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NUCLEAR REGULATORY COMMISSION

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148th ACNW MEETING

ADVISORY COMMITTEE ON NUCLEAR WASTE

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WEDNESDAY,

FEBRUARY 25, 2004

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ROCKVILLE, MARYLAND

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The Subcommittee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 8:00 a.m., B. John Garrick, Chairman, presiding.

COMMITTEE MEMBERS:

B. JOHN GARRICK, Chairman

MICHAEL T. RYAN, Vice Chairman

JAMES CLARKE, Consultant

GEORGE M. HORNBERGER, Member

JOHN T. LARKINS, Executive Director, ACRS/ACNW

RUTH F. WEINER, Member

EXPERT PANEL:

DADE MOELLER, Keynote Speaker, Dade Moeller and Associates

JEFFREY DANIELS, Lawrence Livermore National Laboratory

KEITH ECKERMAN, Oak Ridge National Laboratory

DAVID KOCHER, SENES Oak Ridge, Inc.

MICHAEL THORNE, Mike Thorne and Associates (UK)

JOHN TILL, Risk Assessment Corporation

NRC STAFF:

HANS ARLT

JOHN BRADBURY

LATIF HAMDAR

BALER IBRAHIM

PHILIP JUSTUS

MATT KOZAK

TIM MCCARTIN

CHRIS MCKENNEY

TOM NICHOLSON

PHIL REED

A. CHRISTIANNE RIDGE

CHERYL TROTTIER

MITZI YOUNG

C-O-N-T-E-N-T-S

P-R-O-C-E-E-D-I-N-G-S

8:01 a.m.

CHAIRMAN GARRICK: Good morning. The meeting will come to order. This is the second day of the 148th meeting of the Advisory Committee on Nuclear Waste. My name is John Garrick, Chairman of the ACNW. The other members of the committee present are Michael Ryan, George Hornberger, and Ruth Weiner. We also have a consultant with us today to the ACNW, Jim Clarke.

Today the committee will continue the working group on biosphere dose assessments for the proposed Yucca Mountain high level waste repository. Mike Lee is the designated federal official for today's initial session. This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act.

I don't think we have received any written comments or requests for time to make oral statements from members of the public regarding today's sessions. However, should anyone wish to address the committee, please make your wishes known to one of the committee staff. As usual, it's requested that you speak clearly so that we can understand you and that you announce your affiliation and representation. I think

we'll just go directly to the chairman of the working group session and proceed, Mike.

VICE CHAIRMAN RYAN: Thank you, Mr. Chairman and good morning. Thanks again for a great day yesterday. I think we had some informative and thought provoking presentations. If you recall, we had a homework assignment at the end of the day to come in this morning and think about giving some summary ideas of what you heard yesterday recognizing we'll have several opportunities to discuss those ideas as the day proceeds.

So I just wanted to open with our panel chairman, Dade Moeller, and then ask him in turn to maybe have you summarize a few key comments from yesterday as we then go into our risk insights discussion and hear about research activities in this area. So Dade, thank you.

DR. MOELLER: Thank you, Mr. Chairman. To lead off, I have written down a summary of my own thoughts of what the highlights were from yesterday. The panel members or even the members of the committee may not agree. But I wanted to put them out of the table so that we can discuss them. Then, as Mike says, let's encourage all the panel members as well as committee members and others to contribute your own

additions to my list.

One of the first things that I heard was that there are two types of efforts in terms of doing dose calculations and dose estimates. You do dose estimates to evaluate compliance with the regulations. We also concluded, or at least I believe we concluded, that there are other calculations that you need to do which extend information and incorporate other aspects of the other calculation.

They are more for informational purposes, educational purposes for perhaps hopefully that these calculations will help members of the public better understand what's being done and so forth. I think that compliance calculations are - this isn't exactly true - but they are at least straightforward. We know what we need to do. The degree to which we can do it is always open to question.

But in terms of the second set, I put down some examples of what I heard yesterday. I would encourage the NRC to encourage the DOE to do dose calculations using all of the available sets that we discussed yesterday of sources of dose coefficients, in other words, do it using Title 10 Part 20, do it using Federal Guidance Report Number 11, do it using Federal Guidance Report Number 13. You might even

want to do it using NCRP Handbook 69. But that's open to question.

The second thing I believe would be very useful and it is good to have it written down so that you can distribute it when questions come up is to do dose calculations for different age groups. In other words, you do it for the adult for compliance but do it for a teenager and do it for an infant. We saw the curves yesterday in which one set of curves showed the dose estimates with time for Carbon 14.

Well, there were multiple questions about those dose estimates. So certainly I believe NRC should encourage DOE to reexamine those calculations. We have heard time and time again about the biosphere dose conversion factors. For many people, those are a black box. However, DOE and the NRC, both sets of staffs, have done multiple written reports in which they have explained the components of the BDCFs.

I believe that the NRC might encourage DOE to have available reports on that so that members of the public, if they ask, and even members of the technical community could read these reports and gain a better understanding of just how those are being done. Now, I want to add one other set of informative reports. This was not discussed yesterday. So I want

to clearly acknowledge that it's simply one of my suggestions.

We need a baseline report. Now, the information is available if you read the various environmental impact assessments, if you look at the technical basis document for the biosphere. You can find much of the information that is needed in what I would call a baseline report. To be sure everybody understands, here I have reference to conditions within the region that will eventually be impacted by the repository. In other words, what are the conditions there today?

How much iodine is there in the groundwater, technetium or plutonium or whatnot? You could say why? Well, as all of us know, the Nevada test sight is next door. They have done many underground detonations. To me, it's very important to document all of this information. This includes natural background sources such as the uranium and radium and so forth.

You could say even if we find plutonium or neptunium or americium or et cetera in the ground water, say someone goes out and makes a measurement five minutes after the closure of the repository and they find some I-129. Well, the response and the

almost logical response is that repository is leaking and here is some iodine.

If you have done a baseline survey, which has been done, as I say, I believe most all of the data that you need are available, have those data summarized in a document. That's what the condition was before any waste was even placed in the repository. That will be far better as a reference document at that time then to go out and say the fact that there's iodine there is not a problem because we can do forensic tests and do atomic ratios or isotopic ratios and forensically determine its source.

Well, fine. Well then good but it's much better to have a basic document. Now, you might ask who should do the compliance calculations? Well, certainly both the NRC and the DOE will be doing them. You might ask who should do these other extra informative calculations? I believe again that NRC should encourage DOE to do that.

The second item I have is the regulatory process. We heard and we were reminded that it consists of multiple steps. It permits factoring in new information along the way. DOE well understands this. Our science and technology panel was created to continue the research, to enrich the database even

after the license application is submitted.

Why? Because there will be many opportunities during that review period which legislatively is stated to be three years long, from 2005 through 2007. Obviously the NRC staff will stay active throughout that period of time. But let's encourage people not to cease continuing to conduct studies to reduce uncertainties and so forth.

The third item I have is related to uncertainties. It ties in to what Dr. Till was commenting on. He was pointing out, and the panel obviously was not unanimous in that, but I'm unanimous on it, that there are two types. One is factors that have been fixed by the regulations.

You have to understand how Dr. Till is defining this. He said there are no uncertainties. He's meaning that in a strict sense. But what do these include? Well, the -- He or she drinks two liters of water per day. It's based on this withdrawal we heard of 3,000 acre feet per day. It's based on dose coefficients and at the moment Federal Guidance Report Number 11.

Now, the panel seemingly yesterday said we ought to encourage DOE to move to Federal Guidance Report Number 13. But in that sense, you don't argue

with the dose coefficients in the Federal Guidance Report because that is a decision that they are to be used.

Now, factors that must be measured and have a distribution of values include the obvious things as we're irrigating the crops and there will be uptake by the home gardens as well as the alfalfa and so forth, food for the cows. There's uptake and those uptake factors have uncertainties so we should certainly continue to try to refine those. It's a dynamic process as Dr. Kocher emphasized yesterday.

So that's one example. The biokinetics, there I think, and I hope that this is not incorrect, that one of the major uncertainties is the GI absorption track factor for plutonium or neptunium or americium. Then there's the dosimetry. That involves the distribution of the radionuclides within various body organs there, the types of radiation they emit, the energy of those radiations, how that energy deposits within the tissue and so forth.

So anything we can do along those lines, we, NRC and DOE should be moving ahead. The NRC should encourage DOE to prepare documents in which they express the conservatisms and the uncertainties, quantify them as best they can. What are some of the

uncertainties?

Well, one to me is the solubility of plutonium. I know DOE has put a tremendous amount of effort into this. They have studied colloids. In fact, in the technical basis documents, there's a whole section on colloids and plutonium colloid. So they are making the effort. That needs to be put in a form so all of us can understand. The uptakes of the radionuclides, we have already talked about that. The Kds for the movement of the radionuclides in the soil, I gather that the Kds are one of the factors that play a major role in uncertainties.

In a similar way, they should look at the conservatisms. I don't think I've seen in anybody's report, and someone will quickly correct me and please do because I'd like to read about it, the long effective half-lives of the alpha emitting neptunium, plutonium, and americium give you a factor of two conservatism in the dose estimates simply because of the committed dose concept.

The acute versus chronic intake, the dose coefficients, and I believe Keith has agreed on this, are for acute. Not agreed, he knows. He can tell us. It's for me to agree with him. But they are based upon acute intakes. In other words, I take in the

whole annual intake on January 1. That's not going to be the real world situation.

I think next we need to look at the fact that there are three sets of standards; the intrusion standard, the groundwater protection standard, and the individual protection standard. To me, it would be extremely helpful, and in fact Maryla Wasiolek yesterday pointed out at least one case where which of these - skip the intrusion standard - but for the groundwater protection standard and the individual protection standard, which one governs under what circumstance and for what radionuclide?

To me, that's very important. In fact, if you can do that, it helps people get a grasp of what's going on without being confused too much by the complexity of the regulations. What do I mean there? Several things. The groundwater protection standards, and please all of these statements will have qualifications. But I think in terms of technetium and iodine, the groundwater protection standards are it. That's it.

Now, it's the formula around that Dr. Kocher pointed out quite correctly. They have established secondary standards so it will be the picoCurie per liter limit in the two liters of

groundwater that you consume. But technetium and iodine are in my opinion just essentially totally controlled by the groundwater protection center.

Why do I say that? You can say there's an effective dose from technetium and iodine and it has to be considered in the individual protection standard. But the effective dose for technetium based upon FGR 11 is one-tenth of a millirem a year. Well, in 15 millirem, one-tenth is not much of a contribution. And for iodine it's two-tenths of a millirem per year.

Well, I say therefore the groundwater protection standard is controlling. Now, in a similar manner, the groundwater protection standard is controlling for radium 226 and 228 because I presume that the bulk of the radium 226 and 228 that's in the groundwater, which is now I think two or three picoCuries per liter. It's somewhere in that ballpark. In fact, they took one sample that I saw the exceeded the five picoCuries per liter. Then they resampled and it showed that that initial sample was not correct.

I say or suggest that radium 226 and 228 are controlled by the groundwater protection standard because if they are naturally occurring, they do not

play any role in the individual protection standard because natural sources are exempt from the individual protection standard. Now, where does the individual protection standard play it's major role?

In my opinion, it's in neptunium, plutonium, and americium because the bounding limit under the groundwater protection standards for those nuclides is 15 picoCuries per liter. Well, 15 picoCuries per liter permitted by the groundwater protection standards gives you from three to more than four times the 15 millirem a year limit. So therefore, for most cases, the individual protection standard will be governing.

Now, back to the secondary standards, Dr. Kocher is absolutely correct. They have been established by EPA. As I recall, it's 2,000 picoCuries for Carbon 14. It's 900 for technetium. It's one picoCurie per liter for iodine 129. However, I tried all four sets of dose coefficients. I do not find four millirem per year consistently in any of them.

Let me give you the numbers. Again, I work alone so nobody checks my calculations. I acknowledge they need to be checked. But if you apply FGR 11 with those picoCurie per liter limits to two

liters of water per day for an adult, you get 3.1 millirem for Carbon 14, 3.9 for technetium, and 6.7 for iodine.

Now, I can understand the 6.7 for iodine because EPA doesn't want to say six-tenths or four-tenths of a picoCurie per liter. They want to say one. And that's fully understandable. But someone needs to look at those. Now, if we switch to FGR 13, I came out with 3.1 for Carbon 14 and 5.7 for I-129. I'm skipping technetium for the following reason.

When you shift to FGR 13, the organ with the highest dose is the lower large intestine. In that case, the dose to that organ, I don't know whether the lower large intestine is an organ or whether the colon is the organ. That needs to be clarified. In any event, it comes out almost ten picoCuries per liter.

Now, I'm winding down but I'm drifting into Never-Never land. My fifth item is considerations in terms of the groundwater. The groundwater is extremely hard, as we said yesterday, ranging from more than 200 to more than 1,100 parts per million total dissolved solids milligrams per liter. I have heard and have read the results of the food consumption survey.

There was nothing in there about water softeners. I realize the nature of the community and probably not a single soul has a water softener. But we are supposed to base our RMEI on the average member of the community and his or her dietary habits and living style. Well, there is a casino. There is a hotel. There is a country store, whatever you want to call it. I find it hard to believe that not one of those facilities would have anything in the way of a water treatment facility.

If they do, that's part of their living style. Again, it may only be a minor thing. But I would like to know about it. Is the water potable? It has from 1.6 to 2.3 parts per million of fluoride. One part per million of fluoride is ideal to prevent dental care or to assist in preventing them. I believe 2.3 will mop model your teeth if you consume it long enough. Well, I don't know the ramifications. But I ask, is the water potable?

Carrying on the earlier thing of informative calculations, we have read that the pumping permits, and I realize that's a permit only. They are not pumping as much as the permits allow. But at some time, and this is conjecture on my part, but certainly before the closure of the repository,

you could pump that aqua for dry.

Now then, they have to move to a new source. Whether it's practical to go 30 miles away and drill a new well and pipe it over, I don't know. But I would like to see DOE examine that. That would add to their credibility. It need not be done, insofar as I know, prior to submission of the license application. It's something that could be done afterwards.

The next to last, FGR 13, the panel pretty much said go for it. That would, in my opinion, be a tremendous step forward. My last point would be based upon my experience, and I was not involved in WIPP but Ruth Weiner was and others, after WIPP license was approved, I have been told personally by Wendell Weart that DOE disbanded its staff. Questions have come up time and time again since that facility started operation.

So my final urging, and it's a personal statement, is that for neither the NRC nor DOE to even think about disbanding their staffs until that repository is closed and even after. If it's approved, if it's filled and if it's closed, even after that, do not disband those staffs because you need the legacy of their knowledge, of your knowledge

about the facility as you move forward. I have taken up far more time than I intended. Keith, do you have comments, rebuttals, and additions?

DR. ECKERMAN: No, I think you hit all the points that I really had. I would view the compliance tool as a tool, as an instrument. I think you need to calibrate that instrument. That's these other satellite calculations that we have talked about. I think the compliance tool ought to use the latest Federal Guidance 13 dose coefficients which both the principal agencies have in the past endorsed people and allowed their use.

I would encourage the people that are responsible, if you don't have a copy of the ICRP CD, by all means, get this. We'll just have to calculate and use the ICRP 26 waiting factors that are in the regulations and recalculate what the effective dose equivalent is and use those coefficients. I'm still a little concerned about RMEI.

Is that definition being extended beyond what's really required by the law and whether that's done in a consistent manner? That's some detail that can be looked at later. But I think people should be very careful that they are not making some decisions in an inconsistent manner here as they treat RMEI. I

think that's basically reiterating most of what you had said in one way or another.

DR. MOELLER: Keith, let me ask you for my own education. Did you just say that the ICRP 26 tissue waiting factors are in the law, in the regulations?

DR. ECKERMAN: This is the position the agencies have taken. They say that you can use the equivalent dose coefficients from the latest dosimetry. But they want you to use the waiting factors that are in part 20 and in the law. That's the interpretation I have gotten from people.

Because of the robustness, it really doesn't make a whole lot of difference. Numerically you will see the difference with iodine 129 depending on which set of factors you are using. That probably ought to be clarified with the agencies because that position I had heard some time ago.

VICE CHAIRMAN RYAN: And I think we heard that if a licensee asks for that explicitly, they can sure deal with it on an explicit request basis. But I guess I didn't hear that it's a policy per se.

DR. ECKERMAN: Yes.

VICE CHAIRMAN RYAN: So it would be your advice to qualify it.

DR. ECKERMAN: It has to be qualified. By all means asked, don't take my position on it.

VICE CHAIRMAN RYAN: Thanks.

DR. MOELLER: Tim McCarten, are you in a position to comment? Would you please if you can?

DR. MCCARTIN: No, that's not my area. Chris might have some idea.

DR. MOELLER: All right.

MR. MCKENNEY: It is NRC policy that if a licensee asks, they can get an exemption from the definitions of part 20. Definitions in part 20 unfortunately do have exactly the waiting factors listed in there. That is why there has to be a change to allow the new system.

DR. MOELLER: But they can request it.

MR. MCKENNEY: Yes, they can request it.

DR. MOELLER: Thank you. David Kocher.

DR. KOCHER: It would make no sense to me whatsoever to use the latest biokinetic models and calculate effective dose equivalent. That just flunks the laugh test. I would have to go look in my files. But the memo I remember seeing from NRC talked about you can use effective dose. I could be wrong about that.

Dade, also I was wrong yesterday about the

drinking water standard apparently. I'm told that there was a deal struck shall we say where the part 197 just has the dose standard in there and it doesn't refer to the old MCLs. So you apparently are at liberty to use different --

DR. MOELLER: Concentrations.

DR. KOCHER: You can derive different MCLs from that based on newer biokinetic and dosimetric models. That apparently is the case so I was wrong.

DR. MOELLER: Tim McCartin.

DR. MCCARTIN: Could I just qualify that? I'm not aware of any deal that was struck. EPA chose to write the standard in that particular way that they do not explicitly point to the MCLs. There was no deal that I'm aware of in that regard. That was an EPA decision. The implication might have been that NRC had something to do with that.

