed and I am not sure that they need to be repeated. One of the principal ones is that it is not taken up by the biological systems -- either plant or animal -- to a degree that is anywhere near comparable to that of strontium. Except for that one thing it would be about equal toxicity with strontium in the human body. The same thing is true of the rare earths. They are not taken up by the biological systems, so that we have nothing that approaches strontium within a factor of about 10^4, except possibly iodine.

LIBBY: Mr. Solomon will talk to us on the sampling problem.

SOLOMON: I would like to divide what I am going to say into two parts. The first represents some of the conclusions that our group that met yesterday afternoon arrived at in respect to answers and questions of what we wanted to sample. The second represents some grisly statistics I collected over the last couple of weeks about the availability of human samples. The one thing that we all concluded when we talked about the problem yesterday was that it was necessary to have an organic evolution of any sampling program. We were so much in the area where we did not know how many counts to expect that it was going to be quite important to modify your sampling program in the light of experiences, so that as soon as you got your samples you would have to begin to count them and be prepared to shift all the way along the line. Then, we thought perhaps it would be best to try to get a starting place to see what we really wnated to do, and we did this in terms of two questions, which we agree represented what we wanted. I would

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like to write this on the board. The first one was what we called the pre-pilot query and this was Eisenbud's samples. To see if they have any xxxxxxxxx Sr^{90} in them; and how much. As Eisenbud said, this is a problem which is under investigation, but this seemed to us to be quite an important thing. Then, though we did not specify this yesterday, I am sure that we would all agree that we want to do the same thing to bone samples also. You don't have any bones, have you?

__________: .... A bit abnormal though, because this was right in the vicinity of a heavy fallout, wasn't it? What would be more important would be bones from some other place. ... (discussion about autopsy procedures of one day -- they were negative -- but don't throw them away, they might be interesting to look at when we get a little farther along) ...

SOLOMON: It would seem to all of us, I think, that sometime within the next three weeks we should get ahold of some bone samples and get the yttrium out of them by the most sensitive methods we can, and get some idea of just what street we are in -- whether we are dealing with tenths of disintegration per minute per kilo, or one or ten. Then, the second thing was what we called the pilot query.

(Explanation on blackboard -- how much strontium 90 exists in soils, waters, livestock, foodstuffs, and human bodies within areas of known fallout.)

It seemed to us that we would be very well advised to take advantage of Eisenbud's fallout studies in the hope of finding some kinds of correlations between fallout, and between any one of these sets of variables. I think, although it isn't spelled out here, we are all aware that the human body represents the hazard which we would like most to determine, but it seemed, particularly in the pilot study, that we would be well

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advised to get as much information as we could in a few places in order to see where there were correlations which were valuable. And then we went so far as to pick six places which seemed to us to be reasonable. I will draw these roughly in the order of fallout which might be expected: Northern Utah, or Southwest Idaho would be one. Then we thought Kansas or Iowa, because of the importance of wheat, and also because Eisenbud thinks that there is a ten-fold difference in fallout as you go down here. Then, going down stream, we thought of New England, possibly Boston in view of Bell's studies and the fact that there are a lot of hospitals there from which it is easy to get medical samples. Then, South America. Here we didn't specify a place, but you should specify a place where you have good contacts, since South Americans are notoriously lax in answering letters and things like that. Then, England, and finally, Japan. These four -- you see Northern Utah, Kansas, New England and England are roughly down stream in the fallout area. So if there were any correlation between fallout in these things, we might have enough points.

LIBBY: Isn't Japan on the next scope?

SOLOMON: Yes, I guess Japan is on the next, but we took Japan because of the bombs that fell there during the war. We talked then a little bit about the samples and we didn't come to very much in the way of conclusions. We thought we would have to take some water samples, drinking water, and in towns where there were reservoir and purification systems, we would get samples before and after the purification to see how much was taken out. We thought of soil samples and we decided those would also be necessary and we merely spoke of having them to a depth of 6 inches. We spoke of livestock and we wanted to have their bones of young animals which we hoped to get from local packing [redacted], and then either bones of older

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animals or possibly bone meal. I rather favor bone meal myself, because if we could find a correlation between bone meal, which is an article of commerce and the amount which is taken up in bones of young animals, then we might be able to obtain samples from all over the earth by just orderin g some bone meal. From Russia you might get a reasonalbe idea if you analyzed this as to what the amount of strontium is.

