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UNITED STATES NUCLEAR REGULATORY COMMISSION'S

ADVISORY COMMITTEE ON NUCLEAR WASTE

September 23, 2004

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE

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THURSDAY

SEPTEMBER 23RD, 2004

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The Committee met at the Suncoast Hotel, 9090 Alta Drive, Ballroom A, Las Vegas, Nevada.

Advisory Committee Members Present:

MICHAEL T. RYAN	CHAIRMAN
RUTH F. WEINER	MEMBER
ALLEN G. CROFF	MEMBER

Others Present:

KEITH ECKERMAN	Oak Ridge National Laboratory
FRED HARPER	Sandia National Laboratories
DAVID JOHNSON	ABS Consulting
DR. BILL MELSON	ACNW
MICHAEL LEE	ACNW
JOHN LARKINS	ACNW
B JOHN GARRICK	NWTRB
GEORGE HORNBERGER	NWTRB
JAMES CLARKE	ACNW

Others Present:

WILLIAM HINZE	ACNW
BRUCE MARSH	ACNW
BOB BUDNITZ	LLNL -- on detail to DOE
LYNN ANSPAUGH	University of Utah

A-G-E-N-D-A

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

8:05 a.m.

OPENING STATEMENT

CHAIRMAN RYAN: This is the second day of the 153rd meeting of the Advisory Committee on Nuclear Waste. I am Michael Ryan, Chairman of the ACNW. The other members of the committee present are Ruth Weiner and Allen Croff.

Also present are ACNW consultants William Hinze and Bruce Marsh. Jim Clark will be joining us shortly. Today this committee will complete its working group meeting to review and discuss issues related to the evaluation of igneous activity and its consequences at a potential geologic repository in Yucca Mountain Nevada.

As done yesterday the committee intends to gather information, analyze all the issues and facts, and formulate the proposed positions and the actions as appropriately in the form of advice to the commission.

This meeting is being conducted in the accordance with the provisions of the Federal Advisory Committee Act. The rules for participation in today's meeting have been announced as part of the notice of this meeting, previously published in the federal register.

Mr. Mike Lee is the designated federal official for these sessions. A transcript of this meeting is being kept, and the transcript will remain available as stated in the federal register notice.

It is requested that speakers first identify themselves and speak with sufficient clarity and volume so they can be readily heard. We have received no requests for time to make oral statements from members of the public during today's sessions.

Should anyone wish to address the committee, please make your issues known to anyone in Committee staff. As an administrative matter, if you haven't already done so, it is requested that you sign the table in the back.

We also request that, if you haven't, please confirm that your cell phones are turned off, or alternatively have been rendered silent or on low.

Lastly for those of you who wish to do so there are comment feedback sheets available at the sign in desk. At the conclusion of today's meeting the ACNW will conduct its planning procedures meeting.

For today I'd like to note that yesterday the committee held two sessions dealing with issues related to the evaluation of probability and consequences of igneous activity in the Yucca Mountain region.

The technical discussion was excellent and members have been provided with a lot of information to consider. Today's third and final session are intended to be a follow up of the committee's earlier February 2004 working group on biosphere assessments.

Five presentations are currently scheduled. The first two are by doctors Keith Compton and Don Harper, representing the NRC staff in the center for Nuclear Waste Regulatory Analysis.

These presentations will focus on the staff's approach to modeling doses due to a disruptive igneous event and how this approach will be used by the staff to review a DOE license application.

Dr. Hooper will discuss how the results from the Center's recent tephra ash remobilization study have in fact -- with the NRC's TPA computer code.

At the ACNW February 2004 meeting an ACNW panel of invited experts offered several recommendations for the respective staff to consider in the modeling of dose due to disruptive igneous events.

To explore this issue -- these issues in more detail three subject men or experts have been invited to make presentations at the ACNW. The invited subject matter experts in the proposed areas of discussion I'll talk about when we finish our first session.

This session will be followed by a round-table discussion. Of course at the end any time members of the public can ask questions and provide comments at the -- recognized by the chair.

To help the committee explore the issues and interrogate the right speakers, again in think just converse with would be better, we are reminded that we have several of the panel of invited experts saying, they include Dr. Budnitz, Dr. Dave Johnson, Dr. William Hinze, Dr. Bruce Marsh, and Dr. William Nelson.

Again thank you all for your participation again today. At the conclusion of today's session Dr. Johnson will provide some summary remarks concerning the issues discussed the last two days in the context of the application of the risk triplet. And now for today's first presentation, I turn the microphone over to Dr. Compton. Excuse me, just before you start, we have asked about the noise that you hear.

We're told that it will go on

intermittently at some low level of buzz. I apologize for the inconvenience, so if speakers and questioners would use the microphones it will probably help us all here a little better. Thank you very much.

NRC STAFF PERSPECTIVE ON CHALLENGES TO MODELING DOSES DUE TO DISRUPTIVE IGNEOUS EVENTS

MR. COMPTON: Is this on? Can everybody hear me? Great. I'd like to introduce myself. My name is Keith Compton. I'm with the Performance Assessment section and the Division of Highland Waste Repository Safety at the Nuclear Regulatory Commission.

The first thing I'd like to do is to acknowledge the contributors to the reports, Britt Hill and Pat LaPlante directly contributed to this reports.

And at the NRC Richard Codell, Tim Parking, Tim Rubenstone and John Trapp contributed. And of course there are a number of people who have been involved in the development of the modeling approach.

And I can't list them all by name but, certainly this is a representation of their work. What I'd like to do in this talk is to step back and provide kind of a general overview of the approach that we use and our TPA code.

And I want to emphasize that what I'm, going to be talking about -- what I'm going to be saying today is going to be purely descriptive. I want to simply provide you with an explanation of what it is that we actually do in calculating the doses so that, in the subsequent discussions, you'll understand what model we're actually using, and how we're of calculating things.

And I think that might help clarify issues of where things can be improved or what's in the limitations of these. Okay. Could you go back to the effective waste, few more things?

I also want to put in that I am only going to be discussing published work. Unfortunately I can't give you any kind of progress reports. It is a work in progress.

So I might be limiting my talk to things that have already been published. So I just wanted to say that. And furthermore, although I am not going to be talking explicitly about these insights, I am going to focus my talk on key assumptions and key approximations.

And those are identified based on the fact that they have the most significant contribution to those. And, for example, the first incident of that is I'm going to be talking about the extrusive of events.

I'm not going to be discussing modeling of doses due to intrusive activity of the damages of waste package, at least ground water contamination. But that tends to result in lower doses than our extrusive case.

Now for the outline of my presentation. The bulk of my talk is going to be going through a discussion of when I hold conditional dose analysis. This is the evaluation of the doses given, that an eruption occurs.

So, at this point, the conditional dose analysis does not take into account the probability of occurrence. It assumes that an eruption occurs and then it is done probabilistically to examine what the consequences are.

And I'll essentially be stepping through. This is -- you can kind of recognize this as a traditional risk assessment chain of release transports, exposure and health effects or dosimetry.

And I'm just going to be stepping through each of these and getting some of the key assumptions and approximations. I will end up with a brief discussion of how we go about calculating the risk.

In other words doing the probability rating. And I should point out that that probability rating is not done within the TPA code, that's a post-processing step.

Code Structure, next slide please. The code structure there -- there are four modules that are primarily involved in the evaluation of doses of igneous activity.

First module in the TPA code is called volcano. And this -- you can think of this as identifying the release or parameter that are evective to the release.

And such is the number of waste packages that are entrained, the location of the eruptive center of the repository, and the time that the eruption occurs, and so forth.

The next module is ASHPLUME. We've heard some discussion about that. That's basically very similar to the TEPHRA code that was discussed yesterday.

And the ASHPLUME model takes the eruptive parameters of the volcano and is used to compute the deposition both of spent fuel and of ash -- that's the receptor location.

The next module of ASHRMOVO. This is a code that brings -- or the module that brings in the temporal evolution of the dose. The doses of course that can persist over time.

There are different removal mechanisms for the ash blanket. And ASHRMOVO accounts for the temporal evaluation of those. And DCAGS is the actual dose assessment code that actually does both the exposure and those calculations.

Next slide. Now when I put this up, that's a little bit hard to read. It is dark. But I wanted to emphasize that in, the results, what we find is that the dominant radionuclides that contribute to the peak dose, which occurs at about 300 years in our approximations, the key radionuclides are americium-241, and then the plutonium isotopes. Now if this makes sense, essentially in the kind of period between about hundreds to a thousand years, the bulk of the radioactivity is associated with americium and plutonium.

And a release event that can release these without taking credit for -- to hold over the other things that might tend to delay nuclides would give you these nuclides contributing much more to the dose.

And furthermore, in a highly dusty post-eruption environment you're going to have a lot of inhalation. And so again, these show up and the big contributors.

Putting this slide up allows me to focus on the fact that I'm going to be talking in the dose assessment primarily about the inhalation pathway, because that is the dominant pathway.

And I'm going to talk a little bit about americium and plutonium because that pathway -- those nuclides, if you understand those, you can really understand the bulk of the consequences. Next slide.

Now a number of you are probably very familiar with this type of approach. The dose at any particular time after the eruption is simply the intake -- how much of the radionuclide you take in, multiplied by a dose conversion factor.

And the intake is simply the air concentration times the breathing rate and then adjusted by a fraction or how long you're -- what fraction you're exposed to it.

This is a very traditional dose assessment. And furthermore the airborne concentration is simply calculated by the mass loading, the amount of dust in the air, times the specific activity, or the concentration in the ash.

I put this up, it's very simple but it's useful to think about this equation, because essentially understanding how each of these perimeters is identified will tell you how they come into the calculation of the consequences.

CHAIRMAN RYAN: Can you take just a quick question, if I may?

MR. COMPTON: Sure.

CHAIRMAN RYAN: Say you multiply the specific activity in the ash times --

MR. COMPTON: Mass loading?

CHAIRMAN RYAN: Yes, mass loading, sorry. That needs to be in the respirable size fraction, I would assume.

MR. COMPTON: That's true. And I'll answer that.

CHAIRMAN RYAN: Okay.

MR. COMPTON: Basically what we do is we define the mass load as the respirable mass load.

CHAIRMAN RYAN: But you'll cover those details --

MR. COMPTON: I'll talk about that. That's correct. And, furthermore, I should point out that you realize we do use a mass load approach. We don't -- we define a mass loading.