DR. KOCHER: No, I think this is an internal EPA matter.

VICE CHAIRMAN RYAN: The decision was not to point to MCLs at the end of the day.

DR. KOCHER: Apparently that's so because part 197 doesn't refer to those explicitly. But that's something that a lawyer in consultation with EPA would have to fair it out. Congress, the Safe

Drinking Water Act amendments, may have something to say on the issue if somebody really examined what that means.

VICE CHAIRMAN RYAN: Tim had a comment.

DR. MCCARTIN: Yes, although the differences between applying those MCLs and applying the limits there are very small.

DR. KOCHER: Yes.

DR. MCCARTIN: It's not like there's a significant difference between the two.

DR. KOCHER: It's how many angels could dance on a head of a pin kind of thing.

DR. MOELLER: Dr. Till.

DR. TILL: I might just add a few things. My first point is that compliance with standards for public exposure is public business. I know that's why you are here and that's why these meetings are open. But in the same sense, this is as much a credibility building process as it is a calculational process.

I have always said this. I have been caught in the middle of it. I'm guilty myself as a scientist of thinking that we can do the greatest science, perfect calculations. But if you haven't brought those exposed along so that they understand what you did, then you are actually doomed to fail.

Yesterday I heard several times opportunities for the Department of Energy or NRC or whoever is responsible to earn some credibility. The examples are the evaporators. Perhaps that would be something simple to do if it solves the problem. Visiting the dairy farm, if you have 5,000 cows out there, I would know everything about that dairy farm whether it plays directly or not. I would be able to answer that question.

So I think it's important to keep in mind that this is really a credibility building process. I was very pleased to hear when this question came up about the survey. The first time it came up, we were told the survey was not done in Spanish. Finally, the record was laid straight. It was done in Spanish. That's crucial. So that's my first point.

My second point is, and I mentioned this yesterday, about recommending that the Department of Energy use the best science available in going through this compliance process. I think that should be policy. I think it needs to be decided how you do it and how you implement policy. Just to make a statement is one thing. But how do you decide when there is new science and when you implement new science?

That's like the dose conversion factors. You can't pick and choose among the science. You can't pick one dose conversion factor that makes your dose lower for plutonium inhalation and higher for plutonium ingestion. We know that was the case in the last revision of the dose factors. So some kind of a method that you are going to use the best science and here's how we're going to do it.

This is Keith's point. I think this is a crucial issue about the RMEI. I understand that requirements to stay within the law. And that's important because that's the way the law is set up. But this is certainly not the traditional critical group concept. I would certainly have, within these stylized calculations, in my back pocket what the critical group dose is as well just to be able to answer that question.

I assume we're going to come back and talk a little bit more today about this adult being the individual exposed. I said yesterday I agree with that. I want to talk a little bit more about it and explain why. But I think that's going to raise some questions with regard to the public. It came up yesterday about children being exposed. That all needs to be taken into account. There's a way to do

that. Maybe we can come back and discuss that some more today.

VICE CHAIRMAN RYAN: We'll have opportunities after the presentations to do that.

DR. TILL: Yes, I was very happy, Dade, with what you said about the uncertainties of some fixed and some not fixed. That's a little bit of a change in the way we have done business in the past. I recognize that. But the idea that the parameters that define an exposure scenario for an individual in the future in my view should be fixed.

Quite honestly, at the same time, I would make that calculation with the variability in those parameters and with a distribution. I think what you will find is there isn't much difference. But to me, like I said, it's a philosophical issue that's important to lay very clearly on the table.

Another point that I have a little bit of trouble with is this decoupling of the different elements of the TPA. What we're working on, what we're focusing on in this group are the biospheric dose conversion factors. And that's fine.

VICE CHAIRMAN RYAN: John, just to be clear, I think you mean the TSPA meaning the DOE calculations.

DR. TILL: Yes, I'm sorry.

VICE CHAIRMAN RYAN: Okay, I just wanted to be clear.

DR. TILL: I think what we're doing is fine. It's fine to look at this. But then you need to come back and look at the whole package together. What worries me some is this, and it goes back to credibility. I worry that this element of the calculation is de-emphasized so much because the uncertainty is so small and it plays such a small role overall in the overall compliance process that it's not given the attention it's due for the credibility issues.

Quite honestly, if I had to predict anything, I would say this is the element of the compliance calculation that will give you more trouble than anything in the long-term. It's because people understand. They understand what you are trying to do. They understand what people eat and what their lifestyle is. It will get challenged. So it's important that you come back and couple these together in the long-term. Those are my points. Thank you.

DR. MOELLER: Let me go back to Dave Kocher. I apologize, Dave, you were not through. Please continue.

DR. KOCHER: The bad news is I hadn't actually gotten started yet.

(Laughter.)

DR. KOCHER: I very much second John's concern about this decoupling business for a number of reasons. I think we all recognize we have a fundamental quandary here. The reason we're putting this stuff in the ground is because we think the geosphere and engineered barriers do good things for us. That's clearly where our greatest emphasis should be placed in assessing total system performance.

But he biosphere plays some part. If you are going to do it, you ought to try to do it reasonably well. I also think that there may well be some real couplings between the biosphere model and the geosphere model that simply are not accounted for in the present way of doing things. We learned yesterday that in modeling root uptake from soil by plants that there is a correlation accounted for between distribution coefficients Kd and root uptake factors Bv.

The same kind of correlation presumably applies to whatever distribution coefficient you assumed in your transport model to get to the well. There could well be some correlations. When you don't

account for these, you may under-represent the overall uncertainty in the system when you do stochastic modeling.

I don't think there are a large number of these couplings that would be significant. But it's probably worth some thought. I also agree with Dave's comment --

MEMBER HORNBERGER: Dave, can I? Just for clarification, when you talk about coupling of the transport system to plant uptake, you are suggesting there could be a feedback on the transport from uptake. I lost that.

DR. KOCHER: No, it could be that the appropriate value of a root uptake value is correlated with whatever Kd you assumed to transport the stuff to the well because it's known in its soils that for high Kd things, the root uptake factor is low. For low Kd things, the root uptake factor tends to be high.

So by not accounting for these correlations, you might under-represent uncertainty. If you just treat everything as independent, of course, if you have enough variables, your uncertainty shrinks to very little. I don't think it's a big deal. But it's worth thinking about have you cost yourself something by doing this complete decoupling

of the biosphere model from everything?

MEMBER HORNBERGER: From what consistency then correlation?

DR. THORNE: No, could I come in? I think it's a genuine correlation. What Dave is saying is that the mineralogy and texture of the soils is related to the mineralogy and texture of the underlying materials through which the radionuclides are passed. Unless you recognize that these are related materials, you won't build in the proper correlations between the Kd values that are appropriate to that material.

It's that underlying nature of the physical system which I think goes back to Dade's point that if you have a full site description report, you will recognize those mineralogical and textural relationships in the description. Then you will build them into the model subsequently.

MEMBER WEINER: Isn't this what the PA, performance assessment, in the general sense, does anyway, or are you suggesting something beyond what performance assessment does?

DR. KOCHER: What we have learned so far is that these kinds of correlations are not accounted for because the stochastic modeling of the part of the

performance assessment that gets you to a concentration in a well is completely decoupled from whatever kind of stochastic uncertainty analysis you do for the biosphere component.

MEMBER WEINER: But the way the performance assessment works is that each distributed variable is sampled on. Yes, the samplings are independent. But you are certainly taking into account the uncertainties in both sets of variables.

DR. KOCHER: But the problem is this.

MEMBER WEINER: I'm just asking beyond that.

DR. KOCHER: In your geosphere model, if you by random sampling select a low value of Kd for your transport calculation and then you at random assume a low root uptake over here in the biosphere model, you have ignored that correlation completely. Let me give you a simple example. Suppose you have a bunch of film badge readings.

You make a bunch of readings on a film badge and you want to add them up to get the dose and you want to take into account uncertainty. If you treat the uncertainty in each film badge reading as a random thing, the more badge readings you have, the lower the uncertainty is going to get. But if there's

correlations, the uncertainty doesn't get as low. And you have the same potential problem here.

MEMBER WEINER: Yes, and I can see that. It seems to me that what you are suggesting, and I know we did this on the web and I haven't looked at the TSPA that closely, but there was an attempt to do stratified sampling, do latin hypercube (PH) sampling so that you at least sample more or less equally from the entire range. Now, you're suggesting something else. That's what I was trying to get to. You're suggesting that the performance assessment include positive correlations in addition to just the random sampling of uncertainty.

DR. KOCHER: I'm just suggesting that this is worth looking into to see if it matters. There could be others. I haven't really thought about this. Climate is a tricky business that I know nothing about. But it clearly affects both suites of models.

Are there correlations in your climate change model that you are losing by treating climate as some kind of stochastic variable but treat them completely independently in the geosphere transport part and the biosphere part? Are you losing something by this total decoupling? I have no idea how important this is. But I'm just concerned that

something might be lost when you do this.

DR. MOELLER: Excuse me, David, I'm being nudged from my left that we need to move along. But go ahead and cover your other points. Perhaps we can do the discussion later today.

VICE CHAIRMAN RYAN: That would be great.

DR. MOELLER: But raise your points.

DR. KOCHER: Yes, I have some specific technical comments some of which I have transmitted to the DOE people already and probably should wait until later. It's about the modeling and parameters that they chose. I won't deal with that here.

DR. MOELLER: Okay, thank you. Jeff, why don't we move ahead then?

DR. DANIELS: My comments pretty much echo what you have had to say, Dade. I would only like to add that the extra informative calculations are an imperative. It's very important that we understand in a comparative sense what the age specific dose may be. People want to know. The other thing that I think is relevant here is there is a risk assessment performed.

It doesn't stop exclusively with the dose calculations. It would be done with the appropriate dose conversion factors along the lines of Federal Guidance 13. While this is certainly in the extra

informative calculations, it's what the public is not only asking for but is demanding.

Remember, we're talking here about a situation that is a prospective understanding. We're not talking about an epidemiologic study where people are being exposed and there is evidence of effect. We don't know what that effect might be. Unfortunately we have a model that says it's linear through zero and there's no threshold. At this point, that's the best we can do. It seems to be a conservative model for the purposes of analyses that are prospective.

So I think it's deficient not to advise the public what those numbers are. I think it's also important to recognize that because of the difficulty in comprehending the way MCLs are derived in the present based on the way they were derived in the past, there's an issue here that says risk may be the unifying thing. There's been arguments within the EPA about how the MCLs should be appropriately adjusted.

The fact is that they won't be raised. But they could be lowered. The fact is that with all of that understanding taken into account, there's a great deal of confusion among the public about what might be considered right. But science moves forward.

Thanks to Keith and the new biokinetic

modeling processes that exist, there's a better understanding of how that dose is converted. There's an understanding within the context of today's understanding of the risk what those numbers translate into. Ultimately the public wants to know.

The other points that I would like to make just in passing are I have to commend the process as it exists right now because we're here due to the fact that there is a defensibility and credibility to the documentation. In the past, it may have been a back of the envelope calculation that was done with a certain degree of conservatism that everybody said this is realistic or unrealistic in that case.

The compliance documents have now improved to the point where we can take into account a reasonably maximumly exposed individual. It's important to emphasize what that means. Maximumly exposed, this isn't just to say that it's going to be everyone in the population. It's to say that we're taking into account a certain degree of conservatism as Dr. Till has mentioned, and we fixed it at two liters a day for an adult.

Now, it's important to recognize within extra informative information what that is prospectively related to a child or a teenage. It's

also important to qualify the calculations to recognize that indeed the dose conversion factors assume, and you can correct me if I'm wrong, Keith, but that you are going to get the annual dose all at one time which is a little insincere but is conservative.

With regard to pathway exposure factors and uptake versus intake, I think the best that can be said right now in the process is that these things be documented well and that they be transparent in the way that the calculations are conveyed both to the public and to the regulatory agencies. Meetings like this continue in the licensing process so that all of the concerns, as you brought up, Dr. Kocher, are vented. That's the points I would like to make.

DR. MOELLER: Thank you. Mike.

DR. THORNE: You might feel that coming last I wouldn't have anything to say. But I have one or two extra points. Let me endorse or suggest a way forward on the RMEI. Obviously we are stuck with the RMEI. I think what I missed yesterday was a narrative that establishes the consistency between the RMEI and the biosphere model configuration and parameterization. That narrative would help us to see why the calculation was what it was.

I think, just endorsing John's point, that I would like to see a supplementary calculation with a conventional critical group approach. There is a gut feeling that it doesn't make an enormous difference. But it would be nice to see that quantified. And I don't think that's too difficult to do.

Effectively, internationally there's a lot of discussion on the geosphere biosphere interface zone. It's recognized as a significant source of uncertainty. Effectively, it's regulated out here by the 3,000 acre feet rule. Again, if we're talking supplementary calculations, that's an obvious candidate for variant calculations to show the implications of that regulatory decision.

I would mention that's currently being addressed in the Bio-Prata (PH) project which I know the Yucca Mountain project people have an involvement in. So this is not going to be a new story to them. I think the detailed analysis for contributions by pathway was very welcome. Again, the words that come to mind here are a narrative is what I'm looking for there that describes why the results are what they are and how they could be different if I made different conceptual assumptions or different parametric

assumptions.

We talked about uncertainty and sensitivity analyses yesterday. I think I'd like to see an explicit recognition that both types of analysis are appropriate and that they are complimentary to each other in helping to explain the system. We sort of touched on specific activity models both on the iodine 129 and actually effectively on the Carbon 14 and fish issues.

I think that reveals to you that specific activity arguments can be useful. But they have to be used with considerable care and you have to decide what are the stable pools that are mixing with each other in the system? If you don't get that straight, you get the wrong answers.

Another big message I would send is Redox sensitivity. For things like iodine and technetium and a number of the actinides, chemical speciation and changes with oxidizing conditions are a major factor. I don't necessarily believe that those should be built in at the level of assessment models. But it's interesting when you look at the TSPA that the other parts of the model are underlay detail models which inform the actual assessment level model.

I don't see the same relationship between

the assessment level model in the biosphere and detailed process-based models to define and justify the parameterization and the conceptualization. I'm thinking of things like soil column-type models where you explicitly use Richard's equation where you consider the kinetics of the processes. The traceability from detailed process modeling is an area that could perhaps be useful.

FEP analysis we briefly mentioned. One thing that affects me about the FEP analysis is not surprisingly because it's based upon international experience the FEPs are described at a very high level. They are things like human lifestyle or inhalation which are nice motherhood words. But they don't actually give me a very big handle on how to build a real model.

But I think we saw very usefully that the interaction matrix approach is being applied. I think that gives you a very scrutable audit trail. I would draw attention to the ongoing work of the International Union of Radioecology in that area. I would very much encourage that there is talk between the DOE, the NRC and the IUR program in that area because I think that's where we'll develop much more structured modeling approaches.

I am happy with the compartmental modeling approach. That's standard international practice, as I say, at the assessment level, though I think we need to underpin with process modeling. I am concerned also that spacial heterogeneities in the system are not represented in the model, that we treat the biosphere as if each of those compartments was a homogeneous system. We know that spacial in homogeneity in soil characteristics will exist.

I was a little concerned with the fact that when the activity has passed down through the soil zone, and this goes back to Dave's question on correlation and interactions, that the radioactivity disappears from the system. Now, if I take the NRC example where they irrigate for 15 years, 15 years will drive a soluble nuclide down in the soil.

But when you turn off the irrigation, there will be a net soil moisture deficit and effectively there will be an upward suction. The activity that moved down five or ten meters will move back up again. You have the problem there that you can build up a reservoir depth which is then recovered to the soil zone and is available for exposure again. It's that sort of interaction which is basically an understanding that the surface hydrology and its

coupling to transport that is the sort of thing that I see embedded in a process model.

I'm nearly there you will be glad to know. These are the highlights. One thing we didn't touch on yesterday is when I look at the underlying literature, I would commend the literature. I think the description of the new ERMYN model and the description of where do all the parameters come from is impeccably done. I can see where every number came from which of course allows me to ask more questions about them.

One of the things that strikes me there is that many of the value hues (PH) are derived from secondary reviews of the literature. This has a number of potential problems. Some of those reviews are very dated. One is that these is a Beas Review from 1984, an excellent review in its time but 20 years old. Others are reviews that we use as a basis for other models.

Sometimes those reviews don't fully consider the full range of the primary literature. Sometimes, as in IAE technical report series 364, they are internally inconsistent. For example, animal transfer factors are sometimes considerably higher for goats than for cows for no reason from the underlying

primary literature and notwithstanding the fact that the goat is ten times as small as a cow. That just is logically wrong.

Sometimes the values are not applicable in the local context. The Carbon 14 fact for fish which is taking from IAE 364 is one example of that where it might be okay if you were in a contaminated ecosystem where everything was contaminated. But it's not okay when you are in a fish farm where just the water is contaminated.

There's a correlation to be mentioned. When you have several secondary reviewers, you often find that they point to exactly the same single primary literature source. You can't treat the numbers from the secondary reviewers as if they were independent variables for the purpose of determining a distribution.

I'm a bit surprised that the DOE has not at some point undertaken its own comprehensive review of the primary literature on transfer factors which would seem to me as a desk study a relatively limited cost operation and that you would get enormous benefits from it. And the international community, incidentally, would get enormous benefits from it.

Climate change, it is curious that there

is no recognition even of the possibility of greenhouse warm states and the potential new analog characteristics in the system. I'm not saying that DOE should definitively assume that greenhouse warming will occur. But it should at least be recognized as a possibility and calculations should be made I think for those alternative states. That's being addressed extensively internationally.

Finally, on dosimetry, I think I agree with everybody that use of good science implies use of the latest ICRP, biokinetic, and dosimetric models. We did have a discussion yesterday about where you should look at variability and uncertainty. I would suggest that possibly you might limit that to sensitivity studies for alternative values for aerosol solubility, alternative aerosol sizes, and alternative gastrointestinal absorption and leave the systemic bits of the model alone because that gets complicated because the systemic models are carefully tuned.