________: Couldn't you get also rye, wheat and caviar?

SOLOMON: That is about as far as we got yesterday in specifying the kinds of samples and we were going to ....

LIBBY: Do you have any friends in South America? I have one in Chile I have been very carefully cultivating.

SOLOMON: We have a student at Columbia University. We have lots of friends in South America.

LIBBY: This is the way you get your samples -- somebody will just go out and get them for you.

I think the group in this room if they pool their acquaintanceships could probably stake out the world pretty well.

________: Are you considering the analysis of foodstuffs?

SOLOMON: Yes, we just got this far -- and then it was 5:00 o'clock and we all collapsed.

Now what I would like to do before I go on to the information about the human subject, I gather from some of the RAND people that the conclusions that we have come to are not the conclusions which they have come to, and

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perhaps we ought to throw this open for discussion as to whether these do represent the kind of outline of what ought to be done.

LIBBY: I would like to say that instead of the sampling panel meeting with the other group with the meteorologists this afternoon, perhaps they ought to meet separately and discuss these questions Solomon has raised, and then if they have time to join them later ...

SOLOMON: Isn;t there someone who wants to say that this is all wrong?

________: In a much more modest fashion, there was some talk yesteday about the possible utility of the grease samples which Dr. Spence might have. That came up as an alternate or supplement to you pre-pilot theory. This wasn't available -- this was information we didn't have. He says that he has samples and anyone is quite welcome who wants to use them.

SOLOMON: I think certainly anything we can get in the pre-pilot quony is quite important, because a month's work will put you in a frame of reference from which you can see how much more work you are going to have to do to get numbers.

What we would like to do probably is to hit Salt Lake. It is fairly important to pick a place where there is a good medical school when you want to get human samples. If you can do that, it is much easier. That is why probably in the Kansas or Iowa area we are not quite sure because there are lots of universities there, but we picked a town or community where there is a university with a medical school, then the whole business of getting human samples is vastly simplified. I understand there is a

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very cooperative veterinarian in Utah. You know him, I presume. Yes.

__________: Would you collect rainfall samples?

SOLOMON: Mostly reservoir water or tap water rather than rain water, because that in a sense is a different sort of program.

We want to pick places that are pretty well covered by Eisenbud's sampling program, so we have some means of relating this to what he finds as fallout.

__________: Did you contemplate in your reservoir of getting some of the sediment off of the bottom.

SOLOMON: No.

__________: It is my understanding that at a number of these reservoirs, depending on the temperature cycle during the year, there is actually an upturn in the stuff on the bottom in mixing which might, if it were a separation out by sedimentation, bring it up and make it strontium available at certain periods. It might be interesting to sample sediments as well as the water.

SOLOMON: That would be a very good idea.

One of the difficulties in designing any program like this is dollars. You don't know whether you are shooting for a 10,000 dollar program of a $100,000 program, and until you know this, you don't know how many samples you want.

LIBBY: I think probably what should be done is that these measurements should be made -- just made by some of us here -- and let you know what the answers are, and then you can make a guess as to what you're dealing with. Somehow or other maybe we can get some of these answers. Maybe you, Mr. Eisenbud, can make your sample measurements, and I am doing some work in Chicago on water from the ocean, and maybe you can make some measurements. We will get

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a half a dozen answers which will serve in this pre-pilot query which will give an idea of what you have on hand -- what the problem is.

________: I think, if I may interject something, the dollar sign seems to look my way a little bit, that the problem is first to get clearly in mind what the best plan is -- the best plan is one which is also the one to determine the minimum cost to achieve the thing which has to be done..., what it costs them...

LIBBY: I don't think anybody here knows -- we can guess...