We don't to a calculation of the airborne dust concentration using like a resuspension approach. So again this is -- and I'll talk about it a bit later.

So that kind of private framework for how we go about it, we're going to step through kind of each of these steps, we've -- this -- the first step we really discussed in great detail yesterday.

But I will reiterate the key assumptions and approximations. First is that the number of waste packages effects it is a function of the conduit diameter.

Conduit diameter is sampled, and then the number of waste packages affected is fused with that. It ranges between I think about one percent typically.

And the next key is that 100 percent of the inventory is contained in those waste packages that are affected are assumed to be entrained into Tephra.

Essentially what we're saying is that no credit is taken for a waste package. That's what that means. And furthermore the entire erupted inventory is presumed to be available for atmospheric transport.

This approximation, another way of saying it is that this is all -- the inventory goes all into the Tephra, it doesn't go into a lava fraction or anything else.

So it's all -- all the inventory goes into the Tephra. Those are key assumptions that were discussed yesterday, and I'm not going to go into them in much detail.

Next slide please. Now the atmospheric transport, this is calculated. The ASHPLUME model is based on the model developed by Suzuki, of which a number of you are probably familiar with.

A few of the key assumptions in this model -- or in this limitation is that we -- when we run the code we define that the wind to always blow toward the receptor location.

The reason for this is there is the possibility that, even if the wind is not blowing south at the time of the eruption, the RMEI may still be exposed because of mobilization of ash or mobilization of contaminated material down to the RMEI location.

So this is kind of a first cut at trying to account for the probability of exposure -- of the conditional probability of exposure. I'm going to come back to remobilization because that's something that is of apparent interest.

Furthermore, and this is an assumption that the Suzuki model, the wind field is assumed to be constant, one dimensional. For any particular realization, the wind field is one dimensional that doesn't vary.

Of course we can -- for different realizations we can have it blowing it at different speeds, so forth. Next tephra particle sizes are modeled.

It is a distribution of tephra particle sizes. That's in the ASHPLUME code. We do, to account for the fact that there is uncertainty in what the ash particle distribution size is -- that that's something that's very hard to understand.

That the mean value of that distribution is sampled and between -- at ranges between 100 microns to up to 10 milliliters. And then finally, and this is again -- this is based on the Suzuki model, the deposition is based on essentially gravitational settling model.

What this means is that the transport model is applicable for particles greater than about 15 to 30 microns. Below that gravitational settling is not the proper mechanism to use.

So the codes certainly dependent on a

smaller particle size but it wouldn't be appropriate to do that. So -- and the key -- the output of Suzuki model, as I mentioned before, is that it gives the concentration of spent fuel at the RMEI location, and the concentration of ash.

And a key approximation or a key approach here is that the concentration of spent fuel in the Tephra, and the ten inch soil there is computed essentially by looking at the total inventory and the -- at the location, a totally active inventory, dividing that by the total amount of ash.

So at this point you start to -- and that is carried forth. Essentially what that means is that you don't carry the particle size information from the transport model to the dose model.

At this point you have essentially kind of a homogenization. And I guess --

CHAIRMAN RYAN: Let me just poke at that for a second. What that does -- it takes particle sizes that are not respirable and if I'm hearing you right, turns them into respirable particles.

MR. COMPTON: Yes. That is the potential impact.

CHAIRMAN RYAN: Well it's not a potential impact, it's what you've assumed in your calculation.

It's what you've done. You've taken things that are way above respirable and by doing that arbitrary step function, you've turned them into respirable particles.

MR. COMPTON: That's correct, and I'll give a brief response to that and then for a little bit more I'll talk to Dr. Britt Hill who can address this.

The brief response is that we do observe dustiness at post-eruption, and we do observe respirable particles at an eruption site. So there is some respirable dust.

And because I am not an expert on any technical basis for that, I'd like to ask Britt Hill to discuss that.

MR. HILL: Britt Hill CNWRA. Give you a very brief explanation. None of the computer codes that are available are suitable for modeling both course particles, particles of 100 microns and greater, as well as the fine particles that we see in these deposits.

Fine particles -- and I'll just say 100 microns and finer right now. Fine particles in these fall deposits range from several percent of the total deposit near the vent, to by the time you're around 20 kilometers away from the volcano, they can be 20 to 30 percent of the total deposit.

We can't model those discretely because those fine particles are very sensitive to assumptions that you make for vertical turbulence and vertical mixing within the atmosphere.

Courser particles aren't sensitive to those assumptions, so you can use simple invective relationships in the Plume model, and a simple settling model to account for mass redistribution.

But we are not ignoring the fine particulus in this model. We are taking a total grain size distribution with a mean and standard deviation, and allowing that grain size distribution to change with distance from the vent in the way that we see that occur in volcanoes.

So, while we are not modeling the explicit transport of fine particles in this code, because none of the codes can do this, we do account for variations in abundance in fine particulates by the way the total grain size distribution changes with distance from the vent.

CHAIRMAN RYAN: I'm struggling with two things. And I accept your answer for what you did, but I'm not convinced that it represents the physical realities that you'd see. Let me finish. Respirable to me is 20 microns and down.

You know, two things that strike me are the activity partitioning, where's the radioactive material end up? What size fraction? And just having a step function where you distribute the material out then you kind of make this step over to respirable sizes, it just seems like you're preserving the availability of the radioactive material and not being a justified physically demonstrated assumption.

I appreciate the modeling difficulty you have but just because it's difficult to model I'm wondering how conservative your assumptions could be.

MR. HILL: It's not just difficult to model. We and many people have tried to make models like this. And it's an area of ongoing research involved in the volcanological community.

CHAIRMAN RYAN: Well that would be difficult.

MR. COMPTON: I understand that.

MR. HILL: At this stage there's practical difficulties and impractical difficulties. The behavior of fine particles, 100 or let's just say 20 microns now, during an eruption is very complex to model because in the eruption plume, there are quite simply a conglomeration of effects.

These particles are not discrete. They adhere to courser particles, they have electrostatic charges. They respond to moisture in the plume, and they clump together and often behave like courser particles and break apart on impact when they hit ground.

This is one of the phenomenon has been recognized since Mount Saint Helens' eruption, where you saw a secondary fall out peak around central Washington from just these effects of ash sticking to each other and forming a secondary fallout.

So while I appreciate the desire to have a more realistic modeling, I and our consultants, including some people who are writing the book on these processes cannot come up with a model that will take and account for both the courser bulk of the deposit, as well as the finds that we would like to have to do a particle tracking type approach to dosimetry.

It's beyond state of the science to request that, or think that it's capable in this program.

CHAIRMAN RYAN: I appreciate that but I guess it must be that lack of having the science to move forward has to be put in perspective that the fact is still calculating doses with two significant digits of contributions from different radionuclides.

So I offer there's a lot of uncertainty in the result as a result of what you just stated. I'd be curious to see how you'd get through that.

MR. HILL: I think to a first approximation we're doing pretty good with the finds because one of the things we have done is taken numerical models, and compared them to actually deposits on the ground for an eruption that we have a good data center for, where we went out and measured the fresh deposit.

And these are coming out -- this model is going us a reasonable approximation for the amount of fine material that we're seeing 20 kilometers away from one of these types of volcanoes.

CHAIRMAN RYAN: I'll remain to be convinced.

MR. HILL: Okay.

MR. COMPTON: All right, I do want to bring that up because that is a very important assumption. It's very important to be clear about that so that the panel can understand what it is that we're doing.

It certainly -- it is a challenge to model that realistically you wouldn't need to know much more about the process of incorporation and the process of bringing down the particles.

So I wanted to present this to make it clear as to what were doing, so it can be discussed. The next step is exposure assessment, and the -- as I mentioned previously the airborne activity concentration, now that we've made our approximation in the previous step the airborne activity concentration is simply the mass load, the milligrams per cubic meter, and the specific activity, the multiple of those two things.

And our approach is to assume that the mass load is initially elevated. We vary that between the -- and I should point out that there is a backup slide both on this, and there was also a backup slide on the fundamental model for that transport model.

The mass load is initially elevated, and ranges between two to about 30 milligrams per cubic meter. That's assumed to be the respirable mass load.

And then it is assumed to decline in approximately a first order fashion back to background levels, depending upon what values you sample or both the initial and the background declinement.

The production of that ground takes about 30 to 100 years. And these mass loads that are used are based on, correct me if I'm wrong, I believe those are based on measurements at certain -- magma. This is the basis for selecting these values. Next slide. For dosimetry I unfortunately can't present a lot of great technical detail because our approach to dosimetry is simply to use federal guidance.

And I -- given previously the equation -- and you saw where that was brought in and we simply use the inflation coefficients. We do make the assumption that the particle sizes are of one micron.

We do assume adult dosimetry and, in situations where we're faced with choice of the chemical form, we tend to use the -- or we do use the form that gives you the highest dose conversion factor.

If you go the next slide then I can kind of talk a little bit to the impact of those. Again, bearing in mind that our two dominant, those contribution nuclides under the assumption for making our plutonium and americium, I did kind of a quick check using the code, which allows an approximation or a correction for particle sizes up to -- it'll do it up to 20 but I think it's most -- it's recommended to go up to about ten.

And you can look at the change in the dose conversion factor for change in particle size. And what you see is over this small range, there's not a great -- there's not a significant difference in the americium or the plutonium dose coefficients.

The lower curve is the less conservative chemical form, and there there is some reduction as you go to larger particle sizes. But, again, there's not a huge change in these.

However, I should point out that these are all predicated on the FGR 11 model. If you were do more to a different dosimetry model -- to the some of the more modern dosimetry, I'm not going to say what the impact is going to be.

Hopefully Dr. Eckerman will talk a little bit about that. Again I'm willing to present what we've done.

CHAIRMAN RYAN: I appreciate that Keith and maybe it is a question for Dr. Eckerman a little later, but -- and I don't know the answer to this but, is there a logical inconsistency between assuming a class W for one and a class Y for the other?

MR. COMPTON: The -- I don't know. Well for americium I believe there's only -- in the model there's only one chemical form. And --

CHAIRMAN RYAN: I guess we're okay there, I think -- I don't want to call it in my head but I just --

MR. ECKERMAN: That's the difficulty of using that particular data set. It's -- should get beyond -- and in fact, I thought in our last meeting we had a recommendation to get away from the Federal Guidance 11 information because of questions like this.