There's a lot of correlations between the internal parameters. If you get into that business, I think we should leave that to Keith if anyone is going to do it. That's what I have. Sorry, that was quite a shopping list.

DR. MOELLER: No, that was great.

VICE CHAIRMAN RYAN: That was an excellent summary of where we have been so far today. I guess what I would suggest is that we turn our attention to our first speaker. We can certainly pick up all of these points as people think about them and digest them and hear these presentations. Then we'll come back for a full discussion and questions.

So, our first speaker up is Mr. Pat LaPlante who is a senior research scientist from the Center for Nuclear Waste Regulatory Analyses. For those of you that did not recognize yesterday, we have staff from the center on the TV screen. I'm sure they can see us as well.

MR. LAPLANTE: Hello. Can everybody hear me? My name is Pat LaPlante. I work for the Center for Nuclear Waste Regulatory Analyses, the technical support contractor for the NRC in the high level waste program. Today I'm going to discuss risk insights for biosphere modeling. I don't have a whole lot of time so I'm going to provide a general overview. We'll have time for questions, and we can get into some details if you would like.

In general, I'm going to talk about how our reviews of DOE documents are risk-informed. I'm going to provide an overview of the biosphere risk

insights which will be consistent with what I presented yesterday. I'm going to discuss the agreements that came out of our DOE document reviews and how those were risk ranked or significance ranked, I should say, and discuss some of the effects of the risk insights on our current work plans.

As I mentioned yesterday, we have been conducting dose assessments for quite a while, since the early `90s. So leading into the DOE document reviews for the site recommendation, we already had a fair amount of understanding of the basic system processes. These were process level, modeling, and sensitivity studies that have been published in the past as well as an TSPA, total system performance assessment code development activity which has gone on since the early `90s to the present.

That's included continued refinement of the biosphere models and parameters including looking at intermediate results and doing confirmatory calculations, verification, and so forth. That whole activity has given us vast insights into how the models are operating. So when we did the DOE document review supporting the site recommendation report, we did focus our reviews on those areas that we knew were driving the calculations.

This was based on a process level understanding because those were the tools that we had at the time. The risk insights initiative, in full swing, began after we had developed the comments on the DOE documents. During that time, we had enhanced our total system performance assessment code to allow sensitivity analyses at the total system level on the biosphere parameters because we had actually included the biosphere model completely into our total system performance assessment codes.

So this allowed us the ability to understand how the individual biosphere parameters were affecting the total system performance rather than just the dose that was calculated within the biosphere as a separate process model. The risk insights initiative used this information to significance rank the agreements we had already made with DOE that they would provide information to resolve our comments.

The risk insights essentially provided a context to help us resolve the agreements. How much information do we need on certain topics if they are either important or not so important in the total system calculation? In general, our technical work over the years has been directed towards important

topics with large uncertainties. Obviously we're not spending a lot of time focusing on the drinking water consumption rate or that type of idea.

This slide provides just a basic overview of our risk insights for the biosphere modeling. This is consistent with what I presented yesterday. I've broken it down into insights related to the groundwater release, biosphere pathways, and those related to igneous activity release. In general, for the groundwater release pathways, we're seeing about 50 percent of the dose due to drinking water and about 40 percent due to crop consumption. Again, this is for key radionuclides that are driving the calculation.

The key parameters that we have determined in the process level sensitivity studies include distribution coefficients, plant transfer factors, crop interception which is deposition of material on the crop surface. In general in the crop contamination models, you get a certain amount that's deposited directly on the surface and a certain amount that comes up through the roots. That's what gets you your crop ingestion dose.

The uncertainty in the groundwater biosphere calculations is low relative to other

abstractions. This influences the overall importance of the groundwater release biosphere pathways and the total system calculation.

For the igneous activity release scenario, as I said yesterday, inhalation pathway dominates. That's fairly clear. Key parameters include mass loading and some of the exposure duration parameters. Mass loading is sort of a lumped parameter that includes a number of processes. Of course any of those processes that are driving the mass loading could also be important.

VICE CHAIRMAN RYAN: Excuse me. Because of the problem with the slides, he needs a four minute, everybody keep their place break. I hate to interrupt you. But that way, folks will be able to see your slides.

MR. LAPLANTE: Okay, sure.

VICE CHAIRMAN RYAN: So let's just take a quick break right in place.

(Pause.)

MEMBER HORNBERGER: One thing I'd like to know is that we keep using very qualitative terms; low, high, medium, et cetera.

MR. LAPLANTE: When I say "low," I mean relative to other abstractions.

MEMBER HORNBERGER: Relative to other abstractions.

MR. LAPLANTE: The one I presented yesterday gave you in a quantitative idea the level of uncertainty that we're propagating just in the biosphere calculations. Within our calculations, it's within an order of magnitude that's slightly less than that. DOE, as you heard, have more elements in their model such as swamp coolers and slightly more involved climate fluctuations and so forth.

They are propagating slightly more now. They used to be propagating less than we were. Now, with the new model, they are within about an order of magnitude. But if you consider that, some of these other abstractions, waste package corrosion or whatever, have many orders of magnitude of variation. So as those are causing the dose to flop around, the biosphere is just in the background noise.

So that's the conceptualization at a high level of how the uncertainty in the biosphere relates to the total system uncertainty. You've seen those horsehair diagrams, the TPA output. The variation is quite large from the total system.

CHAIRMAN GARRICK: Question, depending how you do in your uncertainty analysis, the sensitivity

analysis is a subset of that.

MR. LAPLANTE: Right.

CHAIRMAN GARRICK: It's very easy to pull out. If you have a PDF that's an accumulation of a lot of contributions, it's very easy to pull out the PDFs that make that up and display very graphically the sensitivity as well as the uncertainty.

DR. THORNE: Could I just comment on that? I think we are in danger of missing something there. We're in danger of thinking that all uncertainty is parameter value uncertainty. To my mind, the bigger issue in the biosphere is conceptual model. Have we got the structure right?

CHAIRMAN GARRICK: Yes, sure.

DR. THORNE: You don't get at that by doing a Monte Carlo simulation. You do that by brainstorming alternative conceptual models and running them through the system.

DR. ECKERMAN: Right, exactly.

MR. LAPLANTE: Right, although I think you might agree that this biosphere is not extremely complex compared to some biospheres. It's an arid environment. There aren't a large variety of activities. The rule constrains some of the aspects of the conceptual model in a way. Also, given what

DOE presented yesterday, it's a little bit more of a mathematical model.

When they ran a bunch of different biosphere models, they get the same results. I know a conceptual model might draw you to slightly different biomathematical models. I understand when you get into the details you can come up with all kinds of different conceptual models.

But I think our models are maybe a level above that that consider most of what we would expect to be occurring in the biosphere. I don't see a lot of alternative conceptual models that are missing. If you know of one, we're certainly open to hearing suggestions of what conceptual models are being missed.

DR. THORNE: I think we're talking across purposes in a sense. The conceptual model I had as an example was the one I gave earlier where the radionuclide moved to depths, is accumulated in reservoir depths, and then because of changes of either human irrigation or environmental conditions, that reservoir becomes available. Now, that actually falls outside the scope of the standard biosphere models which typically operate top of the soil down into about the base of the subsoil but don't operate

to 15, 20, 30 meters of depth. It's that wider conceptualization that raises the issues in my mind.

MR. LAPLANTE: Is that type of process really what you would expect, or is this more speculative?

DR. ECKERMAN: No, letting the agriculture land rest under heavy irrigation is often done. You irrigate a field for a period of time especially because the solids build up. Then you let that track of land rest, not be in an agriculture practice, and then come back and irrigate later.

MR. LAPLANTE: Right, I understand that.

DR. ECKERMAN: So there's a rotation like how you rotate crops.

MR. LAPLANTE: But I mean the upwelling.

DR. ECKERMAN: The upwelling could occur during those periods.

CHAIRMAN GARRICK: Certainly when I talk about uncertainty I'm thinking both aleatory and epistemic uncertainties. I'm not thinking of just information uncertainties. You can convolute both into the same distributions. You can disassemble the information in such a way to display the relative contributions of both types of uncertainty. And you can decompose it into the sensitivity component as

well if you do it from the ground up with a comprehensive uncertainty model.

MR. LAPLANTE: Isn't the iodine and technetium fairly mobile in terms of wouldn't that continue to wash through?

DR. THORNE: Technetium is only mobile in oxidizing conditions. It's essentially completely immobile in reducing conditions. Those are the conditions that exist below the phreatic surface. So if you have created a water table at depth, then technetium will essentially be stuck where the Redox potential is less than minus 100 millirems or possibility even a bit higher.

With iodine, iodine tends to be immobile in oxidizing conditions with high organic content in the system. So if you have an organic layer and the iodine hits it, then it will tend to stop. So it depends on the chemistry.

MR. LAPLANTE: But would there be an organic layer?

DR. THORNE: Well, that's part of site characterization.

MR. LAPLANTE: Yes, okay. Moving forward, for igneous activity, I think I already went through that. So I think we're on the next slide. In the

next few slides, I'm going to go through the agreements that were established that DOE would resolve certain comments that we asked on the site recommendation report.

I have separated these up into those related to mostly or are more applicable to the groundwater release biosphere pathways. Then after that, I'll discuss some related to the igneous activity release biosphere calculations. The biosphere groundwater pathway modeling agreement topics are generally ranked low significance. If anyone is interested in seeing the detailed descriptions or paraphrasing of the agreements, I have included these on backup slides number 10 and 11.

I'm summarizing them in these view graphs. But you can keep them handy. If you are interested in looking at them, you can. The low significance, again, is related to the low variability in the biosphere. When we made the comments, we did emphasize those parameters that were found to be important in the biosphere process modeling decoupled from the total system calculation.

These included soil partition coefficients, Kds for soil leaching calculations, plant transfer factors, the crop interception. We

also had some comments on the sampling approach. To some degree this related to what Dr. Kocher was mentioning about the decoupling. They had decoupled their biosphere sampling from the total system sampling. We were asking them to tell us whether that was biasing the results or not.

That was maybe less of a numerical importance issue as more of just a how are you doing it. So in general with these risk insights, the effect on our biosphere plans were that we really don't have any plans to do major technical work in this area. I think things are in pretty good shape. DOE subsequently has improved their documentation, as was noted.

They have gone actually quite far in documenting everything. You can identify every parameter that they are using in their modeling. So that resolved these core agreements that we had on the SR. Of course, we will continue to monitor as the documents come in whether they are changing anything and so forth. But we're not really conducting much additional work in that area.

This is just to provide an example of the type of technical information we used to supplement our risk insights when we were focusing on these

particular specific agreement issues. I've chosen the plant transfer parameter as an example. We did have an agreement on that. We're asking DOE I believe to justify the site relevance of their plant transfer factor choices.

So to get an idea, numerically how is this factor affecting our total system performance assessment results, in addition to what we have known from previous analyses, we did a simple perturbation analysis where we perturbed the parameter that's normally sampled at the high and low ends of the range. We can see from here at the 10,000 years it can increase from the base case, totally stochastic, total system calculation about a factor of 3.7.

That's a fairly extreme perturbation because normally you would want to look at does the distribution of that parameter shift to a higher level? This is actually going to the end of where the point value would be. So the conclusion here would be not very significant increase in risk, low risk significance or low significance ranking.

Doing a similar look at the igneous activity-related biosphere agreements, the igneous activity biosphere agreement topics have more varied significance rankings. Those related to mass loading

and inhalation of ash were ranked high or medium. Those that were ranked low were related more to documentation and bases for certain parameters or modeling assumptions.

The inhalation pathway and mass loading, as I have already mentioned before, is highly significant. Our total system calculations, I've already mentioned that so next slide.

DR. KOCHER: This is because this scenario basically bypasses the geosphere.

MR. LAPLANTE: Yes, pretty much, release of the inventory directly into the air from a volcanic intrusion. You don't wait for the decay of things like americium 241. Okay, next slide. So the effect of these risk insights on our biosphere plans are ongoing model development and risk analyses. So we're continuing to dig into this area because it is affecting the total system results.

This work includes refinement of the inhalation models. We discussed a little bit yesterday about looking into the particle size assumptions and better integrating the transport and mass loading models, getting a better understanding of the duration of the mass loading over time and how remobilization of ash after its been deposited might

affect that mass loading over time as well as the magnitude of the value. Again, this is ongoing work so we continue to look at it. I know DOE is continuing to look at their approach as well.

So in summary, the use of risk insights is integral to planning and conducting staff work. Risk informing is an iterative process. It's a learning process. We obviously continue to iterate our calculations and assessments. As I said yesterday, what we knew five years ago was more focused on the process modeling. Within the biosphere, what's important to that calculation? Now, with enhanced capabilities, we can look how is the biosphere affecting the total system results?

I guess the walk away with message would be inhalation of volcanic ash is highly significant. So we have additional work ongoing. The remainder of the biosphere calculations are much less significant to total system performance. Therefore, we don't have any addition work planned other than to monitor what DOE is doing. Of course, eventually we'll be reviewing their license application. That's it.

VICE CHAIRMAN RYAN: Thank you very much. Let me pick up on a point that Michael Thorne made earlier. That is that the risk significant issue

identified which is inhalation of volcanic ash I imagine would be particularly sensitive to those three issues of solubility, particle size, and other parameters that would pretty dramatically shift the inhaled quantity.

MR. LAPLANTE: Yes.

VICE CHAIRMAN RYAN: Again, I concur with the idea, and we touched on it yesterday, of we're really talking about the intake, not the uptake. Let me define that again. The intake is what I breathe in. The uptake is once we get to the blood and we take it forward into organs and calculate those. I concur fully that Dr. Eckerman has a handle on that for us all. We probably don't need to challenge that nearly as much as we need to think about accurately assessing that intake and the ramifications of the variation of that intake.

MR. LAPLANTE: Right, yes, we would agree with that.

VICE CHAIRMAN RYAN: Any questions or comments? David.

DR. KOCHER: I guess I wanted to accept your challenge about alternative conceptual models. I'm pretty sure that a first order biokinetic model for soil erosion is not right. I'm almost sure that

a first order biokinetic model for retention and surface soil going downward is not right either.

MR. LAPLANTE: Right.

VICE CHAIRMAN RYAN: I'll ask the question. What is?

MR. LAPLANTE: I would just say I think it's recognized in the technical community that those models are very simplistic models. In general, if you talk to geochemists, they really don't like the Kd approach because it's a vast simplification of a very complex geochemical system. Yet, the dilemma is once you go further into the details, you're dealing at the atomic level with complex geochemical processes. It ends up becoming a very long, drawn out project.

So I accept the comment. I think we do need to take a look at how alternative models might impact those processes. But we also have to be sensitive to the fact that we can't spend a whole lot of time and resources if it's not going to impact the overall results. There might be some way to more simply bound the effect.

VICE CHAIRMAN RYAN: Michael.

DR. THORNE: Perhaps it's just worth looking at what's being done in one or two other programs. The one I know about is the MACCS program

where we are, for example, at the Imperial College using a 3-D transport equation for soil based on the richness equation to get the flow in the system and then admittedly using an equilibrium Kd in that model.

Then we're using a biogeochemical model based on the SUTRA system but with the add on flow and transport component. So in a way, our soils are looking more like what you actually do in process modeling in the geosphere because the processes are actually quite the same.

MR. LAPLANTE: Have you compared those models with the simpler models just as a matter of interest?

DR. THORNE: We compared the earlier 1-D version, the SPW-1 and SLT-1 models. Those were studied in BIOMASS-2 in the validation exercise against the lacimeter (PH) experiment. We've also compared the data for effects like ground freezing which we observed in our lacimeter (PH). So we looked at things like validation of the model against solude (PH) exclusion and solude (PH) recovery in freezing. So in as far as we can validate those models, we have done so.

The other one that we use, going back to the point that I was making earlier, is the SHETRAN

surface water catch model which basically covers the surface hydrology and subsurface hydrogeological regime in a spatially distributed sense down to about 50 to 80 meters because that's the interesting zone. I don't want to go into the details. But there are programs where more physically-based models are being deployed to underpin the assessment models.

In fact, when I go back, I'm having a discussion with SKB who will be using a similar suite of models, a mixture of possibly SHETRAN, Darcy Tools-type models to explore these near surface processes. But I think if you think near surface processes rather than biosphere, you have a better flavor for what the issues are.

MR. LAPLANTE: One thing to keep in mind before Chris goes is for the igneous activity calculations, I don't believe leaching is a predominant factor in our modeling results because most of those radionuclides, like americium and plutonium, that are driving the dose are staying pretty much in the ash blanket. Now, for the groundwater pathways, we're mostly talking about technetium, iodine, and uptunium (PH).

I know this could change if you change the models. But the drinking water pathways is 50 percent

of that dose. So the question would be, how much could you alter that other 50 percent of the dose which is from the rest of the biosphere by implementing a different soil model? That's the key question. If it's going to be just a small amount, then it obviously might not be worth it to spend a lot of effort in that area.

We have a very large program with a lot of uncertainty and other total system models that are going to be more important for the total system results and understanding repository behavior. Do you want to divert resources from focusing on waste package corrosion to get into detailed three dimensional soil modeling? That's how we have to weigh the decisions on how deep to go and use risk insights and make those decisions. Chris, you wanted to add something.

MR. MCKENNEY: I just wanted a point of clarification. Those lacimeter (PH) studies and other ones that I know of were all for below ground sources. They were not for irrigation sources above. The BIOMASS-2 were all lacimeter (PH) studies where the source was added below the ground and the roots pulled the water up the soil column which is a different phenomena than what we experience with the over-

watering. They are there, but whether it's actually an applicable analog would be a question.

DR. THORNE: Yes, I'll come back now. I'm not saying that those are an accurate analog. What I'm saying is that the type of structural model may be useful in this context. It's interesting. The experiments are now being jointly sponsored by ANDRA because ANDRA is interested in the irrigation pathway. So the extension of those experiments is now to the irrigation pathway as well as the upwelling pathway.

VICE CHAIRMAN RYAN: Thank you. Any other questions from others? Yes, Ruth.