SOLOMON: Another thing I want to talk about is -- I did a little work trying to find out how available human material would be just looking around in the Boston area. There are two figures that I would like to put on the board. One is that 98 percent of the calcuim in the body is in the skeleton and surpdsingly enough almost 1 percent is in the tooth. About 24 percent of the tooth is calcium, and only 11 percent of theskelston is calcium. There aren't very many teeth, as it were, in grams. It is an awfully good place to look. Now, just guessing these things out of your head, it looks as if at the Boston City Hospital you can get 100 to 300 teeth per day, and a tooth weighs 1/4 of a gram. So, you can hope to get somewhere between 50 and 100 grams worth of teeth every day. This is waste material, nobody wants it. You can do something like the same sort of thing in the Forsythe Dental Infirmary which specializes in children, and so you might very easily be able to get a difference between the teeth of children and the teeth of man. I would hope, myself, that we could find some very good correlation between teeth and skeletons, because if we can find that correlation then we will have no trouble at all in getting samples from almost anywhere. There ought to be a perfectly easy means of getting the Professional Dentists Association to send us bushels of teeth from time to

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time.

__________: But the difficulty is that the turnover in the teeth is very slow, especially in an adult. I would only be a good number of years from now that we would have any concentration in the tteeth even approaching tolerance. ...

__________: What about children?

KOMAR: Certainly in the adult, the concentration period is very low.

SOLOMON: I don't know calcium, but phosphate goes into the teeth at a relatively remarkable rate, it is a more metabolic tissue than you think.

KOMAR: We have analyzed teeth after the administration of both calcium and strontium and compared them with bone... certainly the turnover is slow. If you get a tooth that is just forming, then you get a nice active pickup.

SOLOMON: Actually, children's bones are much the hardest to get, and so it may very well be that would be useless. See, this is clearly open to experiment, but if teeth become easy to handle, and if you can get them in large quantities even if you hve a tenth of the up-take, you still may not ...., Now, I have got some more statistics -- Massachusetts is a marvelous state. Anybody, that is anything, which is born after the 25th week and has got to be buried -- and it costs you $25.00 to have it buried, and this is necessary by law. It becomes relatively difficult to do this. I have got some figures on the Boston XXX Lying-In Hospital. It has about 6,000 births every year. Only 75 still births, which is quite XXX nice, and only

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529 miscarriages prior to the 20th week. Now, you are getting to the point where bone formation is not very great, so it becomes relatively difficult to do this, and it also turns out that it is hard to get these things at autopsy. One of my colleagues si working on fetal material, and he estimates that there are 12 stillborn children out of this 6,000 births that come to autopsy every year. The amount of bone which you can get from these is essentially negligible, really. I figured it would be 1.3 grams of calcium a year. This is just not worth worrying about. So I think that handsome as the idea is, it turns out that unless other states have different laws than Massachusetts and less good hospitals, it would be very hard to get enough material.

LIBBY: We have a legal paper from a law firm in Washington on this point. Could you say whether other states are as bad as Massachusetts.

KRAMISH: They all seem to be about the same -- about the 5th month of gestation. The state of Washington seems to be about the most liberal.

LIBBY: And according to what you said, you are just barely beginning to deposit calcium.

KRAMISH: The statutes rêquire a death certificate for stillbirths after that age. However, the laws governing the disposal of that body are the same as the laws governing the disposal of adult bodies, and one can get bodies through will of the parent. This has to be an instance where the parent wants to dedicate.

LIBBY: It looks like miscarriages are out -- at least in this country. What do

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you think about abroad -- is it any more likely to be favorable?