You only -- that was for workers and it only addressed one chemical form for the americium.

CHAIRMAN RYAN: Maybe we can just hold that thought because --

MR. ECKERMAN: I'll amplify that later

MR. COMPTON: And again -- I'm presenting the things that were already published. This is what we have published so I would certainly be interested in any -- next slide please. Now that has finished.

I hope that that's given you an idea of how we actually get from these different eruptive parameters to the conditional dose. The next step is to -- how we get actually to risk?

This is not necessarily pointing to the curve. And the -- this slide kind of gives an idea of how you would go from a conditional dose to risk. These slides, the yellow slides for example, give you an idea of what the output of the TPA code would be.

The dose would be the highest in the year of the eruption because the mass loads are the highest, the exposure is the highest. And then they would decline.

That decline is mainly driven by the decline in the mass load. There are, of course, other factors that would tend to reduce it. But, with our approximation of production with about 10 year half-life, that's the dominant reduction factor.

And to calculate the mean dose at any particular time, because our compliance is based on the mean dose, essentially take the average at any particular point, and average the different doses.

And then account for the probability that an eruption occurs at all over the compliance period. So you said you allowed the eruption to occur at different times and with different properties and then perform this averaging.

One thing that I'll point out that this slide illustrates is the effects of persistence. If you have mass loads or exposures that persist for an appreciable time after the eruption, this of course is not going to affect peak dose particularly, but in the probability weighting process, longer -- higher persistence of longer duration higher tails, is going to result in the averaging procedure averaging over generally larger values.

And therefore it will increase the risk -- the probability weighted dose, and I hope that that is --

CHAIRMAN RYAN: I'm struggling with this curve. I really don't know what I'm looking at. I mean you've got four events followed by a decay period and I think the yellow line in the middle is the mean, is that right?

MR. COMPTON: Say again.

CHAIRMAN RYAN: You've got four events that have occurred, your 100, 300, 500, and 700.

MR. COMPTON: Right.

CHAIRMAN RYAN: So what it looks like is doses delivered from that event over --

MR. COMPTON: Right.

CHAIRMAN RYAN: -- a couple of hundred years, mainly 100.

MR. COMPTON: Let me step back for a second.

CHAIRMAN RYAN: Could you help me

understand the shape and what these terms are telling me?

MR. COMPTON: Sure. And some of that is, well right, let's pick the 100 year event. Let's say you have an eruption in year 100. And you might have a mass load.

And I'm just going to make something up. But you would have a mass load of say 15 milligrams per cubic meter. You can calculate those from that.

CHAIRMAN RYAN: Right.

MR. COMPTON: Then in the next year it's going to reduce slightly but it will still be elevated because your exposure is elevated.

CHAIRMAN RYAN: Wait. How do we get from year one to year two? It's got to go down. What makes it go down? Tell me a little bit about the mechanisms that you've assumed.

MR. COMPTON: Basically we use the reduction as primarily the reduction in the mass loading. That's that equation.

CHAIRMAN RYAN: By ran out, by settling, by deposition, all of the above?

MR. COMPTON: We don't have -- right in the model we don't specify what the process is. This is based on -- and again I'll have to turn Britt to --

CHAIRMAN RYAN: Let me shape my question as Britt's coming to the microphone. I'm trying to figure out how a mass loading of 15 milligrams per cubic meter stays that high for 100 years.

MR. COMPTON: Right.

CHAIRMAN RYAN: Or decays so slowly over a couple hundred years according to these codes.

MR. HILL: Okay, Britt Hill, center. First that was just an example number. That would be an extremely high mass load for what we're dealing with.

What we have measured in the field following volcanic eruptions, four years for example after an eruption, would be on order of a milligram per cubic meter for lightly disturbed activity levels.

Again you have to make sure you're talking about the right activity level, not just a static mass load but an active mass load. Now this is an example on how that mass load through time may decay.

You've got to remember that we are talking not only about the C-2 deposit, but the contribution from deposits around this particular location, including come of the things that Dr. Hooper will be talking about in his presentation.

Remember this is the TPA 4.1 J version.

There have been modifications to that but they have not been presented yet.

CHAIRMAN RYAN: The critical issue in all that then is resuspension correct?

MR. HILL: Pardon?

CHAIRMAN RYAN: Resuspension is really the critical issue here then?

MR. HILL: Yes.

CHAIRMAN RYAN: Of some activity level?

MR. HILL: Airborne concentration above the deposit.

CHAIRMAN RYAN: And again I'm trying to just make sure I'm clear, and maybe somebody else has the same question. But, what you're really calculating at some point in the concentration in the breathing zone of the RMEI or somebody --

MR. HILL: Yes.

CHAIRMAN RYAN: And it's caused by two things. One is blow-in from other areas, resuspension due to whatever activities are assumed in that area, and then the normal deposition processes for the atmospheric condition you assume at that location. Now is that pretty much it?

MR. HILL: That's pretty much it.

CHAIRMAN RYAN: Okay.

MR. HILL: Because the third component where the RMEI location is right next to a major drainage, this 40 mile wash drainage. And again in our first model, the TPA 4.1 J, we are recognizing that mass could be redistributed from upstream down, and deposited in this general area of the RMEI.

So it's not simply blowing it from a regional dust field, but a concentrated or potentially concentrated deposit coming down and being deposited in the general location of the RMEI.

That would also bring in fine particulates into the nearby suspendable field and sustain the mass load, or sustain the airborne particle concentration through time at a rate that would be greater from just measuring it at an in tact deposit and watching the normal soil stabilization processes occur.

CHAIRMAN RYAN: Are any --

MR. HILL: That's why there's a decay function that has a variable half-life. And Dr. Compton is just showing how the assumptions on the half-life in that decay in airborne particle concentration can affect the dose calculations when they're put into a probability weighted analysis.

CHAIRMAN RYAN: More to come

MR. HILL: Pardon?

CHAIRMAN RYAN: More to come.

MR. HILL: More to come.

CHAIRMAN RYAN: Okay.

MR. HILL: And we'll expand in Dr. Hooper's talks some of the technical basis for understanding why there's a decay function and not a very sharp drop off like you would see in a regime that's dominated by simple soil stabilization and surface leeching processes.

This is not a typical soil out around Yucca Mountain region.

CHAIRMAN RYAN: Okay. Thank you.

MR. COMPTON: Okay. And that does bring up the fact that essentially this issue of remobilization or replenishment of dust loads we -- in the current model, the current code we've accounted for it essentially in two ways.

I mentioned previously we fixed the wind direction to blow towards the receptor, and we do have the -- this decay being a little bit more slowly.

Those are the two ways in which we try to kind of mimic the effects of remobilization in the current version of the code. However, as was mentioned, Dr. Hooper is going to up and talk about some further work and remobilization.

We recognize that's something that's important to understand a little bit better. Now final slide. Again I just wanted to go over and identify what were the key assumptions and approximations.

As I started at the outset, my goal was descriptive. My goal was to be very clear about what it is that we're actually doing and then there could be a discussion of the technical basis of those. And then finally I want us to talk a little bit about the factors that are likely to have a significantly influence on the risk or the probability weighted dose, because those -- there's extra factors such as persistence of the deposit and such that come into the calculation of the risk.

That's all that I have prepared. If there are any questions I will try to take them or I will try and direct you to the appropriate expert.

CHAIRMAN RYAN: If you could go to your backup slide 17.

MR. COMPTON: The mass loading -- mass loading?

CHAIRMAN RYAN: Yes, could you talk a little bit about these values that you've shown for the one year mass loading and the mass loading above soil?

Again they seem to be pretty large mass loadings or hundreds to tens of milligrams per cubic meter and then hundreds to tenths.

MR. COMPTON: Right, these are -- as I had mentioned those were based on some measurements that were taken at Cerro Negro. They are something that we're examining.

Again the -- I would kind of separate these two things. One is that the functional form. You could put a different mass loading into that -- into the equation's there the one -- but then there's the values.

Those particular values have a technical basis but they're being examined.

CHAIRMAN RYAN: They seem -- not for any particular reason other than just to help inform the EPA dust loading at worksites, five milligrams per cubic meter.

MR. COMPTON: Right and then the OSHA limit I think for total dust is fifteen, and for respirable five.

CHAIRMAN RYAN: Five. Well TPA and OSAH release the same thing so --

MR. COMPTON: So -- right we do -- it's very dusty. We do realize that.

CHAIRMAN RYAN: Okay. Well and again, I'm not necessarily picking on that but I just point that out because that is in fact a big driver of dose. The numbers -- the amount of dust load.MR. COMPTON: That's correct, and that's the reason that I --

CHAIRMAN RYAN: Okay.

MR. COMPTON: The equation is so that essentially the peak -- the conditional doses is linear. I don't want to immediately say that the risk is linear, I have to think about it but --

CHAIRMAN RYAN: Let's just deal with dose for the moment.

MR. COMPTON: Okay. But that's correct.

CHAIRMAN RYAN: Okay. Thank you. Any other questions? Ruth?

MEMBER WEINER: Thank you sir. I had numbered the same questions that Dr. Ryan has already asked. But let me expand on them. And maybe Britt Hill will want to answer some of these. You made the statement, Britt, that you can't model fine particulates. Since these atmospheric dispersion models are imprecise at best, what would be the problem with identifying the distributed -- a distribution of fractions of your airborne material that are -- is respirable, and that's sampling on that distribution?

I mean, that take into account the uncertainty, well a lot of uncertainties, in an imprecise way. But it's no less imprecise than the rest of your dispersion model.

MR. HILL: One of the things we've -- again this is Britt Hill, CNWRA. One of the things we've observed in our field measurements is that the abundance of suspendable or respirable finds that are in the mass load doesn't seem to be affected by the abundance of that size fraction in the deposit between limits of total suspendable finds being about two percent of the deposit to 20 percent of the deposit.

The mass load is the same. There are many more fine particulates available for resuspension in the deposit than can be entrained at any on time, in fact, quite a long time after the eruption.

One of the insights we've gained from Cerro Negro is that this deposit had received over five meters of rainfall since its deposition, including two meters during Hurricane Mitch.

You would expect, if washing and windowing was going to be a significant process in this deposit, we would have seen a profound effect when we went out and measured mass loads in the breathing zone above this deposits.