MEMBER WEINER: Since your inhalation for the volcanic intrusion has a high significance, what kind of work are you doing to bound the uncertainty in particle size in order to look at particle size distribution?

MR. LAPLANTE: Well, we are currently looking into the transport models. We're looking at alternative transport models. As part of that, they are looking into particle size assumptions that are inherent to those models in the mass loading. All the work is infused with particle size considerations. The work is ongoing. Tim might be able to add more technical detail to it or perhaps some of our staff in

San Antonio.

But like I said before, the key uncertainty is we're dealing with volcanic ash. It's fairly fine particles. There isn't a lot of data on volcanic ash. We've collected some on volcanic ash in Nicaragua. There is spotty information here and there. But I think you have to look to analogs and so forth. Tim.

VICE CHAIRMAN RYAN: Pat, maybe we could defer to Keith Eckerman for a question. Most of the action in occupational exposure circumstances are below 20 microns. Probably somewhere around a micron is not a bad number to think about for a lot of occupational sites. Could you give us some insight as to what's happening between say 10 and 100 microns of what we really know? I know the ICRP has a model to extend to larger particle sizes. What do you think of that? What advice could you give us on that point?

DR. ECKERMAN: Well, the ICRP model, as you just said, has a complete deposition model that's run out to particles as large as 100 microns. You have to consider the inhalability and how the individual is actually coupled with the windspeed. It gets complicated.

VICE CHAIRMAN RYAN: Well, let me just

shape that a little bit more.

DR. ECKERMAN: Let me just go back. In occupational, through the years, like in Federal Guidance 11, we used a one micron assumption with regard to the particle size. Now, in publication 68 which is the ICRP document for the worker using the newer model, we've gone to five microns as more typical of the kind of aerosols that are encountered in the work place.

Now, for the general public, assuming that normal releases from facilities have gone through a processing system and through filters and so forth, we retain the default size as one micron. The data for sizes out to AMEDS (PH) all the way from aerosols that you have to characterize by their thermodynamic properties rather than their particle size, density, so from atomic sizes almost up to ten microns, our dose coefficients appear on that ICRP CD to cover that range.

It's difficult to get much to go beyond much an AMED (PH) of ten microns with the current information that's available. But for these studies dealing with volcanic ash, the inhalation model that you really should be using is of course not the old one of Federal Guidance 11 but you should be using the

newer model because it's responsive to those issues. I'm sure that's the case.

MR. LAPLANTE: Right, we're using or we're currently looking into doing calculations with the newer models to provide a better idea of how much overestimation there is in using the previous models because the newer models are more refined.

DR. ECKERMAN: Now, I think on the Mt. St. Helens event, even the folks at North West Laboratory, there was a lot of effort to collect particle size information and so forth. That was a different kind of --

MR. LAPLANTE: Yes, it was a different kind of eruption. It's also arrange a lot in that part of Washington State. I know there's air of regions in Spokane. But it's not the best analog.

DR. ECKERMAN: Right, I agree it's not the best.

MR. LAPLANTE: There have been discussions between the NRC and DOE on that topic as well.

VICE CHAIRMAN RYAN: Could you expand just a bit, Keith? You said you have to couple the individual at the exposure with windspeed, direction, and so forth. Gravitational settling obviously becomes much more important as the particle size get

bigger and things leave air streams more quickly rather than stay in them.

So what advice would you give in terms of trying to create a range of scenarios? This is for material that's been deposited and resuspended. So that's really where we're starting. We're not looking at the volcanic ash plume coming by. We're looking at a redeposition and then the inhalation. What would you do there?

DR. ECKERMAN: Well, first, it would be useful to look at the information you have on particle size in mass loading situations. What's going to be resuspended, as you said, are going to be the fines. With lack of any better information, I think I would start by assuming that those may well be on the order of five micron AMAD (PH) size.

MR. LAPLANTE: Right, I think our understanding, and someone can correct me if I'm wrong, but the resuspendable particles generally are below the hundred micron range.

DR. ECKERMAN: Yes.

MR. LAPLANTE: That bounds the problem there. Inhalables, I think less than ten or deep into the lungs. So in between that ten and 100 there's --

DR. ECKERMAN: You must have data on what

the density of that material would be. I don't happen to have that in the back of my mind. That's the first thing of course you want to look at is what's the physical density of the material?

MR. LAPLANTE: Right.

DR. ECKERMAN: That information I'm sure is available to you.

DR. THORNE: It must be about two grams per --

DR. ECKERMAN: And I would expect that it's an order of a couple of grams.

MEMBER WEINER: The question I had is do you have any idea of what fraction of what is entrained in the ash plume would be in that particle size range? That's what I meant by particle size distribution really. How much? Because that's the critical thing.

MR. LAPLANTE: What fraction of spent fuel, is that what you are asking?

MEMBER WEINER: Yes, what fraction of what is entrained in the igneous upwelling, if you will, is of the particle size that can be resuspended?

MR. LAPLANTE: Right.

MEMBER WEINER: Has a micron AMED (PH) of about one or two grams per cubic centimeter density.

MR. LAPLANTE: Well, that again I think takes it back to the original source term release calculations. We're looking at the whole calculation. There's no analog really for a volcano erupting underneath a repository. So there's inherently some assumptions about particle sizes and so forth. Generally, we're doing that conservatively. Tim would like to say something.

DR. MCCARTIN: Yes, currently mass loading is one of those parameters that has a lot of processes in it. As Pat indicated, we are in the process of trying to better quantify the uncertainties in all of the assumptions and try to lay that out in a systematic way what the assumptions are and better understand ourselves what is the impact on the dose estimate? As Pat appropriately mentioned before, where do we want to focus the studies and the interest?

VICE CHAIRMAN RYAN: I think in our questions, Pat, we've covered the gamut from source to transport deposition to resuspension to inhalation. So there's a lot of ground covered there. Some of the things carry through and some of them are unique. Solubility is another one obviously that's a driver. If you assume Y class or W class or under the new

categories and so forth, you come up with big changes in numbers. I think you are on the right track. But I didn't want to miss the opportunity to pick Keith's brain while we're all here.

MR. LAPLANTE: Right.

VICE CHAIRMAN RYAN: Mike. Oh, I'm sorry, go ahead.

DR. DANIELS: Do I understand correctly? Are you actually making a coupled model here? You are not uncoupling the BDCF process from the TSPA in this particular case.

MR. LAPLANTE: Yes, that's correct. The calculation mathematically is not uncoupled. For the sake of implementing the calculation in our total system performance assessment code, we do run the GENII code with a unit concentration to start with. Then the resulting dose is multiplied by the concentration. That whole calculation is integrated into the total system realization by realization calculation approach.

So we're not doing the biosphere modeling outside our total system model and then sampling those results like what DOE is doing. We made a conscious decision that we thought it would be better to have that calculation integrated with the total system

parameter sampling and so forth so we could assess sensitivities of individual biosphere parameters on the total system results.

DR. DANIELS: Can you then also at least qualitatively add these relationships that Dr. Kocher was putting out? Can you somehow see if there are --

MR. LAPLANTE: You mean coupling the biosphere with the saturated zone transport, like the Kd issue that he was talking about.

DR. DANIELS: Exactly, is that possible?

MR. LAPLANTE: I was listening to that discussion. We have the capability to coordinate parameters in our TPA code. I believe we can correlate any of them that we want to. We don't stop the thinking if we don't explicitly correlate certain parameters. We obviously thought about the issue of G (PH).

Our hydrologists are making assumptions about the chemistry of the material as it transports through the groundwater. How does that impact the chemistry of the material as it enters the biosphere? Again, once you get into geochemistry, things become very complicated very quickly. In the case of the groundwater scenario, once that contaminated groundwater comes out of that sprinkler and is sprayed

through the air and it contacts the soil, there's all kinds of potential transformations that can take place chemically.

So consulting with our geochemists, we couldn't come up with a very clean association. There's also the soil properties themselves that are not the same as the properties in the groundwater path geochemically. So they can be considered as separate systems.

MEMBER CLARKE: Excuse me, the decoupling issue is a good issue. But I think this might be a bad example because it makes sense to me to have one set of Kds for deep transport then another set of Kds for the near surface soils.

MR. LAPLANTE: Right, we tried.

MEMBER CLARKE: I'm not sure.

MR. LAPLANTE: We tried in our modeling process. I don't just put on the blinders on and say I'm just focusing on the biosphere. We're constantly interacting with our other abstraction modelers. If there is something that they're doing that's influencing something that I'm doing, we make that integration and discuss what the potential ramifications are.

There aren't a lot like Dr. Kocher said.

We haven't come up with a lot of these types of interfaces. But there are some that come to mind and that was one of them. There is chemistry assumptions. How does that affect what we're doing in the biosphere? The particle size issue obviously, we have looked into that. The air transport modeler is --

VICE CHAIRMAN RYAN: And we appreciate that's a work in progress so we'll hear more about that later. I think we're at a point where we need to press on to our next speaker if we may and come back to any other questions on this issue.

DR. THORNE: Mine is just a quick one on volcanic ash.

VICE CHAIRMAN RYAN: Please, yes.

DR. THORNE: The one thing that we missed was the discussion of solubility. Radionuclides will be incorporated in the ash if that event occurs. I wondered whether any consideration had been given to dissolution studies in simulated lung fluid for volcanic ash because I think that might lower the range of uncertainty very rapidly on the solubility issue?

MR. LAPLANTE: Sounds like a good idea.

VICE CHAIRMAN RYAN: Great idea. But where do we get the analog?

DR. THORNE: I'm suggesting you do it on natural ash and look at staple trace elements in natural ash leached out into lung fluid.

VICE CHAIRMAN RYAN: It's a possibility. But again, you have the same is that a valid question to at least wrestle through? Our next presentation is from Ms. Cheryl Trottier, the branch chief of the Radiation Protection Environmental Risk and Waste Management Office of Nuclear Regulatory Research. Good morning.

MR. TROTTIER: Good morning. I know the request was for a perspective from us. What I was hoping to do today was give you a little bit of information, especially for the working group who probably has no idea what we do in the Office of Research at least to support Yucca Mountain, to give you a little idea of what our research program is like.

Basically within this branch, you can tell by the name, we have a variety of disciplines. We look at health effects, research, radiation protection, methodologies, et cetera, and also issues related to ways mostly involving dosimetry transport issues like that. Basically what we do is generic research. What that means is we don't directly

support Yucca Mountain.

We do research at least in this area of the environmental issues mostly for decommissioning sites. But because a lot of these topics, and a lot of the topics you are talking about at the meeting, involve other kinds of agency decisions, that kind of research is very effective for multiple situations; waste disposal or decommissioning.

At least actually at the advice of this committee, we developed a research plan which Ill advertise a little bit. This is the published version of it. It's actually on the NRC website. Because we had a very small program. We were always told how do you know you're doing the right research and you need to have some disciplined process.

So several years ago, we did develop a research plan. We had a lot of stakeholder involvement. We eventually had it peer reviewed. As a result of all of this activity, we then prioritized our research projects. Again, it's only in this area which I will call, even though a lot of staff disagree with this title, radionuclide transport in the environment. It's not just transport. It's the whole issue of environmental contamination.

So what I tried to focus on for this

particular session is that work that deals with biosphere modeling. A lot of work we do does address transport. I think you're going to have a separate session. We can come and speak to you then about some of our activities in that area.

In fact, prior to the research plan, we really hadn't done anything in this area, at least not in the time frame that I have been with this group. The work we're doing is with PNNL. It was recently initiated actually September 2002. So this is very new work. We have set up certain objectives that we're trying to address.

One of those is we have observed that a lot of the models have parameters that either have uncertainty, the data is very old. The idea was that we would try to do an assessment of those parameters and see where we might be able to inform the modeling by attempting to address some of these uncertainty issues. As I said, our overall budget is very small. As you can guess, this is an enormous project.

So we began with a literature survey. Out of that literature survey, that helped us to then narrow down the field of things that we were going to look at. This list is basically those lists of activities that we hope we can address in the next two

years. This is basically a four year project, hopefully, to be completed in four years.

We're going to focus on a few parameters that we think we can have some hope of getting accomplished in a fairly short amount of time. The one area on the animal product transfer coefficients, that may be more difficult. When we get to the next slide, I'll talk a little bit about that. We have already begun the process of looking at soil. That will take up a fair amount of time over the next year or two.

One of the things that we're really trying to do here is work with the international community. I know several of you have talked about that. There are a lot of studies ongoing. The principal investigator for this project has been working with those who are involved in that. We're hoping actually to be able to make use of some of the studies that are going on in the former Soviet Union as a database of trying to inform these parameter studies.

As a result of the literature review, these are radionuclides that we decided to focus on for this effort. As you can tell from the topics that you are talking about, they are in fact radionuclides that are important in this assessment of the Yucca

Mountain impact. The plants that we're proposing to look at are as listed up there.

You can see a note next to the trees under discussion. We're still in the process of discussing with PNNL their feasibility of looking into these larger crops. The time frame is an issue when you have larger crops. So we haven't firmed up the research plan for looking at the trees.

The same with animals. In the area of the large animals, there's a lot of work going on right now with cows. So we will be looking into that. Again, the small animals would be handled within the U.S. So that's again an issue that's under discussion.

For now, the sampling locations have been settled as being in the State of Washington which I believe is actually near the Hanford site. In Nevada, it is the Amargosa Valley. South Carolina, I forget the town, but it is near the Barnwell site. The concept was to pick sites with different degrees of being arid and semi-arid, et cetera, not to pick all from the same type. It would be nice to add a couple more. But I don't know whether we're going to be able to do that.

VICE CHAIRMAN RYAN: Just a quick question

if I may.

MR. TROTTIER: Sure.

VICE CHAIRMAN RYAN: We've heard a lot about americium in our working group in the last couple of days. Was that off the list for a reason? Or is it bracketed by what you have there?

MR. TROTTIER: That's a good question. I'm going to ask Phil Reed who is the project manager.

VICE CHAIRMAN RYAN: Thank you.

MR. REED: Yes, Phil Reed. We had actually considered both the americium 241 and the other long-lived isotope. But our focus here was strictly on the groundwater irrigation pathway and not in the volcanic scenario so we did not put it on our top five priority. We actually have it in our top six.

VICE CHAIRMAN RYAN: Okay.

MR. REED: If funding does become available and if we switch to the volcanic scenario, we will certainly look at americium 241.

MR. TROTTIER: Just as an opportunity to remind you, again, our research is generic. So obviously igneous activity is not a research topic for us. I guess we can move to the next slide. As I said, we have so far to this point published a

literature review. This is the literature review. It is available on the NRC website.

Now, we have probably a few copies still available. In our new electronic age, everybody wants to look at everything on the web. It's much easier to look at a book as a book. So it has a lot of information in it. I really think the lab did a very good job for this first step. As I said, this is very early in the process. So unfortunately, I don't have a lot of results to give you.

I would like to turn to slide nine please. I don't want you to take any great stock in these particular numbers because the QA on them is not 100 percent at this point. But the project manager did this simply as a way to illustrate part of the issues here. These are default values apparently in use for various codes. As you can see, they are all over the place.

And that is an issue. You do want to have some understanding as to what causes these to be different. Hopefully when we get done with this work we will be able to have a better understanding of what values we should be using for these transfer factors. The next slide is very similar. The first one is technetium. The second one is iodine. I realize the

numbers are very small for those in the back.

This is not a log scale. The other one was a log scale. So these numbers are not as far apart as they appear to be when you look at them in this bar chart style. Nonetheless, it does show that there is still a lot of variation among the codes that are in use today. With that, I think I'll quit.

VICE CHAIRMAN RYAN: Cheryl, just a quick question. I'm reminded of Dr. Thorne's comment about the context of a model and making sure that you go back to the fundamentals and the literature which you have done. Could you react to his observation there? Do you think that's on track and you are on track with it?

MR. TROTTIER: Well, yes, I agree. I think you have to look at the fundamental.

VICE CHAIRMAN RYAN: Thank you. Other questions? David.

DR. KOCHER: I'll probably be completely wrong again. Technetium, based on a very weak memory, has been confounded by issues of potted plant studies versus field studies. Help me, Michael. The potted plant studies are now viewed to have limited reliability. Those are the ones that give these humongous values.

DR. THORNE: Right, yes.

DR. KOCHER: So maybe at a minimum when you do literature reviews like this, if you haven't already identified how the study was done and that kind of dichotomy, it might be really helpful.

MR. TROTTIER: Right, I remember that issue being there, yes.

DR. KOCHER: I think technetium is a problem in a lot of these codes.

VICE CHAIRMAN RYAN: Ruth.

MEMBER WEINER: I look at your two slides and I zero in on the codes I know something about and forget the others. I know for example in MACCS2 there were really only two or three radionuclides for which the ingestion pathway was modeled and everything else was done by analogy. So I would encourage you, if you are in the process of recommending a model, to look very carefully at what they actually did to get those numbers.

MR. TROTTIER: Right, in fact, MACCS does fall within my branch also. MACCS is undergoing major revision at this point. It needs to be improved a lot. That's one factor.

DR. MOELLER: What is the name of the project leader at PNNL?

MR. TROTTIER: Bruce Napier.

DR. MOELLER: Thank you.

MR. TROTTIER: That's on one of the back up slides.

DR. THORNE: Can I come back and take up Dave's point? There is this difference between the potted plant. I believe already the principal investigator on this study has been in discussion with my colleague George Shaw at Imperial College.

MR. TROTTIER: Yes.

DR. THORNE: We've conducted over about the last 10 years comparisons between lacimeter (PH) and column studies. We got to the stage at least for chlorine, iodine, and to some extent technetium of being able to relate the parameter values of the models at the lacimeter (PH) scale and at the column scale.

But you can't simply assume that the transfer factor of one is the other. You need to go through some sort of modeling exercise to see which parameters are changed in a pot bound experiment relative to a lacimeter (PH) experience because the hydrology changes and the root density profile changes. It's those sorts of things that affect the uptake.

DR. KOCHER: There have been limited field studies for technetium.

MR. TROTTIER: Right, I understand that.