SOLOMON: It is sort of harder to get in a sense. The bone is so small and it has to be essentially clean from rest of fetal material -- I would suspect that they are probably out, and also if we couldn't get them in this country, where we get our best correlations, we wouldn't know quite what we are doing. With children you can do a little better, but I learned to my great pleasure and surprise that you don't get any bone fragments out of children's operations. Good surgeons put everything back in and let it continue the growth. At the Children's Hospital there are 319 beds and there is one amputation per year. There is a reasonable amount of material that you can get from autopsy, and one could get perhaps from 2 to 3 ribs per day. If we would estimate this as 10 grams per bone we would get maybe 20 to 30 grams of bone a day and taking the calcim concentration this is (from one hospital) -- but that's got -- as Boston hospitals go, it has the largest amount of childern, they deal only in children -- that comes to 2.2 to 3.3 grams of calcim a day, which is still a relatively small number. In adults it turns out that you can do considerably better. You take a figure which I lifted from a pathologist. In Boston, this is at the Peter Van Briggam Hposital, there is one autpsy per year per hospital bed, and this gives you a reasonable figure that you can apply to other hospitals, except that it turns out that more people die at the Briggam than they do other places. Over the country perhaps the figure of 1/2 an autopsy per year per hospital bed would be quite reasonable. Now, if you work through the pathologist at autopsy, you can get samples as large as whole ribs, which may weigh from 50 to 75 grams, so from this

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one hospital in Boston one ought to be able to get some where between 50 and 100 grams of bone every day. The next question that you need is one that is really much more cudous. This is how much strontium will there be in 100 grams of bone in terms of dpm. I played around with this on estimating the available calcium content in fields, then using their figure of how many disintegrations there were per square meter, and I came up with 2.8 dpm per gram of calcium, this is multipling the Alamagordo figure by 40, and that is certainly quite wrong in terms of the total amount of stuff which is up there.

LIBBY: This is one thousands to the tolerance, according to Dr. Mitchell. He had a kilo of calcium, so that gives you a thousandth of a microcurie. Do these check? That is 2.8 per gram and a kilogram ... and that is a thousandth of a microcurie.

SOLOMON: Now, this is in the earth, and the question is what is in man. Here you get out of the region of making a guess of the available calcium and of making the guesses of how much of the calcium that is available in the earth is going to be available in man. I found a reasonable amount of data in the literature, none of it very good, and I didn't really make a thorough literature search, but roughly it looks as though there is a factor of 2 to 1 against you, both in the plant and in man. Plants will pick up calcium against strontium 2 to 1 and man will, so I just took a figure of 4. What do you think, Dr. Komar, how decent is this?

KOMAR: Our experience has been that it would be pretty much the same. We have had a lot of data in animals and the absorption from the gut of strontium

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has been quantitatively the same as calcium. I think Merrill had some figures on plants which indicated that the up-take was not the same.

SOLOMON: This came out of some work by Greenberg and some work of Tweedie's, some of it done over 5 to 6 years ago. It is old work and it may very well not be right.

MITCHELL: There are a series of experiments which shows that as you lowered the percentage retained got closer and closer to the calcium percentages so that in the lower levels you could probably take Dr. Kumar's figures as correct.

__________: How much calcium is there in hair?

Solomon: Nothing serious

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SOLOMON: Plasmic — you take all the rest of the body together which includes hair, and you get only .78%, so there can't be anything appreciable in here; I haven't seen a hair analysis, but when it's gone this far if there were anything larger..

VOICE: This represents a turn-over pretty well, doesn't it? As I understand it, it should be formed from calcium intake day by day.

COMAR: I wonder if I might say a word. I think you have to realise XXXX that when calcium 45 and strontium get into the bloodstream they are deposited in the bone almost quantitatively regardless of turn-over and regardless of growth, and it is just by this exchange process. It comes about because your blood is so low and your bone is so high so the storchometry of it puts this stuff into the bone. So if you really want to simplify things, you can talk about the amount that's ingested, then you know that a given percentage will be absorbed and we have good figures of that. You can say almost quantitatively that this is going to go into the bone and its going to stay there. And the amount that circulates is so small that it is negligible; the amount that circulated is in equilibrium with the hair and soft tissues and the endoginous excretion. But this is so small that you can practically neglect it for all practical purposes.

VOICE: I agree.

HILL: Wouldn't there be a point though that if the natural strontium were in higher concentration in your diet for a given individual,

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[illegible] (continued) then whatever radiostrontium were there would be correspondently diluted?