But, instead, we didn't see much at all. What that is telling us is that we can't make a mass load model that's going to be linked directly to the abundance or size distribution of those fine particulates in the deposit because you're suspending many more, or excuse me, have available so many more particles under any realistic distribution in the deposit that can be suspended at any given time, and for some time after the eruption by typical wind turbulence and resuspension processes.

So I don't think going to a discrete tracking of grain sized bins within the deposit is going to gain us any insight on the mass load characteristics above the deposit. We are already saturated with respect to the suspended particles

CHAIRMAN RYAN: I think I would understand your point. But, if you challenge that the activity distribution might vary the particle size, then I think you've got to rethink your thought there.

Let me tell you why I asked that. I've seen you know sealed sources melt in molten steel. A huge fraction of the radioactivity ends up in the steel blob on the center of the steel mill floor.

A very small fraction ends up in a bag

house, and there is a partitioning in the radioactive material. If, in a igneous event -- and God knows I'm not trying to draw a direct analogy between a source melt in steel and an igneous event -- but I just think that exploring, numerically if no other way the assumption that there's a uniform distribution of radioactive material in the mass is a critical issue of uncertainty that you need to not leave untouched.

MR. HILL: I certainly agree that there's a lot of uncertainty in the incorporation mechanisms for spent fuel during these calculations. However, one perspective is that we need to remember this is a trace component in the eruption.

The mass of the intentionally incorporated waste is on a mass basis or volume basis, exceedingly small, compared to the mass of Tephra that's being erupted.

About point one to -- excuse me, point zero one percent at best. So we are looking at the behavior of a dilute phase in the total erupted mass.

CHAIRMAN RYAN: But you're distributing in uniform throughout the mass, are you not?

MR. HILL: The same way that we see wall rock fragments distributed uniformly throughout an eruption deposit.

CHAIRMAN RYAN: I'm sorry.

MR. HILL: It's not coming in at a particular point. It's a uniform incorporation process.

CHAIRMAN RYAN: Well let me ask it a different way. Any ten milligrams of ejector or Tephra material that comes out of this event has an equal probability of having the same amount of radioactive material in it as any other ten milligrams. Is that correct?

MR. HILL: I believe that's correct.

CHAIRMAN RYAN: My challenge to you is, I don't think that's realistically representative, because there's going to be some fraction of this waste package material that's going to end up as incorporated into a smaller fraction at 100 percent for all the mass involved.

MR. HILL: That's always possible.

CHAIRMAN RYAN: But I, and I --

MR. HILL: But, how do you a technical basis to look at the partitioning.

CHAIRMAN RYAN: By exploring numerically is one way. And to see if what I'm suggesting is important or not, but just leaving that assumption unexplored is not so good either.

MR. HILL: We also need then, in a fully realistic model to start looking at the waste form behavior under these hydrous, high-temperature to low-temperature oxidizing and reducing conditions, and then the surface exposure, along with the chemical leeching absorption processes that go on during a typical volcanic eruption.

I agree a fully realistic model would be most satisfying. But there are a wide range of complexities in trying to impress a realistic particle based or chemical based or chemical based tracking in these deposits through time.

CHAIRMAN RYAN: Again, I'm not suggesting to resolve all the chemical and physical questions here. I'm simply saying that exploring what the impacts would be on calculated dose of looking at different distributions of the radioactivity, and fractions of the total mass might be helpful.

MR. HILL: We have been doing that. Dr. Dick Codell.

(Laughter.)

CHAIRMAN RYAN: I saw her behind the --

MR. HILL: Dr. Codell could spend a minute to explain some alternative incorporation models that he's been working on and has presented.

MR. CODELL: I am Dick Codell from NRC staff. Thank you Britt. I have a paper coming out. It should come out in November in Nuclear Technology based on a presentation on waste meeting, where I looked at an alternative conceptual model for fuel incorporation.

The model we have in the TPA code, the Ash plume model, is a simple model that probably is good enough and has one parameter that you can vary. The doses that you can get don't seem to be very sensitive to that parameter.

It's called the incorporation ratio. I'm this alternative model I used a different idea of how spent fuel and ash or tephra might mix. And I went through all the analysis and it does behave somewhat differently then the model we use, but in the end the results weren't more than a factor of two differences in the element dose people might get.

So, in light of all the other uncertainties, we decided that it was not necessary to change the model.

CHAIRMAN RYAN: Okay. Is that available as a preprint at this point?

MR. CODELL: I'm sure I can send you one. Yes.

CHAIRMAN RYAN: Okay that'd be great. Thank you.

MR. CODELL: Thank you.

CHAIRMAN RYAN: Yes. Bill?

MR. MELSON: What we had -- I think that I would like to continue this line of investigation. Clearly if we have some overpressure inside of a steel of other vessel you'll have a fragmentation.

If it's not steel, or whatever and rock, you'll have a fragmentation. The particle size distributions in those two cases I would assert are probably very, very different.

The metal case it'll be large, you know, fragments. In the case of a bit of rock, there'd be a great deal of dust as well as large ones. And so I'm just going to say that, in the modeling of fragmentation process, we had to distinguish the rods from the rock.

And it's going to make a big difference. If you need one micron in size or whatever for respiration, I would assert that, in the initial material, you're not going to have any pieces of the rods of that size.

They're going to be larger. And there will some sorting of this process. Now I know Britt's aware of this, he's thought about it and I believe he mentioned it just now.

I think that's an important consideration will affect doses. I don't think that can be neglected. And I think you're suggestions numerically modeling that effect.

Just explore it. It would be interesting to decide whether you want to really go after the physical model. The other thing in the -- business, you know if you're scaling Yucca Mountain, in the dry -- in the usual dry says you see dust devils going crazy all over the area.

And these are going to be a constant source of disruption of surface and a mixing and a spreading out and a dilution of these materials that is a real thing which can be measured.

This can be addressed in the field by really good experiments by using some contaminate distribution. And that too will change or give some quantitative feeling to what happens to the layer of contaminated ash.

The other thing, are big storms in deserts are the main source of erosion. And it can be catastrophic, as you all know. And they can remove the contaminated layer in a single storm, and do more than remove it.

They can take, you know, big tremendous galling and other things happening. And that effect ought to be considered to in some way. I not --

MR. CODELL: You'll see that in Dr. Hooper's presentation. I'm sure he'll bring that up.

MR. MARSH: Okay. That was about it I think. A little -- Britt's of comment about uniform distribution of generalized inclusions in lava flows or in pyroclastic flows, if they're mixed, true.

But in a lava flow or other things we know they're not necessarily uniform mixed. And, if we consider the digestion, if it could happen -- the canisters as blobs coming out, whether uniformly distributed or not, I would say it is a question you would have to consider more carefully.

And just assume quickly that because we have some one example, three examples, that will always happen.

MR. HILL: Well first, I'm not really concerned about the lava flows or pyroclastic flows. It's only the tephra fall deposits. And, well again, we have done a lot of work in trying to constrain this to the best that we can using analog information from reasonably comparable volcanoes.

Certainly you can -- we have done some exploratory calculations where you allow the fuel to only come out at an early stage of the eruption. Say you get a contamination layer through there, then your assumptions become critical on how that deposit erodes through time.

On the average though, for any single realization, of course that could make a profound difference in dose through time history, if you had a contaminated layer in the deposit versus a uniform deposit.

When you start to run hundreds of realizations, that peak from the contaminated layer really just averages out to a uniform distribution, unless you're going to an extremely short erosion time.

Then again, you don't think it would be truly supportable. So we have done some exploratory analyses to consider potential instantaneous release of all the material into a layer.

But again, we're dealing with a trace amount of mass, so it's not like we're really talking about a visibly contaminated layer. Even under the worst of assumptions.

And it doesn't make a significant

difference to the risk calculation when we're starting to deal with all the range of uncertainty and considering all the potential future states that we have to consider.

CHAIRMAN RYAN: I'm sorry Ruth, you wanted to finish a couple of questions and then --

MEMBER WEINER: I had a couple more. I guess I'm a little lost in the explanations. The point I was trying to get at, which I think Dr. Ryan has articulated very well is, you are dealing with uncertainties.

Isn't this a good -- to simplify my question, isn't this a good place to incorporate uncertainty into your model, and make yourself, for example, a distribution of the fraction of respirable -- of the fraction of your inhaled mass, or of your mass that is respirable instead of treating all of your airborne stuff as respirable?

The other place that I would have looked to incorporate uncertainty is in the partition, after all, plutonium dioxide is very dense. All the actinide oxides are dense.

And, if we can partition uranium in coal dust, in fly ash, between the fly ash and the bomb ash -- which has been done by TPA numerous times and looking at fly ash emissions.

If we can do that, it seems to me you can make some estimates of the partitioning of the actinides between the respirable, non-respirable particles, or even between airborne and deposited.

What is deposited when? Any my final question is, ICRP 72 -- I would yield this to Dr. Eckerman who is the expert in this. But we've been using ICRP 72 for more than a year.

I'm a little bit surprised that your published work still uses 1988 dose conversion factors -- inhalation dose conversion factors. And I wonder if I -- I mean, I'm sure you're updating it now but, the whole notion that we know we have much better data on the chemical form clearance class than is available in FGR 11 for example, in the 1988 version which you site.

So I really wonder that you can incorporate uncertainty into your model, and run -- sample it on a distribution on some of these factors and get a more realistic estimate of what the inhalation dose to the RMEI would be.

MR. HILL: Keith, could you comment on ICRP 30 versus ICRP 72?

MR. COMPTON: Again just very briefly, and this is not going to be a set-aside answer. But I'm presenting what's been published. We do know that there's a difference.

CHAIRMAN RYAN: Just to add for a minute and schedule Dr. Marsh had a question then Dr. Garrick and I think we'll wrap up and Dr. Hooper give his talk with a quick introductory comment by Tim McCartin.

MR. McCARTIN: Yes. Tim McCartin. Just to put a follow up, I mean, currently Federal guidance is to use FGR 11 so that's what we're using.

We recognize that other people are using more newer dosimetry, but that is the current federal guidance.

CHAIRMAN RYAN: But we did cover in our last working group meeting, Tim, that if licensees, for example, request to use them or updated dosimetry they're allowed to do so.

MR. McCARTIN: Yes and the commission has granted that on a case by case basis.

CHAIRMAN RYAN: Right, okay.

MR. McCARTIN: One thing I would like to follow up with respects to TPA code that maybe to give another perspective on what were doing. When we look at volcanoes in nature, they deposit -- they create deposits with mass loads.