VICE CHAIRMAN RYAN: Cheryl, you mentioned studies in Russia. Could you expand a little bit on what you are bringing from those studies?

MR. TROTTIER: I'm going to have Phil do that because you might get half of my brain working and half not working.

VICE CHAIRMAN RYAN: Okay.

MR. REED: Phil Reed again. These are some studies that we're discussing through DOE with their agreement with the former Soviet Union countries. Apparently they have a lot of contaminated soils and contaminated lands where the United States does not. We would be interested in using those particular actual lands and field studies to use for our particular studies.

Also the fact that the cost is now becoming more involved and it's almost getting prohibited to do some of these animal studies particularly with some of the radionuclides that we're interested in. So we have talked with DOE about the possibility of using their, I forget what their state department agreement is with the former Soviet Union

countries, to make that data available to us and in the process do some coordinated field studies that so far have been pretty difficult to do in the United States.

VICE CHAIRMAN RYAN: Thank you. That's interesting. Other questions or comments?

DR. THORNE: One last one on arid zones. I think one of the things that we saw when we were looking at Chlorine 36 is a very strong correlation on plant uptake with soil moisture stress. Basically there was much greater uptake in arid conditions than there was in temperate conditions. I think this is a caution about applicability of the general literature to the arid zone region.

MR. TROTTIER: Right, yes.

DR. THORNE: But it's also an indication which I know you are aware of in formulating these experiments. I would strongly suggest that hydrological monitoring of the system is pretty fundamental to any new studies which is interesting because the bulk of the literature over the last 50 of 60 years, when you go to the papers, you will be hard-pressed to find any information at all on the hydrological status of either the lacimetery (PH) studies or of the pot studies. That is a real

problem.

VICE CHAIRMAN RYAN: Thank you.

MR. TROTTIER: Good point.

VICE CHAIRMAN RYAN: We are at a break point in the schedule. We are scheduled for a 15 minute break. Cheryl, thank you very much.

MR. TROTTIER: All right, thank you very much.

VICE CHAIRMAN RYAN: We appreciate it. It sounds like interesting work ahead. We will reconvene at 10:25 a.m. please. Off the record.

(Whereupon, the foregoing matter went off the record at 10:06 a.m. and went back on the record at 10:24 a.m.)

VICE CHAIRMAN RYAN: If we could come to order, please.

We have an additional speaker this morning, Matthew Kozak from Monitor Scientific, and his co-authors are Graham Smith and John Kessler from EPRI, Graham Smith being from Enviros.

So, Matt?

MR. KOZAK: Thanks, Mike.

I appreciate the opportunity to come and speak to you today. I'm here representing the EPRI team that conducts performance assessments on Yucca

Mountain independently from DOE or NRC. And you should know that EPRI has been conducting and is maintaining the capability of conducting independent performance assessments for 14 years on Yucca Mountain.

And my purpose here today is, first, to bring you up to date on what EPRI has done in the past and is doing now in the area of biosphere. It's been a very active program. And then I'm going to make a few off-the-cuff remarks about some of the things that I've been hearing said here at this meeting, if I may.

And so before I begin, I would like to explain the mishmash of organizations you see up there. EPRI is the organization that is sponsoring the TSPA work. Over the past year, Monitor Scientific has taken over the prime responsibility for the TSPA itself. And one of our subcontractors is Enviros out of the UK, and the principal investigator there is Graham Smith.

So, in fact, I'm really presenting a lot of the material that is Graham's work, but it was cheaper for me to come than for him to come over. Or, actually, he wanted to come, but he couldn't.

So I want to make it clear that this is primarily other people's work, but it's integrated in

the TSPA.

Could I have the next one, please?

And, really, the main thing that I'd like to do is to get across the idea that it has been a very active program, and that there are a lot of publications that we produced on this subject of biosphere. We started in about 1996, or 1995 was when the work actually originated -- and the first publications coming out in '96 -- and a lot of the early work on trying to establish critical groups, and so forth.

This is back in the days when the National Academy report first came out, and so there were a lot of people trying to figure out what to make of this. And EPRI really had a pretty strong role in helping to identify key concepts that maybe should be considered coming out of the NAS report. Next one, please.

At the same time that we were developing sort of an independent capability of doing biosphere, EPRI has been going along producing a large number of TSPAs over the year, about one every two years -- an update to the TSPA. And since '96, the biosphere has been an integral part of the EPRI TSPA.

And here are the four most recent that actually incorporate something about biosphere.

Previous to that they were Part 191-type analyses, and so there wasn't any emphasis on biosphere.

And for those of you who are following the EPRI program, you should know that we have just completed the most recent TSPA analysis. It's in press. It went to publication in December, and so it should be out on the street shortly. And that particular report contains a significant update to the EPRI biosphere portion.

In addition to sort of the things that we tend to see in the States, the EPRI program has been an active participant in these international programs, BIOMOVS and then BIOMASS. And, really, one of the key areas particularly early on was, again, looking at some of these things on how to define critical groups and the segue into the RMEI, and so forth, and a lot of the discussions that went on related to that.

But the group that dealt with a lot of those issues was actually chaired by John Kessler from EPRI, and so some of these other reports that are -- that have been published by the IAEA from the BIOMASS program had a strong contribution for the EPRI program as well.

Here is sort of a sampling of additional publications that you may or may not be aware of.

These are sort of more recent publications to the original ones that -- there was a large spate back in '96, and more recently Graham and his co-workers have been publishing again on a number of issues related to Yucca Mountain.

So just to summarize, it is a very active program. I would urge you to take a look at some of the publications. They're good publications. There's a lot of good information in them. It is entirely independent from the DOE/NRC world, and so it provides an independent viewpoint on a lot of technical issues.

We've done a lot of work on trying to incorporate international developments into our program. In fact, I've had to argue frequently with Graham that he really does have to go back and use Federal Guidance Report 11 instead of more recent dosimetry, but that's a whole different matter. If you go to the EPRI TSPAs, you'll see how we've integrated that into -- into our TSPAs.

And beginning my segue into my comments on what I've heard here, we've been using deterministic biosphere dose conversion factors as a stand-alone calculation at the end of the TSPA. And in the coming year, we're planning on starting to work into doing some Monte Carlo sensitivity analysis on the

parameters associated with that.

And as an independent group, we have come up with the conclusion, based on analysis, that we can use this approach, that the dynamics of the system are such that the response of the biosphere is much more rapid than the response of the geosphere. And that's one part of the argument that suggests that you can use this approach.

Another part is the decoupling of the geosphere parameters, such as the Kd approach that Dave was talking about earlier, from surface soil -- Kd's that are used in agricultural soils or properties that are in agricultural soils, as opposed to the alluvium, the deep alluvium. There is not any particular reason to couple those.

And so we've come up with this independent -- independently from the DOE/NRC kind of approach. Modeling the dynamics of the system, we've been able to demonstrate that, at least based on our understanding of the system, that this is -- this is an appropriate approach to use.

The second point I wanted to mention in the discussions that we've heard here is -- we heard a little bit about Greenhouse gas warming effect and how it plays a role in the TSPA. And you should be

aware that EPRI, in the past, has had explicitly Greenhouse gas warming kinds of scenarios in their TSPA, and were aware that DOE and NRC have considered this also.

And up until about two years ago, it was an active part of the EPRI program, and then at that point we stopped looking at it, because it didn't matter. We found that it had inconsequential effects on the total system performance. And so while we understand that these things are out there and that this is a potential effect, that it no longer shows up as an explicit part of our TSPA.

The second thing that I wanted to address, which may end up being more controversial than the other things I've said, is we've had a lot of discussion about these ancillary analyses that we would do on the side, because people want to see them.

And I think we want to be careful about this, because we are on a licensing path. And there are uncertainties that are associated with making that regulatory decision, and I think there are separate uncertainties that are associated with scientific evidence.

And it's sort of this argument that we had yesterday that we may have scientific uncertainties,

and they could be substantial, but if the uncertainties are all down at extremely low dose levels, we don't care from a licensing perspective. And I think we need to keep that clearly in mind.

We don't want to start sending DOE marching orders that they need to start doing all kinds of scientific studies, if it's going to affect the licensing path. I mean, I think that's a very important point that we need to keep clearly in mind. Which are the uncertainties associated with the regulatory process, which I'll call regulatory uncertainties?

Those have a different flavor from the scientific uncertainties. There can be a lot of scientific uncertainties, but they may not affect the regulatory decision. And so I think we need to really keep that clearly in mind.

And that's all the comments I wanted to make. I'll keep it short and sweet. Be glad to take any questions.

VICE CHAIRMAN RYAN: Thank you, Matt.

Any questions? John?

DR. TILL: Yes. I'm just curious about this Greenhouse effect. If you have looked at that, is that published in the literature, so then --

MR. KOZAK: Yes.

DR. TILL: It is? So then this can be dismissed as an issue.

MR. KOZAK: Well, I think this -- this crept into the argument as -- as another one of these scientific things that people want to see that you've considered it. I think if you look at the full body of literature, if you look at the DOE FEP analysis, for instance, they may ultimately -- at the end of the day in their TSPA, they say, "We can use paleo climate."

But if you look at their FEP analysis, I think you'll find that, yes, they recognize that the Greenhouse effect occurs. They've done studies of it, and they've essentially dismissed it. And that's essentially what we've done. And to a large extent it's based on the properties of the Yucca Mountain system and how it would behave under the Greenhouse situation. There's a slightly elevated rainfall, but it's not -- it's not a drastic effect. It's not like a coastal site where you have rises in the sea level, falls in the -- falling sea level.

DR. TILL: Well, that doesn't exactly answer the question. I mean --

MR. KOZAK: Yes.

DR. TILL: -- Michael had raised it as an issue, and at least the question -- I guess he said he was surprised this had not been included in the DOE analysis.

MR. KOZAK: Right.

DR. TILL: Okay? So, I mean, if indeed it has been considered --

MR. KOZAK: Yes.

DR. TILL: -- and considered carefully, the way you get it off the table is make sure that it's clearly documented somewhere --

MR. KOZAK: Yes.

DR. TILL: -- in the literature.

MR. KOZAK: Yes.

DR. TILL: The answer, you're saying, is that it is. And it is --

MR. KOZAK: I believe it is, yes. That would be my response is, yes, I believe it is well documented that that does not have a significant effect on the system.

VICE CHAIRMAN RYAN: Do you have some specific references, Matt, that you could maybe point us to? I don't want to try and pick your memory while you stand there, but if you could think about --

MR. KOZAK: Well, I can speak to the EPRI

documentation.

VICE CHAIRMAN RYAN: Yes, that's what I'm asking.

MR. KOZAK: Yes. In the EPRI documentation, the '96 and the '98 versions of the TSPA, although it could be as late as the 2002 TSPA -- I know within that range is when we decided to stop spending significant effort on it, because our results showed that there was no real effect.

VICE CHAIRMAN RYAN: And these are on the list that you've given us.

MR. KOZAK: Yes.

VICE CHAIRMAN RYAN: Okay.

MR. KOZAK: Yes. Those would be the TSPA documents that are on there.

VICE CHAIRMAN RYAN: Okay. I just want to --

MR. KOZAK: Yes.

VICE CHAIRMAN RYAN: -- be clear about where it was.

Dave?

DR. KOCHER: I want to understand your comment about the dynamics of the biosphere system. I gather what you're driving at there is you -- that you think equilibrium-type models are appropriate. Or

do you have in mind a certain time scale that's short when you think about things like that?

MR. KOZAK: Yes. When we derived the biosphere dose conversion factors, we used a dynamic model and reach an approach to steady-state. I won't say it's equilibrium or -- what was the word yesterday? Saturation. It's a steady-state -- it reaches a steady-state. And when it reaches, you can model it out until it -- you can do the calculation, the dynamics, until it reaches some approach to that steady-state, and then you say, "I'm done."

And you look at how long that takes, and it's not that long in the analyses that we've done. It's not -- it's not thousands of years. It's not -- and if you look at the rate of change of the plumes coming from Yucca Mountain, if you remember the ones on the TSPA SR yesterday, that rapid rise that everyone was talking about, that's on a log scale out in the hundred thousand to million decades.

That's actually a very slow rise, and so it's basically a stationary -- you can think of it as a series of stationary steady-states that the biosphere has a chance to respond to.

DR. KOCHER: And I assume that the time to steady-state or some approximation of it is pretty

much driven by how you model retention in soil?

MR. KOZAK: Yes. Yes, the surface soil compartment is what drives the approach to steady-state. That's correct.

DR. KOCHER: So you would think order of a few thousand years and less is -- is basically nothing on the time scale we're talking about here?

MR. KOZAK: I think a few thousand years -- I don't think we've seen anything that's a few thousand years.

DR. KOCHER: And it's all in the assumptions, of course.

MR. KOZAK: Yes. On the order of a thousand years would almost be constant concentration on these scales.

DR. KOCHER: Okay. Great. Thank you.

MEMBER WEINER: Since you mentioned the cumulative distribution functions, the TSPA results that Dr. Swift showed yesterday --

MR. KOZAK: Yes.

MEMBER WEINER: -- do you have -- could you point out for me any significantly different results that your independent TSPA showed? Or just summarize them qualitatively?

MR. KOZAK: Yes. Our results show

something quite similar. The results that we just came out with, our most recent ones which I can speak to, since I was most intimately involved with those, show a very similar type of behavior. The exact numbers may be slightly different, but the key points are that it's well below the regulatory limit in -- within the regulatory time period, and it doesn't rise to incredibly high numbers after that.

I mean, if you looked at those TSPA SR numbers yesterday, keep in mind that the highest peak, way out at a million years, is below the public dose limit. It was below 100 millirems. So it's not -- they're not astronomical doses, even though on that scale it looked like they were going way up. They're not -- they're not really high.

MEMBER WEINER: I'm more interested --

MR. KOZAK: And we find something quite similar.

MEMBER WEINER: Oh, you have that --

MR. KOZAK: Yes.

MEMBER WEINER: Are there any input parameters where you differ markedly from DOE? I'm interested in the -- since yours is an independent TSPA, independent of both DOE and NRC --

MR. KOZAK: Yes.

MEMBER WEINER: -- I'm really interested in what -- if you could highlight the differences.

MR. KOZAK: Okay.

MEMBER WEINER: And not so much, you know, that it's a low dose or a high dose, but just --

MR. KOZAK: Sure.

MEMBER WEINER: -- what the differences are.

MR. KOZAK: We are a considerably smaller program than DOE or NRC.

MEMBER WEINER: Yes.

MR. KOZAK: And as a result, we rely fairly heavily on their breakdown of the raw information. Based on that, we come up with an independent evaluation of whether or not that's reasonable or if their are conceptual models represent what we consider to be the best available science kind of approach, and then we come up with an independent approach.

So if we immediately go to parameters, we've got to be careful, because our models are different. We've got a totally different modeling structure. And within that, then there's also independent estimates of the parameters.

And so what we do is we have people on

subcontract who are really high-level people. We have Ed Sudicky from University of Waterloo doing the groundwater modeling; Frank Schwartz from Ohio State doing some of the data interpretation for the groundwater hydrology. So these are very well-known top-level people. Graham Smith is well-known for his biosphere work.

And so we rely on those people to come up with -- by evaluating the information that both DOE and NRC come up with -- to come up with their own independent ideas. But primarily where we focus our attention is in the assumptions and the modeling to come up with independent models rather than focusing so much on the parameters. We do that, too, but that's probably not the crux of the difference between them.

So it's a hard question to answer is what I'm talking a long way around about is -- is we really have taken a totally different approach and come up with somewhat similar results, rather similar results I would say.

MEMBER WEINER: I guess what I was trying to get at was -- and maybe you can't answer the question that simply -- is some significant difference, either in model or in parameters or in

results somewhere, what -- are there any significant differences? And what are they?

MR. KOZAK: In the realm of the biosphere, there are -- I can't give you specifics off the top of my head. I'm sorry.

CHAIRMAN GARRICK: What do you see as the primary purpose of the EPRI TSPA? I know it's independent and --

MR. KOZAK: Yes.

CHAIRMAN GARRICK: -- and I know industry needs to have --

MR. KOZAK: Yes.

CHAIRMAN GARRICK: -- access to their own resources for getting an essence of what's going on. But what do you see as the primary purpose of this TSPA?

MR. KOZAK: I think it serves as a good in-depth review of both programs to make sure that --

CHAIRMAN GARRICK: How is it used in that context? What do you --

MR. KOZAK: Well, I mean, we publish our work and go to conferences, and so forth. And if a significantly different conceptual model -- for instance, let's say for some of the things we're talking about here -- significantly different

conceptual model for inhalation, for instance, I don't think we do, but that's an example.

If we were to have that, we'd go and present it, and we would try to get it on the table -- get it out and published, and the information out there soon enough so that it could be taken on by DOE, that they would get the benefit of our independent viewpoint. They could take it on if they felt they needed to, or that NRC would be able to take it on as they saw fit.

So that's really the role that we play is to be able to provide information as an independent evaluator of the system that might be useful to the regulatory process.

CHAIRMAN GARRICK: Now, you've been doing this for a long time. Do you -- can you point to areas where you think you've influenced --

MR. KOZAK: Oh, yes.

CHAIRMAN GARRICK: -- the DOE and the NRC models?

MR. KOZAK: Definitely. EPRI, in a number of cases that I could point to, started putting some of these things out first, and sort of drew interaction matrices. We were the first person -- we're the first group to produce an interaction matrix

for parts of the Yucca Mountain system, for instance, and that was one of the things that we saw here. That was back '96, I think.

So that's one approach that we brought from the international community and published it. Whether or not it was actually seeing our work that influenced DOE to start producing interaction matrices, or whether it was their participation in international programs, I can't say.

But there are a number of things along those lines. We've done different types of source term modeling, which is outside of the realm of this. But in our TSPA, our source term modeling has been significantly different from either NRC or DOE, and has led to some changes in the DOE modeling.

CHAIRMAN GARRICK: Yes. Now, just one final comment. I don't like decoupled models unless what's been decoupled doesn't make any difference.

MR. KOZAK: Right.