COMAR: That's right. I think we have to divide the problem there into two parts as to whether we are looking at a continuous ingestion, since the conception of the fetus or whether we are looking at whether this thing is getting into humans who already have an appreciable amount of bone formed. There are almost two distinct problems. In the latter case then, you can make your calculations on the basis of isotope dilution along with your normal strontium. But in any samples where you are thinking of a new-born, where most of the skeleton has already been formed, then you have to look at this as an exchange proposition where you are getting a one-way deposition.

SOLOMON: If I go back to this arithmetic of mine rather than to try to change the figures, I think it is easier to give them in the terms in which I had. I assume that in adults, 25% of the calcium in the bone was exchangeable. This is necessary to take this figure of the total calcium in the bone and turn it into dpm. I don't know whether you have done any work on that or not.

COMAR: This is an acceptable figure, around 20% of the calcium in the bone is correct.

SOLOMON: Then this would work out that a 10 gram bone sample would yield 1.1 gram of calcium and a tenth of a dpm, using all of these factors of mine. And so a hundred grams of bone would give you one disintegration per minute, which is, with your methods,

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SOLOMON: (continued) relatively measurable. So it means, therefore, that something like a one-to-two day collection from a medium sized hospital will give you a sample which can be measurable, and not much more than that in terms of teeth, neglecting the uptake factor.

LIBBY: But isn't there a question that even if you can measure it, it seems to me you need bone ash from the whole body. If you get somebody's rib, doesn't it matter whose rib it is?

SOLOMON: Yes, but you can get, I guess, bone ash from the whole body, but that would become terribly difficult to do. What one would have to do, would be to get a correlation...

LIBBY: Can't we get permits from people who are going to die and get their ashes?

SOLOMON: Yes, I would think so. We've thought of crematorium robbing as a possibility — don't rob them, get the permission.

HILL: Bill, didn't someone tell us earlier when we (you?) were out here that work is going on at the University of Rochester on this radium program where they use the whole body bone ash? Couldn't that material be available?

BUGHER: There are two places for that sort of thing..

SOLOMON: What one would want to do would be to establish a relationship between whole body ash and then ribs, because ribs are apparently very easy to get, and you can get ribs from a lot of places if you have some idea what this would mean.

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BUGHER: The main difficulty is that most of the bodies one gets like that are old peoples, 60, 70 years of age. It's not quite as good as you would like.

LIBBY: I think somebody mentioned human milk. What do you think of that idea?

SOLOMON: Well, milk is perfectly O.K. There's quite a reasonable amount of calcium in milk.

MITCHELL: I might just add one thing here. Two people by the names of Spires and Burke at the University of Leeds are using crematory ashes, and they've mentioned the fact that the thing imposes limitations on their investigations.

A short time-out break here.

LIBBY: Now the statistical aspects of sampling are obviously extremely important. Mr. Marshall, statistician of the RAND staff, will speak to us for a few minutes on the sampling problem.

MARSHALL: I'm afraid I don't have very much to say right now except some generalities, because until this question of what it is that we're after has been answered more precisely than it even now has been answered, and before any guesses have been made about the underlying fluctuations in the things which we are trying to sample, not much can be done. For example, let me just illustrate what the situation is if while we're just looking over the soil problem and wanted to find out in a particular area what the mean level of the strontium was, a great deal is going to depend

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MARSHALL (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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MARSHALL: Well, I think there all you could do is really get an average over time, that is, this week...

LIBBY: Oh, I see.

MARSHALL: What I would really be interested in is the variation between human beings living in the same area and eating roughly the same diets. Now this may turn out to be very small, but I'm not sure.

VOICE: Libby? Can't you get this variability by using pigs?

MARSHALL: Yes, you could get it from pigs — I mean you could get the variation in pigs, but I'm not so sure about...

LIBBY: You are not going to be able to take a whole family and get all their bodies.

MARSHALL: No.

VOICE: I just wanted to know if randomly selected individuals in this city, of age approximately five...

LIBBY: Maybe they can. Maybe it isn't so difficult. Even the small amounts that are in blood may be enough.

VOICE: It's a lot of blood.

VOICE: How about hair?

LIBBY: There are some things you can get, and hair is one of them. How about fingernails? and teeth?