The TPA code is trying to represent the persistence of deposits in nature as the geologic record supports. And, in addition, Dr. Hill presented that we've gone out and measured mass loadings above deposits and we incorporate some of that information.

There is a lot of uncertainty. We do vary it. But it's a recognition that it's important to account for the mass loading and its persistence. And we've demonstrated that with our TPA code.

It is for the department to come up with appropriate supporting information for the mass loadings they expect. But what we've done is look at ranges of possibilities.

And I think it's based on what we observe in nature. And one other thing I would like to bring up that the UO_2 -- I know there's a lot above the respirable aspect of that.

But the UO_2 fuel pellets will degrade with time. And larger pieces will become smaller in a relatively short period of time. Like the surface of the earth.

CHAIRMAN RYAN: Thanks Tim. Bruce?

MR. MARSH: Yes, I mean getting back to what Bill Melson mention in terms of how this material gets in there and things, I think it's, you know it's a very critical issue.

However, in doing these kind of calculations, that complex interaction of how much magma's involved with how many canisters, and how the canisters come apart and how the pellets get in and how the panel -- pellets come apart dissolving, and the pellets don't actually -- are pulverized and probably not even involved, because they will come out very soon and it would be in a respiratory sort of range.

However, I mean that's how you have to do the calculation I would assume in here, that Britt Hill's stuff is that you have to assume this stuff is just uniformly dispersed at all PSD particle size distributions through there.

And then you start the calculation after that. However, the important thing that Bill's bringing up I think is this is a critical junction where this is great inhomogeneous process in that you know, you can have the way from a big canister being picked up and just carried along in a local area to one set of partially fragmented and things.

So this is one of these serious areas where there are factors where you really need to know what's going on in detail. But to do a calculation like the Center is doing I think you have to start somewhere with it.

So they start with the universal dispersal of the homogeneous materials.

CHAIRMAN RYAN: Thank you. Dr. Garrick?

MR. GARRICK: Yes. I just wanted to go back, and think a little bit about what the purpose of the calculation is, because it's certainly not to calculate the risk.

Maybe it's to calculate what some of the bounds on the risk are. But I didn't hear much about that. And, when you talk about a calculation that fixes wind speed, fixes wind direction, fixes location, fixes elevation, fixed the radionuclide inventory into the tephra, fixes the resuspension, fixes the uptake, I'm not sure what we learned from doing comprehensive uncertainty analysis about one piece of this, and then fixing as many things as are fixed in this calculation except that we sure as hell don't want to have a release.

And it seems to me that one thing that we need to do here is do a real -- if we stop talking about uncertainty analysis, and I'm a great pusher to that -- is to do a consistency check with respect to the parameters to see if what we're doing with respect to uncertainty has any meeting.

And one of the things that's bothered me about the dose calculation, it's fixed all over the place. Part 63 fixes the uptake, fixes the amount of the inventory into the 3,000 acre field and so forth.

What I'm searching for here is, what can we do here that's really meaningful? If there's any kind of part of the TPA calculation that's hungry for transparency or that's totally opaque, it's the dose calculation.

And I haven't been very reassured here by what has been shown here. And maybe what we need to do is, as I started out, is to say what's the objective of these calculations?

Because it certainly isn't a realistic assessment, even on a conditional basis.

CHAIRMAN RYAN: Thanks for that comment Dr. Garrick. I think as we explore with Dr. Hooper and the other presenters this morning we might get some insights into the various pieces of this.

So perhaps we can keep your suggestion in mind as we move into these other presentations. But I share with you, you know, from the perspective of those calculations, that many other of the elements of it are fixed, and --

MR. GARRICK: Yes.

CHAIRMAN RYAN: And exploring -- is that informing us or are we just kind of calculating with variations in parameters to think we're really assessing variations in risk.

MR. GARRICK: Yes. I think my main point in my question is, what's the context of the calculation?

CHAIRMAN RYAN: Right.

MR. GARRICK: What's the purpose? Is it to get some sense of the bound of the risk or what? Or what is it?

CHAIRMAN RYAN: We are getting a little long in time, so I would -- unless it's a critical comment I'd like to get Dr. Hooper up to give his presentation.

MR. McCARTIN: Okay. Tim McCartin NRC. But I would just say a lot more things are varying than was suggested. I can talk --

CHAIRMAN RYAN: What I would like to do is maybe touch on those as we go through the other parts that I think we'll touch on, Tim, if that's okay.

MR. APTED: Nick Apted, Monitor. Two quick comments. One, Matt Kozak's presentation yesterday almost time limited in terms of the information done.

In that same report we've done a lot of looking at this issue on partitioning, using analogs and so on, in terms of how material will actually -- during melting and a sense in a hot magma partition the activity around.

Again, look at our report there. And, for time reasons, didn't have a chance to touch that. The other thing is, going back, and these excellent presentation one of the slides there are a lot of other assumptions there, and Mr. Garrick is touching on some of that such as constant wind velocity and so on.

Some of that -- that's natural variability just -- natural variability, wind velocity, direction will lead orders in order and orders magnitude reduction.

And I would encourage looking at those sort of natural variabilities in the system.

CHAIRMAN RYAN: Thank you. Thank you very much for a stimulating start to the day. I appreciate it.

MR. COMPTON: Thank you.

CHAIRMAN RYAN: Dr. Hooper, welcome. Thank you for being with us.

(Pause.)

FLUVIAL REMODELIZATION OF TEPHRA ALONG FORTYMILE WASH, YUCCA MOUNTAIN, NEVADA

MR. HOOPER: Okay. I'M John Hooper from the center for Nuclear Waste Regulatory Analyses. And, of course, we've benefited from contributions from John Trapp.

And I'm going to be talking about the fluvial or stream remobilization of Tephra along Fortymile Wash, which is the main drainage system for Yucca Mountain.

And note that is does say first quarter conceptual model, as you already heard over the past two days. I'm going to be presenting work from the first publication.

So there have been ongoing modeling results, new models. And some of that's incorporated in today's talk, but for the most part, other work undergoing. Next slide please.

For a potential volcanic event within the repository footprint, you are going to get some tephra or ash fall around the area, around the Yucca Mountain area.

And, to begin, note that when I'm speaking of tephra or ash fall here it's going to be in the context that we're going to assume that the eruption has interacted with the repositories of any tephra will be contaminated from high level waste.

So whenever I speak of tephra it will be tephra, it will be in that context. And so you have an eruption that goes through the repository and then proceeds to be deposited on the surrounding hills.

And then through time water and rain will erode these deposits, transporting the ash southwards down Fortymile Wash. And, of course, this is going to be moving it closer to the RMEI location.

And then as -- for the last two point there, the last two bullets there, talk a little bit about the dose. Surface winds can entrain fine grain particles and they can be possibly inhaled.

And then long term remobilization will look a little bit at how these are potentially significant to risk calculations. This is a satellite image of the Yucca Mountain region. Note the scale bar down on the bottom.

The potential repository is right in there. And so the Lathrop Wells cone is right in here. And so, if you were to draw a circle with a radius of 18 kilometers from the repository and consider the direction of stream flow down Fortymile

Wash from north to south, the intersection at 18 kilometers between those would be right around this general area here.

And so this is the Fortymile Wash drainage system outlined in yellow. And we're mostly focusing on the southern half of the watershed where the basin.

So about half of the basin is up in here. But as I say we're mostly focusing on this area down in here. And the white lines that you see, those are measurements of tephra thickness.

The superimposed on this image is the results of one possible model scenario, or one possible eruption scenario, or one possible model realization.

So thickness around the repository in this example would be around two meters. So that's a two meter ice pack right in there, and extending outward to center portions.

And so what this does show, though, that out by the RMEI location you'll have a thin deposit of tephra from the initial eruption. So you would get a contribution from the initial tephra fall.

What we also know about this area is that there are low rates of no slope erosion. Rainfall is only about six inches a year, and so you also get low rates of sediment yield.

We have been able to determine that sediment yields from this watershed equal about three to 30 cubic meters per square kilometers per year.

That's a little bit of a mouthful, but that's the typical of measuring sediment yield. You can either use a volume term, or you can use measured times, or even kilograms.

And this system is episodic. It's an ephemeral drainage. It's quite unlike perennial stream flow. And so this also makes this a much more difficult problem.

You're talking about episodic and flood events. And, to give you an example for what the system is like, in 30 years of recording at this area, there have been 11 flood events measures at the southern most stream gauge near highway 95, or the one also closest to the RMEI location.

So only 11 flood events. So only a certain amount of material then is removed. A low amount of material is actually being moved over 30 years.

And then I would talk to you about the analogs. So what's important to keep mind is that erosional characteristics of analog tephra deposits strongly depend on site characteristics.

So Yucca Mountain is quite different for example than examples, and on Mount Saint Helens. But yesterday afternoon the point was made about Mount Saint Helens.

It's not a very good analog for Yucca Mountain, but you can still gain some insights from analogs, even for analogs. So next slide please.

MR. HINZE: Excuse me. Don, is this an example or is Fortymile Wash the only drainage that contacts the RMEI?

MR. HOOPER: Well let's just go back real quick. With the potential repository being right here, the RMEI being down in this area, this is the primary range that would affect the RMEI, yes.

MR. HINZE: Is there any other drainage that could?

MR. HOOPER: There's -- drainages are adjacent to each other so yes, there is another poorly defined drainage off to the west. But this is the main drainage that would affect the RMEI, yes. Okay, next slide.

Okay so I'm going to be talking about remobilization in terms of a concept called a sediment budget. And that was to a mass balance accounting for all the sediment moving through the drainage system.

So allow that to get a monetary budget, a sedimentary budget keeps track of all the sediment within that system. So everything is being accounted for, the tephra, the non-tephra sediments etcetera.

So, a sediment budget, as you can see, there -- a sediment budgeted drainage basin that is a quantitative relationship that links all the sediment sources, the transport processes, stores your remobilization, and they discharge from the basin.

Okay. What we're doing here with the first of our conceptual model, we're looking at simplified mass balance approach using this method to evaluate tephra remobilization following a small volume eruption.

In this case the type of eruption that you see here -- type of eruption. I'm going to do some analog comparisons in just a moment and analog sites, model development.

They include Paricutin volcano in central Mexico which erupted in 1943 -- 1952. The Sunset Crater cone from the San Francisco volcanic field in northern Arizona. It erupted approximately 900 years ago.