CHAIRMAN GARRICK: What's your comment about that, about your biosphere -- how coupled is your biosphere model to the geosphere model?

MR. KOZAK: In terms of intimate coupling that you need to have information from one compartment that's used in the next, there's very little. But the

point is is that we've gone through -- about it in a justified manner. We've said, "We recognize that these couplings can occur, and so we want to look at the dynamics of the system to justify that we can decouple them."

My belief is that probably even though we haven't seen that necessarily in this meeting, my belief is probably that DOE has done that, too.

CHAIRMAN GARRICK: Yes. Okay. Thank you.

VICE CHAIRMAN RYAN: Michael.

DR. THORNE: Could I ask a question of clarification? On the Greenhouse modeling, you mentioned that the precipitation was slightly increased in the Greenhouse --

MR. KOZAK: Yes.

DR. THORNE: How was the increase in precipitation quantified or limited for those calculations?

MR. KOZAK: I'm going back a couple of years, and I hesitate to misspeak. We had -- we had a professor of climatology on our team at the time who was going through the data and the modeling that were available at the time to come up with an independent estimate.

That independent estimate was consistent

with the types of effects that both DOE and NRC have seen from the Greenhouse gas effects in their interpretations of the data and modeling as well.

DR. THORNE: Okay. The reason I ask, for other people, is that GCM-type modeling, both transient and point-estimate type, or point-in-time estimate, have moved on a lot in the last sort of five or six years. But one of the things I'm struck with continuously is that while there is some broad agreement on temperature change in those models, the projections of precipitation change, even for areas like Northern Europe where it's rather constrained --

MR. KOZAK: Yes.

DR. THORNE: -- are quite often very variable. And one of the problems that I see with the Greenhouse gas business is that you may get a reasonably constrained envelope for the temperature changes from modeling exercises, but you don't get such a reasonably constrained envelope for the precipitation changes from those modeling exercises. And if you're in a non-analog situation, it's very difficult to use past data to constrain the precipitation regime.

MR. KOZAK: Yes. Although to some extent -- I was going to say to some extent there is -- some

of the information that I'm aware of, in terms of the paleo record, the correlations of CO2 records in icepacks to rainfall in arid regions, and so forth, I think is one of the significant bases. So you can draw a correlation between Greenhouse gases in the environment at a particular time and the paleo climate at that time.

So I think that there are -- and I'm stepping out of my -- my realm of particular expertise here, but I know that that's one of the threads of evidence that has been used.

DR. THORNE: Yes. And I think that's a legitimate argument, but I think you have to recognize that those CO2 levels are pre-Quaternary. So they're more than 1.6 million years ago. In fact, often quite a lot older.

MR. KOZAK: Yes.

DR. THORNE: And that a lot of other elements of the climate system, like the rise of the Tibetan Plateau, the drift of Antarctica, have also occurred over that period. So I'm -- the world was different then, and I'd be very cautious about using those as a strong thread of argument, though I think it -- basically, in this business we're looking for every bit of argument that we can get.

MR. KOZAK: Yes, absolutely.

VICE CHAIRMAN RYAN: We have a response here.

DR. SWIFT: Peter Swift, Bechtel SAIC and Sandia Labs. The Department of Energy has not attempted to use general circulation models directly to do forward modeling of climate for Yucca Mountain. The Department made a decision there that -- this would have been many years ago, but uncertainty in forward-looking climate models was just going to be very great and was not going to provide a credible basis for going forward.

Instead, we chose to look at paleo climate data -- a broad range of possible sources of information -- available sources of information about past climates in the region, and then to model -- conceptually model, not numerically model, forward climates with an assumption that future climates would repeat those of the past.

We're well aware that anthropogenic change may disrupt that assumption. It may create anthropogenic changes that would lead to future climate changes that do not follow patterns of the past.

With respect to the magnitude of those

changes, changes in -- there certainly is uncertainty about future precipitation and temperature. So we have uncertainty bands on our future states, so we have a -- and on the present state, too, for that matter. But the monsoonal climate comes in drier and wetter versions. So, too, is our glacial transition climate, and so, too, for that matter is our future flow glacial climate.

Those enter the geosphere modeling system through changes in the amount of infiltration entering the unsaturated zone. So we have low infiltration states and high infiltration states, and intermediate ones, for each of our future climate conditions.

So it's our belief that the uncertainty associated with anthropogenic changes in precipitation will still fall within the range of basically wet and dry infiltration states that we have for our future climate states.

Now, can we prove that? No, that -- this is a conceptual statement. We believe that the anthropogenic effects will not take us out of the range of uncertainty already included in our models.

There has been quite a lot of consideration given to that. That's the best I can do for an answer.

MR. KOZAK: Yes, that's -- thank you.

VICE CHAIRMAN RYAN: Any last questions? Yes.

MEMBER CLARKE: Just a follow up to Ruth's question that came out in response to another question -- that you're handling the source term a little differently. How about transport in unsaturated zone, VADOS zone --

MR. KOZAK: Yes.

MEMBER CLARKE: -- saturated zone, dimensionality -- any major differences in the two models that we're hearing --

MR. KOZAK: The short answer is yes, there are differences in all those aspects. I guess to borrow Dave's comment, we don't want to get up in the weeds on things that are outside of the biosphere. But my understanding is that other people from our group will be addressing the ACNW in future meetings, and certainly at that point -- in fact, I think Frank Schwartz is supposed to be talking at one of the upcoming ones. And he would certainly be the one to address a lot of the conceptual model stuff on the geosphere.

MEMBER CLARKE: Thank you.

MR. KOZAK: He's outstanding.

VICE CHAIRMAN RYAN: Thank you. Oh, yes. I'm sorry. Dade?

DR. MOELLER: I don't know if it would be proper, but could we ask Dr. Swift or Dr. Wasiolek what impact the EPRI work has had on DOE?

VICE CHAIRMAN RYAN: Sure. Please be my guest.

DR. WASIOLEK: Basically, what is going on here is that Graham Smith, who is the primary author or one of the primary authors of biosphere models for the EPRI work, is very heavily involved in what's going on in the European community in all programs that are international programs that are -- like most currently completed BIOMASS program, and there are several programs that are going on now, like BIOPROTA or BIOCLIM, and there is a whole variety of programs that looks at various aspects of biosphere modeling.

And we are familiar with the programs. We are familiar with BIOMASS and biosphere model developed for BIOMASS or in -- in this effort is one of the models that we compare our model with in the model validation. So we just take the very same model that was used for EPRI, and not because it was used for -- in the EPRI evaluation, but because it is the most current European model, which just happened to be

used by EPRI.

So it's a sort of coincidental correlation here, I would say, plus we are -- we are not on the uninhabited island. We've participated in international effort. We've participated in BIOPROTA. I am a task leader on one of the BIOPROTA -- I mean, Mike contributes very heavily to -- I mean, he is one of our primary contributors, and we really appreciate this, because we are getting input from the whole international community.

I am a member of IUR, and so we're just trying to stay on top with the current development of biosphere models, and so does EPRI. So this is where the -- where the commonalities come in place, and not because of the association with particular institutions. It's just the -- that we are all trying to stay abreast with the current development in the discipline.

DR. MOELLER: Thank you. I'm glad I asked.

(Laughter.)

This is a terrific answer.

VICE CHAIRMAN RYAN: Thank you. Any other last questions? We've had a request for a couple of additional speakers during this time, so -- Steve

Frishman I think wanted to speak. Yes?

MR. FRISHMAN: I just wanted to sort of follow up the presentation before you have your further extended discussion by pointing out that I think it's important to sort of go back to before the beginning of this whole discussion. And if you have it handy, look at page 8 of Peter Swift's first presentation yesterday. It's the false color IR photograph of the region.

The discussion for the last day and a half has, to not coin a phrase, been in the box. Now, the system doesn't end at the end of the blue flow paths shown on this map or on this photo. So if you go sort of back to basics, when you're talking about contaminants being released into the environment, very quickly you get to questions of what are the -- what is the fate of those contaminants?

And this discussion, as has the biosphere model, both used by DOE and NRC, doesn't ask that question. Well, we're in a situation where we know in general terms the fate of those radionuclides that are transported out of the repository, and that fate is that they come back to the biosphere, just outside of this box if they're not captured by a well.

And we know that we are in a closed basin.

We know the bottom of that basin, the bottom of the gradient, which is Franklin Lake Playa, which is an area south of the box, and just the very beginnings of the white area due south of the box on the photograph. This is an area that is an evaporative lake. When there's a lot of water in the system from heavy rains, it's a lake. It flooded one time this summer and washed out a road across it.

Now, it's dry most of the time. It generates a lot of dust. The water is -- when it's not a lake, the water is very close to the surface. If you try to walk across it, you sink in the mud, if you break through the salt crust on the surface. So it's an integral part of the system, and it is the -- the primary sink under current climate conditions for the radionuclides escaping from Yucca Mountain.

Now, what happens if we have different climate conditions? The extreme that we know of is one that, at least according to the modelers, fits -- is bounded by the -- that the climate model that's used, and the extreme representation are spring deposits right at the foot of Yucca Mountain.

Those spring deposits right now are on the order of 100 meters above the water table, and the age on those spring deposits, the youngest that I know if,

is on the order of between 8,500 and 9,000 years. So we have -- we have one extreme which is we're very near the edge or very near that extreme, which is the dry condition right now, and we know where the sink is.

And we have another at least extreme from the record, which is a water table 100 meters higher and springs flowing out, so, therefore, Amargosa Valley being essentially a large area of standing water, and water that the water table is constantly feeding.

Well, the biosphere model takes only the current condition of pumping. And I think that it's not out of the question that we have to, at this point, say that the biosphere model is limited and actually artificially truncated, both in space and time, because it doesn't deal with the -- the sink of those radionuclides that are right now thrown out if they get below the root zone.

They not only, under current conditions, probably do come back up, but they also are in a condition to where they can, with a little bit more moisture, however you break that balance, they can go back down to the water table and be redeposited out in the Franklin Lake Playa area, picked up by the wind,

and moved right back into that same biosphere where you're trying to do your measurements.

So I think when you talk about conceptual model, the conceptual model is an incomplete conceptual model at this point, not -- not necessarily wrong but I think clearly incomplete. And it also does not take into consideration the -- even the 10,000-year time period, where it's possible that we would not get a very rapid rise in the water table to the point of springs at the location that we see this one near the foot of Yucca Mountain.

But there are also indications of other small spring deposits farther out in Amargosa Valley at a lower elevation. So I think it's -- it's fine to discuss the biosphere under the current condition, but it has to be looked at under other conditions as well.

And I guess I've sort of not always, because it hasn't gone on always, but I've been concerned about the regulatory framework and how it is applied into this system, because the regulatory framework sort of makes you do it wrong. And in this case, it makes you take everything out of the well, and it's I think in part because the EPA rule sort of drives you to the current human condition, and the current human condition is that you're going to take

the water out of the well.

But if you're looking for realism, the realism is that not all of the water goes up the well as the regulation requires, but whatever doesn't -- whatever radionuclides don't go up the well come out in Franklin Lake Playa to be broadly dispersed in the environment.

So this brings to at least one more point that needs to be made -- and the Europeans are much more conscious of it than the people in this country -- and that's that when you're talking biosphere you're not only talking dose to humans, you're talking about radionuclides in the environment.

And that's also apparently left out of this whole discussion, whereas in Europe it's becoming more and more common to be brought into the discussion. And the regulation I think is negligent in that area, at least on EPA's side, and it's -- some people maybe as cynical as I figure we'll get another bite at the EPA rule pretty soon. And I think that's one area that we're going to be exploring, along with many others.

So, but the biosphere discussion right now I think is artificially truncated in space and time and also in scope. And I'll leave that with you for

your future -- for your discussion.

MR. COLEMAN: Steve, would you -- this is Neil Coleman, ACNW staff. Would you identify the spring deposits that you mentioned, where they are?

MR. FRISHMAN: Well, there's one where if you look at the blue flow lines, right at the foot of Yucca Mountain you see a white line that is another drainage. It's the first one that goes off to the southwest. It goes all the way down to the boundary.

Then just to the left of there is a red square or a red cross indicating a well. That's a well that Nye County put down in the area of that spring deposit. And then there's another one sort of on the other side of the hill just north of it, on the other side of the hill from there.

MR. COLEMAN: I believe these are the ones referred to as the Lathrop --

MR. FRISHMAN: Yes.

MR. COLEMAN: -- along 95?

MR. FRISHMAN: Right.

MR. COLEMAN: Okay. I think you mentioned that this was at the foot of Yucca Mountain, these are 20 kilometers away, 12 miles, not quite at the foot of Yucca Mountain.

Also, the fact that you have spring

deposits there doesn't mean that you have -- you referred to standing water bodies. At one time it was thought that Las Vegas Valley was one big lake, and Marty Mifflin pointed out that these, in fact, were not lake deposits but spring deposits. You had, certainly, a lot lusher vegetation than you see today.

But I just wanted to clarify --

MR. FRISHMAN: I'm not suggesting that Amargosa Valley was one big lake. But at --

MR. COLEMAN: Well, you used the term "standing water bodies." I just wanted to specify this is 20 kilometers from Yucca Mountain, and these are paleo spring deposits.

MR. FRISHMAN: Right. And we do know that the water table, at its maximum, has been about 100 meters higher than it is right now. And it's no coincidence that these spring deposits are at about that same elevation. So I'm not suggesting that I know that it was one large lake at one time, because there are lots of factors that control whether it was. But there was certainly surface water in the area.

And if you go back to I think a 1982 panel from the National Academy that was chaired by Tom Pickford, one of the things that they discussed about the Yucca Mountain site, or a site like Yucca

Mountain, was concern for it as a repository, because at some future time people would have access to springs that could be contaminated due to releases from a repository. And that becomes sort of the Yucca Mountain picture.

And they -- they suggested in that report, if I recall, that sites like that should probably be -- not be looked at because of the potential future danger to -- to people given climate changes and water table changes.

Well, for your consideration.

VICE CHAIRMAN RYAN: Thank you.

Other questions or comments? Are there other comments from other speakers? Yes.

MS. TREICHEL: Judy Treichel, Nevada Nuclear Waste Task Force.

In the discussion, there are suggestions that studies be done on victims of Hiroshima and other -- perhaps Chernobyl -- where there has been radiological damage and exposures to people. And I found it very interesting when the suggestion was made that there should be a baseline study in the area of Yucca Mountain. And you will find a lot of people in communities there that really want that to happen, and they've never been able to get DOE to actually do

that.

And I know that there's at least one group down around Shoshone and Tekopa and Death Valley Junction where they've tried to find their own money and find free -- free help in doing that with epidemiological studies, and so forth, and they've not -- I don't -- as far as I know, they haven't been able to get it on.

But it would be interesting to go from the reverse in this case and find out -- what you would find out is that, by and large, the people are well. And how come they're well? Why is this a good place to live? And why would it be justified to create a risk or to create the possibility that they would get sick?

And you've got Amargosa Valley, which as a place to live is also very attractive, and certainly more rain would make it even more attractive. But Nevada and Nye County and Clark County are one of the fastest-growing areas in the nation, and that's probably why.

And the Amargosa Valley region is one of the few places in this country where the land is affordable, and the opportunity is there that if you wanted to be a subsistence farmer, if you just wanted

to go out, have a bunch of children, live off the land so to speak, you could really do that.

And so that's why I think people out there and people like me and others who care really -- really get sort of disgusted at some of the discussion that goes on about the contamination and what it would be and how it would work, and the idea that, well, maybe if you put in water softeners that would be a big help.

They don't need water softeners. They have very good water. And even if you put water softeners in, you wouldn't be irrigating with soft water. You wouldn't -- you don't even drink soft water.

But -- and the idea that you would ever pump the aquifer dry -- that doesn't happen. If you have wars now in the west, it's wars over water. And we don't allow aquifers to be pumped dry, and that's why the state engineer is sort of the sheriff in Nevada these days.

So I just don't feel that a lot of it is justified, and it certainly would do somebody some good to check and see why those people are as healthy as they are now and why Amargosa Valley is as attractive as it is.

Thank you.

VICE CHAIRMAN RYAN: Thank you very much.

Any additional comments?

I think the schedule that we have is we're probably at a break where we can break for lunch, Mr. Chairman.

CHAIRMAN GARRICK: Sure.

VICE CHAIRMAN RYAN: And I think what our -- let's just take a couple of minutes and think about the rest of our working group session. I'd like to come back after lunch -- we've had I think an excellent start this morning on summary comments.

I would like to do that again, based on this morning's presentations and any other comment you might like to make about the working group session overall, and then have further discussion with ACNW members and any comments from other participants, and spend from 1:00 to perhaps 2:00 or 2:30 with that discussion, and then have an additional period for public comments. And then we'll have a close of the working group session.

And keep in mind that I think we can combine -- the last item on our published agenda is to think about a letter that the ACNW will generate, and I think what I'd like to suggest is that we pick up

points for that letter in our discussion earlier, so we're not repeating the same points over again.

So we'll try and combine those two things, and then I think aim roughly at adjourning somewhere around the 3:00 time or so with the working group session to give people a little bit of advance for planning the rest of their day.

Does that sound reasonable, Mr. Chairman?

CHAIRMAN GARRICK: Yes.

VICE CHAIRMAN RYAN: Okay. We'll stand adjourned until 1:00.

(Whereupon, at 11:14 a.m., the proceedings in the foregoing matter went off the record and resumed at 1:01 p.m.)

VICE CHAIRMAN RYAN: Okay, thank you very much for your attention. This is our last session for this working group roundtable on biosphere issues and modeling.

I think what I'd like to do now is have each of the expert panel members offer their kind of summary and closing comments, being careful not to repeat too much of what was said in our summary this morning from yesterday, but maybe focusing on today's issues and then some of the global items and comments that you might want to talk to us about.

I think I'll then go starting my far left at the table and coming back this way, getting the consultant and member comments and then we'll bring our workshop to a close at that point. And that will give us, I think, an excellent review for the preparation of a letter which we might do.