MITCHELL: [redacted] I would say once you leave bones or teeth you are cutting your factor of availability by such a tremendous amount. You are going

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MITCHELL: (continued) to have [redacted]ing 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 pick up a sample of blood today, you have x amount, you will

[redacted] have the accumulated amount of x times a certain number of years

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MITCHELL: (continued) during which this person has been exposed in the bone. Suppose a person took some material in yesterday, and none today, it just wouldn't be in the blood; it would be in the bones. It is deposited just about immediately.

COMAR: We have some animals that received some Sr^{90} about two years ago, and we'll be glad to make blood, hair, bones, etc., available to anyone who wants to measure them, and we can tell you what..

LIBBY: How much did they have in them?

COMAR: We gave them about a millicurie, some five millicuries.

LIBBY: Could you send me some bone ash? And also some ash from other parts? Just half a dozen parts. It would be very easy to make a rough idea.

COMAR: I'd be glad to. This will give an estimate of the body burden in these various tissues.

MITCHELL: The thing that might be of value is the fact that when a person reaches equilibrium after radium exposure, the daily excretion is .005 percent of the amount of bone.

SOLOMON: A possibility comes to mind of a tiresome thing to do. From Dr. Comar's suggestion of the blood. You get so much of it. Depending on how the national blood program collects its blood, one of the techniques of collecting blood is taking it over an ion exchange resin, which takes the calcium out and this prevents

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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: (continued) we have better spent our time taking an individual cow, or pig, or something?

[illegible] ([illegible] MARSHALL): I don't think so, unless this place that the pig came from had some special significance for you. If, for instance, he came from the outskirts of a city in which you were also taking other samples, this might be related to other things that you find out about this city.

LIBBY: We have various kinds of samples which have a lot of averaging already in them. We have this milorganite thing, we have this bone meal thing, and we have the fertilizer made from tuna fish bones, and there are several others of this class which come to mind immediately.

MARSHALL: I think the situation is that since they are very inexpensive to get and probably not too bad to analyze, they are something one would do, but do they answer the question: what is the average amount of strontium in american cattle? Drawn from certain areas you could find out about..

LIBBY: Some of these samples we can probably get from abroad... that sort of thing. Bone meal we could get.. and I think probably fish bone, and stuff.

SOLOMON: Actually, maybe fertilizer manufacturers ought to be looked into. I don't know all the components of fertilizer, but some of them come from very wide parts of the world. A lot of them, I am sure, will contain calcium.

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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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[illegible]ZMAN: (continued) participated in any atomic tests, and simply took swipes from the air scoops and ran just a check on these things. I've forgotten the details, but about half of them showed activity about twice that of background, indicating at least that we were getting something. These experiments were designed just to give us an answer as to whether we got something or wouldn't get anything, and certainly that can't be used quantitatively. We would have to design an experiment that would be more meaningful, because we are not sure where these aircraft went or how high they were flown. This stuff might have been accumulated over many months. The fact that we just took a swipe, it might have been a light swipe or a longer swipe, but it did indicate that they picked up something, and if they design an experiment properly, I am sure that at Kirtland, at least for our aircraft, we could do something that might contribute. Colonel Hocks is my boss here, and I'm sure he would be quite willing to do anything we can to cooperate. We have our headquarters represented here...I think Colonel Crowson is here, and so if you want to expand something, say, get into jets over Korea or some other place, the equipment and what not are very simple and easy to handle, and if it does not offer any awkward techniques, and I'm sure we can get data.

LIBBY: You have radiation monitors at most of these bases, don't you?

[illegible]ZMAN: Yes, we do.

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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.

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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

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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 isonthropic flow down slope. You see air doesn't flow horisontally, 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

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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.

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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 squeese 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.

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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.

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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 litho-sphere 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.

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[illegible]INBUD: 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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I ?: 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 tollerance. 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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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 that 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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KRANISH: 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.

KELLOGG: 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 those; 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 device, 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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KELLOGG: (continued)

of sending an expendable [redacted] equipment 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 factor 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 expanded 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 [redacted] 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 194?. 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.

WEILER: 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.