And then Cerro Negro volcano down in

Nicaragua, where Dr. Hill and colleagues did some measurements. And then at the bottom there, the Nahal Eshtemoa, an ephemeral ground based stream in the Negev Desert in Israel.

I won't be talking much about that today but this was very important for doing modeling analog work because they did measurements of suspended sediment yield.

And so, that was very important for this type of modeling approach. So, next slide please. So here it Paricutin volcano, a violent strombolian type of eruption.

Rainfall here is quite a bit heavier than what you would get at Yucca Mountain. But all is not lost, it's still a reasonable analog because of the eruption type, nature of the deposit and thorough documentation of subsequent work.

Segerstrom, and others for the USGS -- they did this volcano quite often during eruption in years following. So there's some very good records of subsequent erosion.

And then with Paricutin we did some simple comparison to Cerro Negro in 1995. Next. Here's another view of Paricutin looking out roughly from the base of the cone at the nearby old cone that's now covered with a think deposit of tephra.

And so this was taken by Segerstrom in February 1957, so five years from the end of the eruption, and was very obvious there all the rills and gullies.

So, with that high rainfall, in this setting you're getting extensive rilling and gulling. But, because of the underlying older cone and lava and the relatively impermeable nature of that, that's also leading to the formation of the rills and gullies. Next slide.

Now here's a look at Cerro Negro in 1999, four years after the eruption. This area also has very high rainfall. Once again a deposit several meters thick as you're looking out from the base of the cone.

But note this time you're not seeing rills or gullies. So, the underlying cone then the lavas are relatively permeable. So it's important to keep in mind then that the extent and characteristic of erosion in a complex function specific processes and characterizations. Next please.

On this prop, we have time down here. A 30 year period. So, for Paricutin, this is from 1943 to 1972. And on this axis is the relative sediment yield.

So what's plotted here is on the Segerstrom data. And he calculated the relative sediment yield for the Paricutin area. Now, in a normal pre-eruption sediment yield, you assign that a value of one.

And what Segerstrom recorded was that the sediment yield was about seven times normal two years after the eruption. And then it slowly dropped off, decayed over time.

So after 30 years you're back to a normal, or pre-eruption yield. So with that sediment yield recovered in about 30 years in this well characterized system.

And what you're getting here then is a balance between runoff, filtration, and slope. And this is important because it does show a brief period of accelerated erosion following erosion -- following the eruption.

And that's what you'd expect. You've disrupted the normal fluvial system after an eruption. You've deposited amount of tephra in the system that any rainfall even is going to start moving relatively easily, that material.

So you got a system at equilibrium for a while. So with this in mind we can use this for a simple model for change and sediment yield through time. Next slide please.

Now on this slide we are again looking from 1943 to '72. And then here we have cumulative tephra removed by erosion as a percentage. And then we have area of tephra fall.

That's a fairly large number there but that's based on U.S. Scientist reporting a one millimeter of ash falling in Guadalajara.

And then for these modeling runs for Paricutin, the range of erosion can be between ten and 100 cubic meters per square kilometer per year. And what's presented here is an erosion rate of ten cubic meters per square kilometer per year.

So for this site's analog we expect those erosion values to fall between ten and 100. And so with this in mind, and since at Lathrop Wells we see very little remaining tephra.

So we could ask ourself how long to remove 100 percent of the Paricutin tephra deposit. And using this rank of values you get a number between 2,200 and 12,000 years.

And then you can ask yourself basically that same question for Sunset Crater. Now here we have a cone that's about 900 years old. So it looks a little bit different.

And our precipitation, our rainfall and snowfall, is quite a bit less in this area but still a little bit higher than what you would get at Yucca Mountain.

And now again, asking that same question, you'd get about 14 percent. We can determine that, after 900 years using these values, that 14 percent has been removed.

And, if you use a slower rate of erosion within that range, now that number changes to about 10,000. So you're going from about 1,000 to 10,000 by varying the erosion rate.

So what we're seeing then, analog scale to appropriate order of magnitude. And then we're getting a reasonable variation between analogs. Next slide.

Okay. So where are we going with this model and this work? The sediment yield for Fortymile Wash is between three and 30 cubic meters per square kilometer per year.

And that's a measurement that we could say is for about the last month out of the 10,000 years. And, with these analogs, they show an increase of about one to seven times an increase in sediment yield following an eruption.

So we assume Yucca Mountain would show similar increase following an eruption. And then, with this type of approach, we can quantify these relationships and apply it to what we'd expect to see at Yucca Mountain.

And then at the bottom there, extracting model for mass load at the RMEI. So you're getting contributions from the original tephra fall, contributions from fluvial remobilization, and then also some Aeolian remobilization.

Next slide please. Now to sort of present this as a bit of a flow chart and get summarized to some extent, over on the left sediment sources and we'll start with potential tephra.

And basically what we're seeing here is that over the area of tephra fall the mass -- or, if you want to use volume -- is changing over time. And that's what you'd expect.

Erosion is going to slowly be depleting the deposit and etcetera. And so normal surface process then are going to remove that material, and in this case the surface processes would be -- would include such things as slope wash, drilling, shallow land slides.

And then these would be about one to seven times the ambient sediment yield. And so our ambient system is the normal sediment yield from the system.

And now we're going to assume that the change of mass over time and would be a constant. So that could be applied to that portion of Fortymile Wash drainage system that's not affected by tephra fall.

And so there again same numbers for the ambient sediment yield. And, just like the diagram shows, you're going to get some mixing, some dilution, through the transport process.

And then, for sediment storage, each year you're going to get a certain amount of sediment production, but you're not going to get enough rainfall in almost any fluvial system to remove all that sediment.

So if there is sediment storage. And sediment storage includes fluvial processes, interior -- hill slopes, as well as alluvial deposits and channel fill in parts.

And so this continues. Next slide. We get deposition finally. And in this case we're talking about sediment yield at Fortymile Wash Fluvial band, and basically where the system ends.

And there was the expression for the change in percentage in tephra over time. So, through those processes, the amount of contaminated tephra is going to vary over the years.

And then for mass loading it's going to be some proportion then of that value as well as other contributions. And there you see the change in mass load over time at the reasonably maximum exposed individual, the RMEI.

Next slide please. It is summarized at Paricutin. We see up to a seven times increase in sediment yield after the eruption. And then we see this value dropping off to ambient yields in about 30 years.

In comparison to Cerro Negro, it shows mush lower increase in sediment yield for short periods of time. Other analogs, like Sunset Crater, show substantial tephra deposits can persist for 1,000 years, even with that period of accelerated erosion or sediment yield.

So from this we can get a general understanding that sediment yields increase from about one to several times, for some time after an eruption, but the duration strictly depends on the nature of the deposit, the local substrate characteristics, as well as rainfall, which is a huge driver for erosional processes.

Next slide. To conclude, after 80,000 years, tephra from all cones, meaning Lathrop Wells and the Yucca Mountain region, have been essentially eroded away.

So we need to use an analog approach to evaluate the potential redistribution or remobilization process. Analog volcanoes show an -- a period of accelerated erosion in sediment yield for decades following an eruption.

And then sediment yield from Fortymile Wash is being used to develop a mass balance approach for long term fluvial redistribution. And then work on going through model variations in airborne mass load through time at the RMEI location. Any questions?

CHAIRMAN RYAN: Thank you Don, I appreciate the presentation. Any questions from members? Allen?

MEMBER CROFF: I'm at the end of my roll here. On page eight and nine in you presentation you show a couple of graphs. One decline in erosion and then the cumulative tephra released by erosion.

I was a little bit puzzled. The first graph shows basically the erosion rate turns back to normal in about 30 years.

MR. HOOPER: Right.

MEMBER CROFF: And my first thought was well, gee if most of this stuff is gone and we're back to a lot older rock underneath. But then the next one says that only above three or four percent of it is gone.

How come the remaining tephra behaves so much like the much older rock in the area after that short of a period of time.

MR. HOOPER: Because, after a brief period of time, a drainage system become integrated and, even in material like this it fairly rapidly stabilizes.

So, even though you're still seeing all those rills and everything, that's an integrated drainage system that's becoming fairly stable.

MEMBER CROFF: Okay. And this methodology treats all particle sizes the same?

MR. HOOPER: Yes, for right now it does.

MEMBER CROFF: Okay. I don't --

CHAIRMAN RYAN: Does Ruth have a question?

MEMBER WEINER: In a place like Yucca Mountain, wouldn't the Aeolian remobilization completely dominate the fluvial remobilization? I mean, you really don't have much rainfall. And what would that do to your model?

MR. HOOPER: We are accounting for Aeolian remobilization through a model abstraction. But even in a system like this, even though some periodic, episodic rainfall events, those are still responsible for use -- for transporting a large amount of sediment than Aeolian.

We have this perception that Aeolian moves a massive volume. But, in reality, it really doesn't move as much as people think.

MEMBER WEINER: Can you give me a reference for -- do you have some actual measured evidence for that?

MR. HOOPER: Yes there were some geomorphological studies done decades ago. And what one of them concluded is that the total global Aeolian remobilization was equal to about twice of what the Mississippi moves each year.

So that's global remobilization compared to just one -- a large fluvial system.

MEMBER WEINER: I have a little problem with that. Applying that to -- that kind of macro scale consideration to the relatively micro scale consideration.

And you've used as a basis for your model the volcanoes that are in regions that have ten times the rainfall. And let me just leave it at that.

This I think you need to justify, for me at least, and perhaps I'm wrong in this. You need to justify this more than just on a macro scale.

CHAIRMAN RYAN: Bill Hinze, a question?

MR. HINZE: Well there are many drivers to the distribution and redistribution. Certainly climate variability is one of them, and not precipitation but raised precipitation amounts, wind direction, dust devils, etcetera.

How are you incorporating the amounts of climatic variability that you would expect over a 10,000 year or 100,000 year time period? And I guess the next step for that would be something really involving global climatic change.

MR. HOOPER: Okay. If you wanted to account for climate change, assuming climate would become more humid, you could just adjust parameters to greater rainfall, meaning you would just increase the sediment yields, if, after say 5,000 years it the belief that precipitation would increase, then simply the model encompasses would move to high values.

MR. HINZE: Well, do you have models to incorporate into that? Do you have models to even incorporate climate variability that we would expect with 100 year storms or 100 year overland wash?