I do not plan to have a separate letter-writing discussion because I think this will actually serve both purposes to both summarize and to give us specific things to think about as we then move into a letter-writing phase, perhaps a little later on. So that will be two separate activities.

Let me turn the meeting back over to Dr. Moeller for a review from the expert panel members.

DR. MOELLER: Okay, we'll go the opposite direction.

Dr. Thorne, would you begin, please?

DR. THORNE: Yes, I think there is not a lot that I want to add to what -- the remarks I made this morning. I think I'm still bemused a bit by this business of climate change. We heard that it had been studied in the program and I'm sure that's right, but there hasn't been a model underpinning of future climate. As I said yesterday, future climate is very much a new analog situation for the paleoclimate data,

so I find myself a little stuck. There hasn't been a modeling study looking at greenhouse warning, no GCM simulations undertaken. And then a statement you rely on the paleodata, but the paleodata doesn't apply to the new analog situation, so I'm still in this uncertain feeling about how you bound temperature and precipitation data for the future if you don't really on models and if you can't legitimately rely on paleodata. And there's a big question there about the adequacy of the models, but either you accept some sort of modeling projection of future climate or you have no way of specifying a bound on future climate, except sort of physical plausibility arguments that say something like I don't think it's going to turn into the Himalayas.

I'm struggling as to how DOE can provide a bounding argument for future climate change that allows them to eliminate it formally from the rest of the assessment, if that's what they're trying to achieve.

DR. McCARTIN: The regulation does limit the climate to arid to semi-arid, so there is some bound by regulation that can't go to say a tropical jungle certainly, but arid, semi-arid is provided as a limit.

DR. THORNE: Right, so I think that might be the point to build on and the question will be an agreement on how far you go before a climate stops being defined as semi-arid and that's perhaps the point for discussion.

DR. MOELLER: Any other comments?

DR. THORNE: No, that was the main one that arose, I think

DR. MOELLER: The regulations also say and Tim McCartin can undoubtedly help us, that you can only use something -- I'm paraphrasing. You can only use information that's on the table up to the day the license application is submitted or something like that although you've told us that we can incorporate, improved or reduced uncertainty or parameter. But I'm wondering in terms of climate change we're restricted, I gather to a model that's been developed before December 31st of this year or does it even apply?

DR. McCARTIN: I'm not sure what you're referring to. Certainly it's limited to present knowledge. We're looking at current conditions, but I mean with everything in NRC license, I mean, if after the license is submitted there's some scientific breakthrough and oh gee, we now understand this that would be expected to be evaluated. It would have a

significant effect.

DR. MOELLER: I'm wrong. I totally misinterpreted. Thank you.

DR. THORNE: If I can come back, I think in this case there is present knowledge in that when you do GCM calculations in the future, by definition, those are global simulations because they're all total globe models with a grid that covers everywhere. When we've used them for Northern Europe what we've done is extracted a sub-domain which applies to Northern Europe.

But you can do exactly the same thing for the Western United States. You can say here are runs that have been done by various people for various purposes. I can acquire the data sets, abstract the results and look at the range of variability of the results and this might help with Tim's point. If we're defining what semi-arid means, you could look at the results from those models, say what the range of them is and evaluate them against the semi-arid criterion and that might help you to come to an informed scientific view about how far you can legitimately go in that directly.

DR. MOELLER: Okay, thank you. Jeff Daniels?

DR. DANIELS: I'd like to just add to the comments I made this morning, that I think it's critical to have some quantitative approach that sort of allows us to look at all three of the modeling approaches, the NRC, the work that's being done at EPRI and the work that was done for DOE, so that there's a fluid understanding of where they are different and where the comparisons are the same.

I find it very hard to decipher from a qualitative presentation where all of the issues are specifically identified. Now we talk about it, but I'd like to see something more substantial in terms of dismissing some issues and how other issues have been addressed quantitatively, if I understood some of the presentations.

The other thing I'd like to point out is I think there has to be some further bounding analyses that take into account some of the uncertainty in the technetium issues that were brought up which includes both the potting soil in the field study environments, issues that relate to iodine biology and if there is an issue with a pathway specific uptake, that those be addressed as well.

And finally, I think that there has to be a definitive statement as to what is prescribed and

what is going to be varied. I think it's very confusing that we're following regulations on certain things and we're asking the same questions over and over again and I think it's very important that they be identified up front as to how it's prescribed and that there is an opportunity to improve those calculations, either by request or by new information that becomes available.

And I would recommend that DOE does request the opportunity to use the latest dosimetric calculation.

DR. MOELLER: Thank you. John Till?

DR. TILL: Just one point. I don't know if this is relevant at all, but if you think about the future and if there should ever be a challenge to compliance at Yucca Mountain, it's probably going to come from measurement data. In other words, somebody measures something in something. And my question is really how well the background at the site has been categorized and I know everyone's response is going to be to say well, I know this is done very well. It has to be done very well. This is of such importance. But quite frankly, I have not seen a DOE site where background has been characterized thoroughly and correctly. And what I mean is things like discerning

and defining plutonium background, for example, from the weapons, from the Nevada test site.

This might not be relevant to what you do at all, but I think in the long term, it's going to be absolutely crucial for the operation of this site.

DR. MOELLER: Well, for new data, of course, they do have QA hoops to jump through, but you're talking beyond that. What about baseline data?

DR. TILL: Yes, baseline data, exactly. But I'm talking about things like products that are grown, agricultural products and to define what exactly what background is right now for those products, water, surface soil, whatever the media, a very defensible characterization of the radiation background.

DR. MOELLER: Thank you. Okay, Dr. Kocher?

DR. KOCHER: Yes, I don't for one minute doubt the capabilities of any of the people working on these programs, but I must say that I'm kind of disappointed in the effort that's been put into the biosphere modeling. Much of it is not site specific, if I understood. We just saw the tips of icebergs here. We didn't really get into the details.

But I saw no evidence of any kind of site-

specific information on food chain transfer parameters, distribution coefficients, things like that that enter into the model.

Yes, I know that at the end of the day in terms of a licensing decision, these kinds of things probably don't matter, but if you're going to do something, do it reasonably well because you don't know what other kind of challenges are going to come along. It's not totally obvious, for example, what use, if any, will be made of these calculations beyond 10,000. I mean a court of law may have a different view about what those calculations mean than what the NRC and EPA do.

One of the things I did a little bit of homework before I came here was I read this little slim blue report of a review of the DOE biosphere program that took place about three or four years. And I was kind of struck by the things that were sort of requested in here that still were left unattended, many of which deal with site-specific issues of transfer parameters and the model for retention and the soil root zone probably not being right and here are some things you think about.

I'm conflicted because it probably doesn't matter, but yet I'd like to see it done better.

That's just not a very helpful statement. That's really the way I feel about it though. There are just some things that need some attention, if you're really going to pass muster as a biosphere model by itself.

VICE CHAIRMAN RYAN: Just so everybody can share in their thoughts today, would you tell us a bit more about that publication so we can know what it is?

DR. KOCHER: This was a report from the International Atomic Agency. It was a review of DOE's biosphere modeling program. It was a small committee chaired by Roger Clark. They did their work in the -- I think it was December 2000, January 2001 time frame.

VICE CHAIRMAN RYAN: Is there a document number on it, that would be helpful?

DR. KOCHER: No, it does not have any kind of -- it was published by the agency in the year 2001.

VICE CHAIRMAN RYAN: Okay, thanks. That's fine.

MR. COTORNARY: Dr. Ryan, we're familiar with that report. We can get copies for everyone.

VICE CHAIRMAN RYAN: I'm sure you would be, but I just wanted to make sure everyone in the audience had a chance to hear it. Thank you, Neil.

DR. MOELLER: I wonder if it would be appropriate, Keith, you're waiting to speak.

DR. ECKERMAN: Well -- go ahead.

DR. MOELLER: What I was thinking, again, Dr. Wasiolek is here. Would you comment at all on that? Specifically in terms of whether the degree to which your input parameters are based on site specific data.

DR. WASIOLEK: Well, as far as -- well let's start with the recommendations that were in the IAEA panel report. We tried to address -- this is one of the reasons, this report was one of the reasons why we decided to change the model, so we could address many of the panel's recommendations and we did so.

A lot of these are discussed in our current documentation why we have chosen to select specific parameters, values and we tried to present arguments why we went with certain values and not the other.

Wherever we can we try to use site specific parameters when they are available. We certainly use site specific parameters when it applies to characteristics of dietary and lifestyle characteristics of the receptor because there were surveys that we have, census this data. These are available.

Yes, this is true that we lack in the area

of environmental transport. And therefore, we very frequently would go and use literature data. We will do literature review and base our model input parameter values on existing published information. We try as best as we can to go to reputable sources to grab something that is -- that has some weight behind it.

We are aware that there are like, there are data bases like Radflux which -- did it ever get released, by the way? Officially and not under the table?

DR. THORNE: You mean as a CD? That's what I have. It's never been officially released.

DR. WASIOLEK: These are details like this. I have had a Radflux for those who don't know, it's a European Community has under the auspices of International Union of Radioecology. There was this very precious effort to create a data base of transfer coefficients that are both under -- which incorporated all the IUR data base of transfer coefficients, plus coefficients, transfer coefficients that have time in them.

And then I mean I really had my hopes high because I got under the table a disk, a CD, and I've had it in my drawer for I don't know how many years.

Can I use it? No. Because we are working in the highly regulated environment. These are not the types of publications that we can use although I mean you can use them in academia. You can use them under some other circumstances, so very frequently this was a problem that we were facing. We were aware of some available information, some data base. And it was a big effort. It's not something that you can do in your spare time. I mean there were a lot of people involved and yet, they could not finish the job and make the CD available in an official format to everybody.

So it has original data. It has the original measurements. It does not contain some chewed up something. So we were trying, as best as we could to rely on available information, adjust it wherever we could for site specificity and in terms of characteristics of the receptor, I think we did a pretty job of that.

In terms of environmental transport, we have a lot of generic information. But we try to bind it as far as we could, such that we made sure that we did not underestimate the value of the dose which certainly, I'm sure, is appreciated by the stakeholders.

DR. KOCHER: Can you do something about your item model in soil? Please.

DR. WASIOLEK: Well --

DR. KOCHER: Please.

DR. WASIOLEK: We can --

DR. KOCHER: You can't claim that that's an overestimation of dose.

DR. WASIOLEK: Maybe Dave would like to comment on that.

DR. KOCHER: It's possible that you're too low by a factor of a 100 or a 1000 given the way you modeled the system?

DR. WASIOLEK: I think that if you factor in iodine-27 we are too high by several orders of magnitude.

DR. MOELLER: There's work remaining there. Thank you. That was helpful.

We'll move --

DR. THORNE: Could I? I think I would just like to endorse the remarks that have just been made. I think it was behind one of my remarks this morning that for a small amount of additional resource and I don't point this on DOE, I point this on waste management organizations in Europe and the U.S. together. I think we could have moved to a much more

comprehensive data base at the sort of level of comprehensiveness that I think that we for internal dosimetry.

It's worth recognizing that we don't have the same well characterized, well defined data base for environmental transport parameters. We have a number of partial data bases as has been outlined and IAA technical report 364 is an absolutely wonderful example of that. You look through it. You think this is an IAA standard document. It's got all the numbers I need. No it hasn't. It's got a sprinkling of some numbers, some of which I trust and some of which I don't trust. And I'm hoping the current EMRAS project for the agency, if properly funded and directed, ought to deliver us the sort of level of comprehensive documentation of transfer factors that we haven't quite got in the Radflux data base and that we know that everybody has got in their drawer around the world. But it just needs to be brought out and systematized in that fully qualified assured for use. We just aren't at that stage yet.

DR. MOELLER: I wonder, David, if you could -- you said iodine doses in your opinion are under estimated by a significant. Now why -- could you share with us specifically why you believe -- why

you have adopted, reached that conclusion?

DR. KOCHER: As I understand it, and again, I'm just looking at -- I haven't seen the details. As I understand it, their model assumes that iodine is quite mobile in the surface soil region, so therefore that equilibrium or its steady state, the concentration of iodine 129 in soil is not that much higher than the concentration in water because it kind of just flushes right on through. You don't have this long-term build up over hundreds of years like you do for plutonium say.

But there's plenty of information out there to indicate that iodine is a lot less mobile in surface soils than people commonly believe and so the equilibrium, the steady state concentration of iodine 129 in the surface soil compartment conceivably could be a lot higher than what they're assuming and of course, the food chain dose is directly proportionate to that increase.

DR. MOELLER: Thank you. Well, that clarifies it certainly for me.

DR. THORNE: Can I clarify as well? There is an experimental program in the Narick side which I mentioned this morning, where we've been putting iodine, actually using iodine-125 as a tracer and

putting it into soil columns with a moving water table and tensiometer and soil solution and Redox probes so we can try and get some handles on that. But obviously, it's a limited scale program and I think there's room for quite a lot more research in that area.

VICE CHAIRMAN RYAN: Michael, when do you expect results from those studies?

DR. THORNE: They're being written up at the moment.

VICE CHAIRMAN RYAN: Okay.

DR. MOELLER: Okay, we'll move last to Keith Eckerman.

DR. ECKERMAN: I'd just come back to some of our discussion with regard to the decoupling that's been done and we've talked a lot about the decoupling of the geosphere and biosphere, but there is a decoupling within the biosphere of man from the environment, particularly through the use of the committed dose coefficients.

Now the total problem, looking at it, there's a host of time constants in this problem and you really are approaching it largely by looking at the specific solution at a point in time rather than having wrestled with the general solutions of the

problem. And part of the -- I talked about earlier this morning about calibrating the system and in part, sometimes instruments have a self-calibration in it that you could look at and in fact, there are aspects of a more general solution with coupling that would have been satisfying for individuals to look at to understand how the time constants are all working in this process.

Coming back to the dose coefficient, the assumptions that we're making in dose coefficients are part and parcel the same that you're assuming in the whole analysis that the system is linear and so there isn't a real hang up between chronic exposure and acute exposures, particularly when you look over the integral.

And in many of the new biokinetic models, we've dealt more with a lot of the short term compartments and so -- and when you're dealing with the effective dose you're seeing an approach to the integral converging a lot faster than what you may think based on looking at and thinking about the half lives of the materials we're dealing with.

But there's no reason you could not put the information that is available into the model and couple man tighter with the biosphere responses to

really look at this in detail and that may well have to be done and to answer some of the general questions and that would include the consideration of an individual as he ages through life in living in that postulated reference of biosphere that you've created.

I think you have to be a little bit careful again thinking through the issue of the coupling of the models and what you might gain from that in a more general analysis.

DR. MOELLER: All right, I personally really have nothing to add to what's been said, so I think, Mr. Chairman, with that, this side of the table is wrapped up.

VICE CHAIRMAN RYAN: Okay, well, thank you very much for chairing the expert panel. I want to thank each and every panel member for their time and efforts over the last few days and all the time you put into preparing to come and be with us today. I think we're -- we've been enriched by your commentary and observations and without this panel we wouldn't have gotten nearly as much out of this two-day working group session as we have.

So with that being said I'd like to turn our attention to Jim Clarke, do you want to start with any comments, observations? The floor is yours.

MEMBER CLARKE: Thank you. Let me start with what I was going to end up with since many of the other comments that I had have been very well articulated already. But towards the end of the day yesterday, John Garrick asked what I thought was a very interesting question. He asked what about chemicals? What do we know about chemicals? Who might be able to help us here?

And it strikes me that there are two reasons for that. One is to put radiation in perspective which I think needs to be done. It may be unique. It may not be unique, but it may not be helpful to dwell on that.

Secondly, there are a number of chemicals that have been studied a lot. I would mention lead, benzine, vinylchloride, arsenic, just to name a few. And the whole area of biokinetic models for chemical, internal chemical exposures is an area of great interest, if only to replace our reliance on animal testing. The fact that we still do rely a lot on animal testing may help put it in perspective. In any event, I still think there's merit for pursuing this for a couple of reasons, the reasons I mentioned, putting radiation in perspective and seeing what the approaches that are being taken for toxic chemicals

could add to our analysis.

My other comments, environmental exposure analysis has always impressed me as a great example of the devil being in the details and this is not a meeting to get into the details. I understand that, but some things have come out along the way that suggest that someone should perhaps make a pass through the assumptions that are being made. For example, when we saw the information on relative contributions, there was, I thought, a good example of something that appeared very counter intuitive, that Dr. Kocher brought up and again it suggests to me that it's worth another pass looking at the assumptions that were made, if.

If only for a few reasons. One would be to check consistency. In some cases, bonding assumptions were made and others, distributions were made, so just an overall consistency check. And a check with the consistency of our understanding of the construct of RMEI. When you integrate all of this over all the pathways and all the different kinds of exposures, do you, in fact, end up with RMEI, as we understand it to be?

And then finally, I think that would go a good ways towards making a lot of this more

transparent. So a very nice list of here's what we did, here's what we assumed. This is the degree of uncertainty we think is associated with it. This is the degree of conservatism we think is associated with it and this is how we think it's consistent with RMEI. I think it would just be a nice thing to have.

VICE CHAIRMAN RYAN: Thank you very much.

DR. MOELLER: I'm sorry --

VICE CHAIRMAN RYAN: Please.

DR. MOELLER: This morning when the discussion or when Dr. Garrick asked the question who has looked at chemicals versus radiation, I'm sure Doctor, Professor Clarke is acquainted with Ed Calabressi at the University of Massachusetts at Amherst. I went up there last summer and he had a program on toxicology, you know, a seminar, a meeting. And his objective was to look at the health effects or health responses, human body responses as you increase the dose of chemicals and as you increase the dose of radiation.

And it was well attended. It was a toxicology meeting. There were only one or two of us who were not professional toxicologists, so I found it very educational from that point and I came away with the following fact or something that I gained, the

following lesson that I gained and he and all of the people who presented the papers, it was international. There were people from Europe and all over.