WEILER: 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

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

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KELLER: (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 80,000 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 precipitator 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 affective 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 precipitator. 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 -

~~MEIKI~~: 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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ILER: (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 then 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.

WEILER: 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 radio-sonde 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 in 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 hmm. Well, as I said earlier, I'm not ever ashamed of giving a meteorological reason for wanting to find something out; how-

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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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KELLER: (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 17 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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W[illegible]ER: (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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[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?

ICE: 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?

WYCKOFF: 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.

WYCKOFF: 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 got 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 $1,000^{TH}$ ~~xxx~~ of cosmic ray background.

KELLOGG: Which is $1,000^{th}$?

BACHER: The Mike cloud.

LIBBY: So it couldn't be checked, but it's still an important question, even if it is only 1000^{th} 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.
WEILER:	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 aga[ins]t 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__SY: 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, for 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 ray 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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WESTERN: There is another factor, Dr. Comar, if you assume that it is from the beginning the accepted value of approximately three-tenths per week is based primarily on occupational exposure, however, the report of our International Committee on Internal Dose has not been published but it seems very likely that for lifetime exposure they will recommend a factor of one-tenth of that, so it seems more appropriate that they are going to make a comparison and use a value of one-tenth of the .3 r per week for the case of uniform deposition and use the value of 1 microcurie where you are making the comparison directly with radium. I would like to point out also that the comparison with radium is not made in the way he indicates here but is made experimentally on small animals, it is true.

BETHE: What does the comparison say?

WESTERN: The comparison says that one microcurie of strontium is--that the ratio of the effects of strontium and radium--are the same as the ratio of a dose of 1 microcurie of strontium to one-tenth of radium. That is, the ratio of ten to one activity. Those are the experimental results on small annimals as best as we can interpret them.

BETHE: Well, if this is so, this, I think, contradicts everything that I have heard before, namely, that concentrated ionization is very much more effective than dilue ionization. It contradicts, for instance, the statement which is usually made about neutrons being much more effective than gamma rays, and so I must say I just don't understand how this could be.

V E: May I ask if you have taken the energy of the yttrium daughter into account?

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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 realistice 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 calcium atoms. In the results of investigators at UCLA, the ratio for adults, that is from 5 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 58 atoms then per thousand?

KRIEGER: Yes.

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

KRIEGER: For natural water the figure is about 53. 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.

KULP 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.

..BY 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?

THE 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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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 x 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 it's 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 it 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 mineralise 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.

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SCOVILLE 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 sludge contains all kinds of stuff which has gone into the sewerage and is not of human origin.

HAMISH 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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BOHR: 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.

LONCH: 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 .....

HILL: 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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BOLZMAN: 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 4.10^4 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, $4x10^4$ - I'm sorry - 800 megatons

LULEJIAN: 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. These 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 these 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

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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 analyzed 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 possible argument for the experimental program not only just assay but also biological. Is there any feeling on the part of anyone 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

[redacted]

[redacted]

LIBBY (continued) in the limestone eventually 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.

[redacted]

[redacted]

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?

[redacted]

[redacted]

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 Wilamite 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.

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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]

[redacted]

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.

[redacted]

[redacted]

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.

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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.

LIBBY: .... 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

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[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.

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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.

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

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KRAMISH: (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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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

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[redacted]

W..ERN: (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 Holmman'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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L[illegible]Y: 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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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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LANG: (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..1.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 6 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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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.

V. E: 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.

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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

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LARSEN: (continued)

experiment we did. 1.8% of the Strontium is retained from a dose of 431,000 d per m fed daily over 17 weeks. Plant dry material grown on contaiminated soil which would give an average daily feeding of about 2700 d per m, 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 ehere 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.

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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 as 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.

L: 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. Holman'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 tmorer 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?

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

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

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

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

HOLDEN: 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.

COL. 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.

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

BUGHER: 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

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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,

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in order to be consistent with a 1/10 of a microcurie for radium or with 300 miligrams per week of gamma radiation. This factor 10 and the other factor 10 between the tolerance dose of radium and the sorias 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[illegible] 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

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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.

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