Do you have those, and are you incorporating those in the models?

MR. HOOPER: Yes, there are reports that have a predictive nature for future rainfall. And, at this point, the first model -- no I haven't accounted for that yet but it will be easy enough to do.

MR. HINZE: I think Bill will remember there was a large storm about 1992, '93, something like that which had tremendous -- in the Fortymile Wash area.

And there were geomorphologists working on it at the time -- and this just certainly wasn't one condition. Perhaps you can help me with something that I saw in the technical basis report of the Department of Energy and the publication of geology, we can talk about it as you'll understand.

But they show a tephra distribution for Lathrop Wells which extends only a very short distance to the to the south, a very large distance to the north.

This -- if we can extrapolate that, if

that's correct, and can be extrapolated, what does that mean to the distribution of ash from a repository igneous event with deposition in Fortymile Wash?

MR. HOOPER: Well that is an alternative conceptual model. But Lathrop Wells is not correctly a part of Fortymile Wash.

MR. HILL: This is Britt Hill from the Center. What we're going to implement in this model is a different tephra distribution approach than what was done in previous versions of the TPA.

We'll be using a fully realistic wind field. And, for the simulated eruptions, we're using the desert rock data, which is the nearest wind radio sound information that we have for Yucca Mountain.

And for each of the realizations, we're given eruption mass and duration. We'll be calculating the tephra and then sampling a realistic wind field for that realization.

So we will be distributing the model tephra according to the wind field information and only modeling those parts of the deposit that fall within the potential redistribution basin, or Aeolian basin in each of the model simulations.

So we're not taking an analogs approach for kind of a deterministic calculation on where the tephra may or may not be.

MR. HINZE: Is there any reason for us to question what we see in that technical basis note? That's -- I shouldn't ask that.

MR. HILL: I'm afraid I can't answer that.

(Laughter.)

MR. HINZE: I know what the answer is. Britt, you know, you've got desert rock I think it is, meteorological station. That's one point that's relatively short time span.

MR. HILL: It is 30 years.

MR. HINZE: As I say a --

MR. HILL: Very short time span.

MR. HINZE: What kind of input are you getting from your meteorologists, your climatologists, as to how we might use that to look at the total variability that we might expect over even 100 years, even 1,000 years. It worries -- I worry greatly about using this short time span for one observation to constrain these models.

MR. HILL: Certainly that's one of the things that our group is looking at. The global scale wind models that have been developed for various applications.

We're trying -- some of the team is trying to look at relating the model winds for the current condition to the actual data to gain some measure of confidence that the models are simulating a wind field at the scale that we need for tephra modeling.

And then, of course, look at how the models may change with different assumptions of climate change through time. And give us a first sense of, given the climate change scenarios that are being used on other aspects of the performance assessment, would those climate assumptions impart significant or insignificant variations in the wind field at the scales that could affect tephra distribution?

So I can tell you that is an area of numerous ongoing work.

MR. HINZE: Is it possible to get some feeling for what percentage of the redistribution is fluvial overland or Aeolian?

MR. HILL: I'm afraid I can't give you any real hard numbers, but I can just give a perspective that the fluvial basin is really that area that's very close to the potential repository site, or within the first ten -- five to ten kilometers from the volcano.

When you look at the grain size characteristics of deposits around the volcanoes at that distance, you see that they're dominated by non-entrainable fractions.

Most of the deposit if courser than 100 microns. So if we're looking at a normal wind entrainment process, where you can suspend 100 micron and finer particles, you're really only depleting the uppermost layer.

Several millimeters of that deposit can be entrained by turbulent winds suspending it. The bulk of that deposit near the vent is going to remain unaffected by any windborne process.

You would allow -- the finds from the surface layer, and leave a course lag. So I think, as a very geologic perspective, most of the potential mass of these calculated eruptions in these basins is going to be dominated by an invective process of fluvial release, rather than Aeolian transport, just because the grain -- the deposit itself is really too course to have much Aeolian transport in where the bulk of the mass is going to reside.

Of course, when you go farther away, outside that basin, on distances of 20 maybe 30 kilometers, then you're in a much finer grain deposit.

And the windborne transport is going to be much more important than stabilization or fluvial reworking.

CHAIRMAN RYAN: Bill?

MR. HILL: But that's really a small part of the mass.

CHAIRMAN RYAN: Sorry Britt, sorry Britt. I want to ask that, based on time we don't have a break in our schedule. I would like to put one in.

MR. MARSH: I would to.

CHAIRMAN RYAN: Okay. So that being said, it's now quarter of ten. Let's reconvene at ten and we'll continue questions perhaps after the break.

(Whereupon, the above-entitled matter went off the record at 9:45 a.m. and went back on the record at 10:00 a.m.)

CHAIRMAN RYAN: Okay. We'll go back on the record please. Thank you. Are there any -- I wanted to make sure we had a chance for any last questions for our previous speaker before the break, Dr. Hooper.

Hearing none we'll move on to our next three presentations. And the next speaker Dr. Fred Harper, Dr. Lynn Anspaugh, and Dr. Keith Eckerman. So, without further ado, let me ask Dr. Fred Harper of Sandia National Labs to talk to us on his perspective on aerosol model issues.

I think he'll have some interesting information for to see.

MR. HARPER: Is it okay if I talk from over here.

CHAIRMAN RYAN: Just fine I think if everybody can hear it. We can hear okay. Just fine thank you.

MR. HARPER: Can I -- I want it dark.

CHAIRMAN RYAN: Oh, yes. I'm sorry.

PERSPECTIVES ON AEROSOL MODELING ISSUES

MR. HARPER: Those of you that need to sleep can go ahead. We've been doing explosive aerosolization experiments as Sandia for over 20 years and the -- I don't need it that dark.

CHAIRMAN RYAN: Okay.

MR. HARPER: There's a lot of electro-micrographs in this presentation so you need it a little bit dark. But we've been doing this. And our goal is to understand radiological dispersal devices, and improvise nuclear devices.

The non-yield aerosolization characterizations of a nuke, and the radiological dispersal devices, the aerosolization. We've been looking at this for over 2o years.

And done about more than 500 shots. And

since 9/11 everybody's interested. It used to be that even the people paying for this wouldn't come out to visit.

Now there's all kinds of people coming in to see our work. This is not a perfect analog for your problem. I talk in terms of times of micro seconds.

I hear hours and weeks as far as the interaction goes. So we're not talking about a perfect analog here. And as far as the pressures go I speak in terms of giga-pascals and I hear mega-pascals in this.

So hopefully you'll be able to take away something you can use from this presentation. But it's not directly the same sort of events. I can't even spell igneous, so if you please stop and ask questions because you may focus on something that I wouldn't even think of focusing on from here.

So let's run this part a little bit less formal. First I'll show you a couple of things about what problems we're looking at. This is a large cesium chloride mobile food irradiator from China, about 250,000.

So the form of the material is a salt. We've done many cesium chloride shots. These are radio-isotopic thermal generators. They contain strontium titonate, an oxide. Am I standing in anybody's way?

CHAIRMAN RYAN: No your fine thank you.

MR. HARPER: They contain strontium titonate. We're very interested in ceramics, specifically strontium titonate and the actinide oxides.

These are -- these specific ones are about 45,000 -- these are bigger right here, these are smaller. Up to maybe 10,000. And they come encapsulated.

And we've done experiments with encapsulation, without encapsulation. Generally the sources come encapsulated, so we would be remissive if we just did our experiments on the basic material without considering encapsulation.

This is a cobalt pencil from a cobalt irradiator. There's lots of cobalt irradiators around. And this is the size here, those are centimeters.

This is about 1,000 curies worth. Nordien has provided us with several of these, and we blow them up and check for aerosolization, check the aerosolization characteristics.

They of course give us the cobalt in the cobalt 59 form prior to neutron irradiation. We tend to do a shot a week, or two shots a week. And we stayed non-radioactive.

So we're looking at material surrogates, like cobalt 59, like cesium 133, strontium 88. So we're doing it clean. And at some point we attempt to simulate radiation aging by introducing defects into the crystal matrix.

What's important to the aerosolization potential is what form. It's critical what the form is. Metals behave completely different than ceramics, and liquids and powders behave differently as well. In this talk --

MR. HINZE: Can you explain just a bit on that ceramic versus metal?

MR. HARPER: I'll explain a lot.

MR. HINZE: Okay great.

MR. HARPER: That's what the whole talk going to be about.

MR. HINZE: Sorry.

MR. HARPER: I think you're mostly interested in ceramics but I'm going to talk about metals as well. I'm not going to talk about liquids, and I will really only touch on powders.

So I believe that that's your interest. If that's not the case let me know. And the material properties depends on whether you're looking at a ceramic.

Fractured toughness is key for the ceramic, as well as density, speed, and sound, and the thermal properties for ceramics. And for the liquids of course there's a whole different set of properties that are important.

I'm going to drag you through a little bit of shock physics here. I apologize, but this is important for -- because what ends up happening is change of phase is critical to the size that the particles end up.

And what we have, this is really important for metals and for salts. And it's important for ceramics in that they do not change phase. They're in the solid fracture mode.

You start out -- this is pressure volume diagram. Start out down here. Shock up Raleigh line, directly with the straight line. And then relief along the isotope that we have approximated by there.

Not important. What you need to know is when the amount of energy that is left after the shock has come and gone is represented by this shaded area.

Now the size -- the thickness, the size of that shaded area depends on things such as the speed or sound in the material, the bulk modulus, the density of the material, basically shock physics parameters.

This has a very small banana, bismuth has a very large banana, uranium has a medium sized banana, silver has a medium sized banana. It depends on the properties.

You compare the amount of energy left in the material after the shock has come and gone to the thermal properties. How much energy does it take to melt it?

How much energy does it take to sublimate it? And if it -- if you have more energy left then that amount of energy, then you're in that form. Now you're only in a form for less than second.

You know, what I'm talking about are solvent aerosols. But that dictates the size of the aerosol. Oops, went the wrong way. And this is why I'm standing up here doing this by myself.

There it is. Now this a detonation wave traveling through PBX-9404. Pressure is about between 35 and 40 giga-pascals. Is meets a particular flavor of plutonium, spikes up to about 60 giga-pascals.

Now, at this point, if this was TNT or if it was PBX-9501, the spike would have only gone up to about 40 giga-pascals.