I came away with the conclusion that you should not just adopt a linear relationship or a threshold relationship, but he said look at the data. And he presented slide after slide of data on different chemicals and showed -- and he said that if you really look at the data, you'll come out with the fact that not everything behaves in the same manner. And he showed though slide after slide or graph after graph of reactions to chemicals in which a small amount was beneficial, the J-curve he called it, down and up and he really stressed the fact that you should look at the data and nonetheless, having said that the fact in terms of most of the chemicals was that the J-curve applies.

There were one or two speakers who claim the same J-curve applies for radiation, but that really wasn't the major subject of the conference. The conference was really more on chemicals. And I don't know if that helps at all, but in other words, he said look at the data, don't adopt a generic dose response curve for everything.

DR. ECKERMAN: I might just add that the

lead, an example you just mentioned, is one in which they -- the lead model that was developed for the radiation side recognized, of course, the skeleton as a storehouse of lead and that has been brought into the chemical considerations with regard to blood levels of lead in children.

And of course, the very early cross over link was recognized from Day 1 in the Manhattan Project was the nephrotoxin, the toxicity of uranium and of course, that had always been part of -- so the heavy metal kind of cadmium leads, there's direct applicability of a lot of the modeling that we do. Dealing with the organics is the difficulty, of course.

VICE CHAIRMAN RYAN: Thanks, Keith. Ruth?

MEMBER WEINER: Since my chemical thunder has been stolen, I'll confine myself to nonchemical comments. I think the point was made, but it deserves reiteration that you do really need a baseline of information about the biosphere.

Now the site was extremely well characterized. There are volumes and volumes of the site characterization report, but I did not see the results of that report connected to the biosphere analysis. And if that connection exists, I think it

should be made overt.

From that, I'd like to reiterate what several people said about the RMEI. I think Dr. Moeller began this workshop with a little lecture on realism versus conservatism and I'd like to know how the RMEI is correlated with a realistic picture, who is the RMEI? The regulation itself specifies two numbers and almost everything else, if you read the regulation is available for estimation. It just specifies the two liters per day and the 3,000 acre feet. That's it. And everything else you can estimate.

I think this is an area that needs to be very transparent to the public and I would second the notion that the RMEI be related in some way to the notion of critical population because critical population is something that has been presented publicly and people have some idea of what that means. And we need that same idea for the RMEI.

Sometimes we get bounding values and sometimes we get realistic values and that's in the nature of this kind of analysis. However, there should be some definition of the circumstance. When do you use a bounding value -- and this is really for NRC who is going to review this license application.

When should a bounding value be used or what are the guidelines for using bounding value versus a realistic value and how do you really define realistic?

Moving to the question of climate change, we do need a very transparent definition of semi-arid. Dr. Clarke asked me what's semi-arid and I said Albuquerque is semi -- where I live is semi-arid. Twelve inches of rainfall a year. But that needs to be very clear.

We've heard two -- I heard two different views of the incorporation of climate change notions into the TSPA and Dr. Swift said that climate change won't exceed the paleoclimate changes as modeled by DOE if you represent climate change by infiltration.

I think NRC, if they look at this notion, it needs to be substantiated. I'd like to know if NRC agrees with it, disagrees with it, what they have to offer in that area.

And finally, since ash inhalation for the igneous event is considered to be the heaviest impact for the igneous event, we really do need a particle size distribution for the ash and by particle size both AMAD and density and size.

And as well as everybody uses the standard Galcian dispersion model to disperse everything.

Well, an igneous dike is an area source. It's not a point source. We are not saying at what temperature it is released, whether there's thermal lofting. There is no detail given as to how good the modeling of the dispersion itself is and this is really part of the biosphere and I think that is something that needs to be looked at.

And I said finally, but I wasn't through, so I'm sensitive to what Dr. Eckerman said about lifetime dose. People move around. The Census Bureau has done a very careful analysis and has quantitative estimates of how long a person resides in a particular place. They've done a very, very careful job of that. On the average, people in the United States move every three years.

However, there is a good way to take the Census Bureau's quantitative estimate of residents in a given place and apply that and it's not just you don't have to apply the every three year average. This is a very carefully worked out thing.

If we are looking at lifetime doses, lifetime doses to adults, or however, the RMEI is defined and however that's correlated with childhood exposures and adolescent exposures, I think you also need to look at how long people live in a particular

place. That's all.

VICE CHAIRMAN RYAN: Thank you. I think Dr. Moeller had one comment on the RMEI.

DR. MOELLER: Well, there are two comments on the RMEI. If you read Title 10 part 20 and unfortunately, I just haven't read it in the last week or two, but it says that the dose limits that the Nuclear Regulatory Commission comments or sets in Title 10 part 20, that those dose limits are for the, I believe it's something like the individual receiving the maximum dose.

MEMBER WEINER: Yes, yes.

DR. MOELLER: All right, at the time that Part 20 was promulgated in 1991, I was heavily involved in the review as the regulations were being drafted and finally perfected and finally promulgated. And the review group that I was on pleaded with the NRC to not say to the individual receiving the maximum dose. We said the ICRP has developed this concept of a critical group and you should use that.

Well, apparently the process was too far along to make any change, so what the Nuclear Regulatory Commission did which we appreciated very much was they issued a Regulatory Guide and in the Regulatory Guide they said if a licensee or an

applicant desires to use the concept of the average member of the critical group in place of the dose to the person receiving the maximum, that was acceptable to them. So a regulatory guide is not a regulation, but it outlines a procedure that if the applicant follows it, the Nuclear Regulatory Commission will accept it.

So in the case of the Nuclear Regulatory Commission, the average member of the critical group is part of their regulations in a secondary way.

All right, let's just over to EPA. EPA initially directed their regulations and standards to the maximum exposed individual. A number of us pleaded with them, I didn't play a major role, but a number of people talked to them and they changed it from the maximum exposed individual to the reasonably maximally exposed individual and when they did so, if my memory is correct, they said our desire is to have this be synonymous with the average member of the critical group.

So I presume that if DOE preferred and desired and came to the Nuclear Regulatory Commission and said would it be permissible for us to use the average member of the critical group with Amargosa Valley, I'm not the NRC, but on the basis of what I've

heard, they would approve it.

DR. KOCHER: Yes, in fact EPA has regulations on the books that say the dose to any member of the public shall not exceed and that's an impossible standard to test.

VICE CHAIRMAN RYAN: George.

MEMBER HORNBERGER: My turn. I just have one major comment I want to make but I'll perhaps, as a preamble, may lead into it.

Let me say just a couple of things about climate change. I actually -- I don't disagree that somebody needs to make sure that this gets addressed. I actually think that it will turn out to be fairly straight forward and I think that I actually believe the EPRI position that they have looked at it and found that it really is a no nevermind.

I haven't seen any credible climate model forecast that converts southern Nevada to the tropics. That's just not credible. And so what you see is all of the climate models agree that it will be warmer in a greenhouse, not a lot warmer, a few degrees C. warmer. The precipitation forecast, as Michael has said, are a lot more tenuous and so some of them show drier and warmer. Some of them show a slightly enhanced monsoon which brings a little more

precipitation, but none of them, as Peter alluded to, gets anywhere near an interglacial 400 millimeters of precip.

Furthermore, if you then look at all of the TSPA models that have been done, it is, in fact, infiltration rate that drives -- is very, very strong condition on calculated doses. And so the higher the precipitation, roughly speaking, means the higher the calculated doses.

So I think that this is probably going to be fairly easy to take on. I don't disagree that it should be taken on. I'd be surprised is somebody hasn't already done so and perhaps we just haven't had the paper brought to our attention.

This leads me to the main comment that I have is that whenever we have discussions like this, it is really easy, I think, to point to science issues that we would like to see addressed because there are all sorts of fascinating questions out there like climate change and what it might do to change water use efficiency in plants and on and on and on.

And I think that we wind up always being faced with some kind of a balancing act. It's not necessarily best science. It is credible science. I like the word that John Till used. We have to

maintain maximum credibility. Sometimes that means that we have to use the absolute best science available. Other times, I think that we are satisfied with what loosely might be thought of as a bounding analysis because it just doesn't matter.

And the balancing act is being driven toward more and more measurements or better and better science and actually figuring out whether this site is suitable for a waste repository and those two things are not in my estimation one and the same.

And in this kind of venue we often get caught up with the interesting science questions and don't necessarily have that balanced view on getting the job done.

VICE CHAIRMAN RYAN: Thank you, George. John?

CHAIRMAN GARRICK: Yes. Coming from a background of nuclear safety and risk, there are certain things each time we have one of these sessions that I look for and one of the things that I look for is what we have been able to ferret out of a discussion that would allow me to write down some sort of importance ranking associated with the topic, namely, the biosphere.

Certainly issues were identified and

they're very interesting ones and they include everything that we've been talking about here, background characterization, uptake models, model coupling, the use of site-specific information, realistic calculations versus compliance, definition of the RMEI comparison with other interpretations and so on.

But still, I think that in terms of understanding what the issues are with respect to their contribution to performance, and how the biosphere plays out with respect to that, it seems as though there's still quite a bit of work to do although we got some very good insights into that yesterday.

So importance ranking and context is a very important issue here and we got some insight into that, but it appears that there's still quite a bit to do and that as far as risk insights are concerned, we don't seem to be anywhere near as far along in the biosphere as we are in the geosphere and I'm sure there's good reasons for that.

The other issue that is of great interest to me is this issue of who's doing the realistic calculation. We heard excellent discussions yesterday about the perspective that if you're getting a license

application, obviously you're going to gear your analysis and gear your application to meeting the requirements of the regulations. And no question that that takes you away from doing a kind of analysis that you would do if your whole view was to get the best possible result you could in terms of what realistically might happen.

And we have some very interesting discussions about that on the pros and cons, but I'm still not clear, it's still not clear to me who's accountable for doing the realistic analysis here because as best I can tell, it's not being done.

Now the other thing that I was very interested in at the outset here was the issue of the prescriptive nature of the biosphere calculations and whether or not they might mask realistic issues. And I thought we had an example of that. For example, if you take the 3,000 acre feet and assume that all the radionuclides that reach that region are -- have -- are able to be pumped up into the surface and into the food chain, then you have on the one hand made an extremely conservative assumption with respect to the removal of radionuclides from the biosphere, but you've made a very nonconservative assumption with respect to the final disposition of radionuclides as

was very well pointed out by Steve Frishman. And I thought that was a very interesting observation.

And it reminds me of so much risk work we did in the nuclear power industry where you have several performance parameters, core damage frequency, release fractions and dose. And we discovered very early in our risk work that when you do a fix to reduce one of those, you don't necessarily fix the others. When you do a fix to reduce the core damage frequency, on the contrary, you may increase the dose, the off-site dose.

And I have questions about that here. When you make an assumption about the biosphere such as the disposition of the radionuclides, what does that mean in terms of how you've underestimated other things if your approach had been to do a realistic analysis.

Another area that I agree with David Kocher on this one and I didn't see much in that I would have liked to seen more was are we getting our money's worth from the billions of dollars of site characterization work that's been done and how has that manifested, how is site characterization manifested in the biosphere work? And I didn't see a whole lot of evidence of that.

I would think that the site specificity issue is something that is going to be of great importance to the public and the more that they can see a connection between what we learned from the site characterization program and how it impacted the dose calculations, I think the better off we are.

So those are some of the things that come to mind and I think that one of the things that I like to do whenever we have a session like this is to identify what appears to be the most important issues and I think the things that I've mentioned are in that category and some of them are analytical issues and some of them are data collection issues and some of them are modeling issues and what have you. But I think that's all I need to say right now.

VICE CHAIRMAN RYAN: Thank you, John. Boy, it's hard for me to add after all of these smarts who have said what's on their minds.

DR. KOCHER: Can I ask John a question?

VICE CHAIRMAN RYAN: Sure.

DR. KOCHER: If I understood you right, you said you didn't really come away with a lot of warm fuzzies about risk insights in the biosphere part?

CHAIRMAN GARRICK: Yes, that's correct.

I saw a lot of useful information and some risk insight, but I saw very little that would give me the feeling that there was a real risk analysis model that was the underlying driver of the results.

DR. KOCHER: Okay, I was wanting to explore what you meant by that.

CHAIRMAN GARRICK: What I would like to be able to see here is that the end of the exercise here we have a PDF on the dose and I'd like to be able to decompose that PDF into the contribution from different segments of the model that you might call it the infiltration model, the near field model, the geosphere model and the biosphere model.

That's the kind of models that we've learned how to develop on reactor risk assessments.

DR. KOCHER: And the question you posed early yesterday or the problem you posed about you'd really like to see, get a firm idea of uncertainty in the biosphere part compared with uncertainty on the other part and I don't really think you can come away with a warm fuzzy about that because it depends on when in time we're talking about and a host of other things, so I agree with you, that issue is kind of still -- most of us believe that the uncertainty is under the ground somewhere because we've studied this

stuff to death for 50 years. But there are issues --

CHAIRMAN GARRICK: But you can't take snapshots of different discrete time intervals. You can do things to at least develop a sense of understanding about how things might be for different discrete time pieces.

VICE CHAIRMAN RYAN: It's going to be hard to add to that, but let me try and add a few thoughts. I think first of all I'd like to recognize all of our speakers and presenters for these last two days. I know they put a lot of time and effort into preparing. I want to thank the staff at the Center who is on the TV screen for their participation and for their preparation and for their representatives here today. And I'd like to especially thank Mike Lee who has been the lead staff person in putting together the biosphere working group and organizing all of the attendees and participants and that's a tremendous amount of work and we all appreciate your effort very much, Mike, thank you.

Let me try and summarize with a few themes. I think the themes that I take away from this biosphere working group are some interesting aspects that are probably unique to this project. There's a very specified and stylized calculation and we've

heard a variety of opinions and issues regarding that stylized calculation, ranging from fixed values, we do it in that specific way. And then we underpin with examinations of models and supporting evidence and so on. That's one aspect.

So we have a fixed requirement and we have a lot of other science questions and modeling activities that are underpinning that assessment that's pretty fixed. The second to me and it comes from I guess my bias of focusing on short term exposures in the work place as an area of major concern as opposed to chronic exposures in an environmental setting of somewhat a complex nature that's very much protracted in time over lifetimes and many lifetimes and that, I think, is something we can all think about as having special aspects that maybe need our thought and attention.

I think we have to be careful to take too much away from this working group because it's part of many working groups that we heard about, you know, from package performance to the waste interaction, waste package interaction, the environment of the repository itself to performance confirmation and soon to be upcoming the geosphere working group that will examine the coupled part of this.

So in all of these pieces and parts will integrate in DOE's license application and it's the totality of all that that I think will be assessed and judged and I caution us all not to pick on one or two parameters or issues from this working group as critically central and that's part of the risk ranking process and I think everybody realizes that, but I just remind everybody that this is one slice of a big piece and it's helpful for us to look at them that way. In fact, it's the only practical way to do it without spending weeks on end in one room.

And with that I think, Mr. Chairman, I'll turn back the working group session and declare it closed and turn back the meeting to you.

CHAIRMAN GARRICK: Excellent. Okay, let's see. I think that probably what we ought to do is we've got -- it shows on our agenda that we should have discussion of the letter. Yes.

MEMBER HORNBERGER: At the risk of going backwards. it just struck me that probably one of the first things that Dave suggested was that we should have some kind of discussion on this age dose business and I don't think that -- did we bring that to closure? Did I nod off?

None of our panel mentioned that in the

final summary.

VICE CHAIRMAN RYAN: We talked a lot about age dose issues. What particular --

MEMBER HORNBERGER: Well, I mean we talked about it, but I didn't hear any resolution. I mean I heard it as a question, what are we going to do.

DR. ECKERMAN: I thought the dosimetric information you need to look at age is available. I thought we had passed that off to the supportive satellite calculations that have to be done because of the regulation focusing in on the adult.

There are other -- there are a number of other ways to handle that problem. One would have been to have looked at a per capita kind of a dose coefficient, but that largely forces you over to pretty much the adult anyway because most of one's life is spent as an adult rather than as a child. But I think the age in my mind, the age issue has to be addressed with respect to the supportive information and it may well not be an issue with regard to the compliance kind of calculations. So I would definitely encourage that -- that would be my resolution to the comment.

CHAIRMAN GARRICK: Yes.

DR. THORNE: Just a clarification on that

as well. I think if you're going to make that comparison, what you can't do is do the infant calculation and the child calculation and then compare with the RMEI because the RMEI isn't the same kind of thing that you need to do infant, child and adult as if you were doing a critical group calculation for each one.

DR. KOCHER: Yes.

CHAIRMAN GARRICK: Dade, do you want to add to anything that's been said about that, since you asked the question?

DR. MOELLER: I don't believe that I do. I thought that Keith wrapped it up in several ways in that the -- and I hope that I'm not misquoting you, but the fact that the dose -- that a person spends most of his or her life as an adult. The dose coefficient for an adult, if it's applied even over the full lifetime of an individual yields reasonable, very close estimate to the dose.

Now it's of interest to know the dose to an infant or a teenager, but that only takes place, I don't know what an infant is, you know, whether it's up to 2 or 3 years, but it's a short time. You're a teenager from 13 to 19, whatever that is, 7 years. So in that respect I felt that it was resolved.

MEMBER HORNBERGER: I guess what wasn't clear to me was on a chronic exposure why this was or should be a big issue.

DR. ECKERMAN: And it shouldn't be a big issue.

CHAIRMAN GARRICK: Well, I want to add my thanks to what Mike said to the panel and the consultants and the members of the committee.

These working group sessions are extremely valuable. They give us a chance to bore in on issues that are important to the job we're trying to do. We know that all of you put in a lot more time than your pay scale probably warrants and some of you have come from long distances and they are a very valuable part of our whole process.

So we are very grateful to you and we hope that, of course, that we have an opportunity to interact with all of you more as we move closer and closer to a license application.

I think what we'll do now, the committee has to somehow figure out what we're going to do with all of these fine words of wisdom and we need to figure out and agree as a committee the points we'd like to cover in a report to the Commission.

And so what I think we'll do is we'll do

that, but I think that before we engage into that process, we'll take a 15 minute break and then come back and work on our reports.

Thank you.

(Whereupon, the proceedings went off the record at 2:07 p.m.)