So the type of explosive makes a difference. Now this particular flavor of plutonium takes about 34 giga-pascals to melt. So this is just hockey puck on hockey puck geometry.

I can't go into any other geometries in this kind of environment. So, in this case, you melt about a centimeter and a quarter worth of material.

Now, if that was something other than that particular flavor of plutonium, such as cobalt, it would take 208 giga-pascals to melt. If it was bismuth it'd take about 11 giga-pascals.

If it was cesium metal, which you never run into, it'd only take one, one and a half giga-pascals to melt. And I won't give you -- bore you with the sublimation properties.

The point is, there the metal, the explosive, all make a difference in to how much melts or sublimates. Now, let's put this together in kind of an integrated fashion here.

Metals can change phase to vapor, to liquid. If they change phase to vapor -- and remember it goes from vapor and back to solid in about less than a second -- they typically come out less than one micron.

You know, the aerosol will be less than one micron and I have some pictures of them. If they change to liquid, they're always less than 20, in our experiments. You know, velocities will change this number.

If you get higher velocities and you end up with smaller particles. They're usually less than 10 microns. So, if you change phase from metals, you end up in the respirable region, the highly respirable region.

Now, for metals, it turns out that you're either respirable, you've gone through the phase change, if you're in the solid fracture. The solid fracture ends up, and I'll show you some pictures of this.

You basically pick it up off the floor. There's very little in the middle. There's very little in the 30 microns, the 20 microns to 200 micron range.

Either you're either respirable, or you pick it up off the floor. Ceramics, in our experiments, we haven't seen any phase change for the ceramics.

And we're talking about strontium type cerium dioxide and a few other ceramics. We have seen phase change for the salts. For the cesium chlorides, and barium sulfates and other salts that we've looked at.

So, for ceramics, you're stuck with a solid fracture. Now we see peaks several there. There is a peak that is under -- that's about -- it's consistent.

It's always at 2.2 microns, aerodynamic. Now that translates to about one micron. If you go into the literature, you'll find that for -- you know, you find papers from back in the 1800s about a comminution or a grinding limit.

And this is a limit below which it is very hard to get particles smaller than. You can do it with you know like advanced techniques and stuff. But there's a few reasons for that.

One is that, at about that sized, brittle materials start to behave ductile. That's maybe one reason why it happens. Also about that size, cracks can't get any closer than that.

Vanderwhal's forces. There are several reasons. And, below that size, things like to agglomerate back up to that size. So within the grinding and crushing industry, they talk about this comminution limit, comminution limit, I said that wrong, grinding limit.

And we, coincidentally, every ceramic shop we look at we see a peak right there. Theoretically, and we haven't seen this evidence, theoretically there ought to be a peak where the grain boundaries are.

Well we don't -- we aren't as quantitatively good there as we are down here. So we're not positive whether we see this or not. And then there's energy limit and spall.

In a nutshell what we see when we do ceramics is not the same that -- when we see metals. We sort of see a fair amount in that intermediate range. We try to do this to try to study the larger particle size ranges as well.

So there is a lot of material between the 30 micron and 200 micron range. This is where we do our experiments. This is 1,000 cubic meter air supported building. And we do up to a half pound of explosive in there.

We try to do a shot a week in that facility. And this is a smaller facility. And what we try to do here is we put about 30,000 worth of stainless steel on this old generator box.

And we do smaller shots but we are able to vacuum up and pick up the sweepings. We can get the sweepings from the bigger containment as well, but it's trapped up with some concrete.

And what we do inside these buildings is we had -- have a bunch of sampling instruments, laser defraction instruments. This is a cascade impact that does a good job sizing from 25 microns and down below.

We use cyclone separators that look at between 30 and 100 microns. We have other kind of deductive techniques where we actually get information above 100 microns as well.

We're not trying to look at just the respirable because we're after the physics, and then we try to fit response surfaces to extrapolate to materials that we can't do in our containment, such as uranium, plutonium, that sort of thing.

So we're after the full size -- particle size distribution, not just low teen microns which happen in a lot of experiments. This is the result of a bismuth experiment.

I can't talk about geometries in this environment. But this is what it looks like if it's gone through the vapor phase. This material is sublimated.

And this is a one micron reference bar. You can see that the material is much smaller than one micron, from a uniform condensation. But the peak is actually at .7 microns in our cascade impactor.

And what happens, and I'll talk a bit about the conglomeration is that these things stick together because they hang up in the fireball for a matter of a second or two, that's all. And that's enough time that that density of particles to conglomerate to a significantly larger material.

This is what the bismuth looks like. Now right here there is about 70 grams of bismuth in this developing fireball. This is actually before the fireball has stagnated and set up and done the turbulent anything.

You can see at this point it's all in turbulent jets. And there's about 60 grams in the space, that in this case is just a couple or three cubic meters.

And I'll talk a little bit about the fireball when I have a different picture. At that point -- at this point it is -- it has come and gone from the vapor phase.

This is what silver looks like. In the

case of silver, silver takes 80 giga-pascals to melt, so you have to use a technique to get higher pressures for silver than you would for bismuth.

Bismuth you only take 11 giga-pascals to melt. And silver turns out that we have a vapor peak, and a liquid peak. A vapor peak -- that's a one micron reference bar.

Again you see the small snowy kind of appearance. And the, in liquid peak you see nice spheres, isotropic surface energy makes it spherical if you end up in the liquid phase.

And this was less than efficient an aerosolization situation than the bismuth, because of the properties of silver. In this case the geometry was identical to the silver base.

These are the prettiest spheres we get. These are from Tamen. This was a fun experiment because we ended up setting fires all over the mesa out to about half a mile away from our containment, which was, you know, like it's a fabric containment.

We had particles going through the containment, setting as many as 11 independent fires our there. We don't do many -- experiments, so these are sort of special.

But anyway, in this case, the peak -- we didn't see much of a vapor peak at all. It was mostly liquid at about 7.5 microns aerodynamically. So this is showing -- this again was the same exact geometry.

We varied the materials, we varied the geometries, we varied the devices, trying to get a handle on all aspects -- of what's important to aerosolization.

This is an aluminum shot. The aluminum shot is different from the others because it oxidizes. The explosive environment is under -- an under-oxidized environment, so when aluminum meets air on the outside of the fireball, it ends up lighting up.

In this case about 10 percent of the original mass aerosolized. The rest of it ended up in big chunks that we picked off the floor. And these were big chunks, like about an inch.

So they were pretty easy to find. We came close to full retention in this experiment because of that. But what ends up happening is you end up setting up this firewall, it stagnates at a particular size, and the concentration of the aerosol in that is such that for about a second or two you get heavy -- and I'm lumping conglomeration, aggregation, flocculation all together and just calling them a conglomeration, but it's even something different from that.

Because, in this case, you're not talking about slow diffusion processes, you're talking about inertial things. So it's almost more like temperature.

It's a kinetic kind of a sticking. You've got these turbulent eddies going around, and all of these -- of this aerosol material in it. So we get fast conglomeration, something different then what you think about in a nuclear containment where you've got a little more time than a few seconds.

Now I referenced before that -- I mentioned that cobalt takes about 200 giga-pascals, 208 giga-pascals to melt. So what happens what you do cobalt? One of those pellets? You end up with fragments.

You get a very tiny percentage in the respirable region. You do get a little bit that comes out in spheres, showing change of phase, but very, very little -- through cobalt much different than --

MR. HINZE: Excuse me, Fred. Are those abraded? Those particles have been abraded as elements. Have finds been lost from within the process? You've got the chunks, but they seem to be abraded.

MR. HARPER: Yes. There were -- we did measure some finds, but it's less than one percent of the original mass. Much less than one percent. About .1, .2 percent was all we got from this.

And I've got a spall surface from one of these pellets in the future.

MR. HINZE: What's the physics of it?

MR. HARPER: What's the physics of what?

MR. HINZE: Of producing the finds.

MR. HARPER: Actually if, you go through and look at the models, they say there's no way that we get anywhere near that temperature. The physics is that every time we're looked for aerosols we see them.

Whether the models tell us they're there or not. Models often times tell you they're not there. We always -- in the case what I think is going on, I didn't mean to get to this level of detail, but that's the surface from cobalt.

It's a spalled surface. You can see sights there where you can imagine that there was localized energy events that caused enough energy to change phase there on a very small scale.

That, plus the fact that one of the things we do is we string wires close to the blast, and they're copper coated steel wires. And what we do is we capture the aerosol, and I don't have any -- I don't think I have any pictures in this presentation, capture the aerosol and look at the cross-section with an electron microscope.

And we actually found droplets of iron on lower -- you know one of -- the one that was there inches away from the charge failed due to shear, not due to compressive, and it ended up putting droplets of iron on one below.

Now that -- now physics don't predict that either. But, when you have a fracture event, enough energy happens that you have these localized, not-easily modelable kinds of events going on is my take on it.

CHAIRMAN RYAN: Fred, just one quick question. If you go back to your aluminum slide there, it looks -- right there it looks interesting from the standpoint that -- I mean I get the picture in my mind that the energy distribution throughout that system is pretty -- you know, varies quite a lot, based on where you are here in the center core.

I know it always an equal charge and the shape and all that. Is -- how much does the energy distribution from that event vary over the same, what is the magnitude?

MR. HARPER: Across the fireball?

CHAIRMAN RYAN: Yes. That'd be a great place to start.

MR. HARPER: Well, we usually assume its uniform across the fireball but that's an assumption. And that's the assumption that drives us to our buoyant models that give you a feel for that.

What you've got going on there is you certainly have an energy difference on the outside because you've got combustion going on, and so there's --

CHAIRMAN RYAN: Outside to inside more than axially across.

MR. HARPER: And I pictured more turbulent eddies, so that you know there's substantial energy on the inside as well.

CHAIRMAN RYAN: Okay.

MR. HARPER: But it's -- what ends up happening is it stagnates there. You end up growing very quickly to, in our case it about a diameter of about three meters.

And then it just stagnates there for a -- you know we're looking at, you know this is a -- if I kept this going for about lets say several 20's of milliseconds, it wouldn't grow.

It starts to grow at a second and then it grows slowly. So there's much different time scales going on here. But anyway, so for some metals, depending on the properties, that's what they look like.

Now we did lots of ceramic studies as you know. The ceramics are very important to our aerosolization. This is strontium titonate. That up there is high density strontium titonate.

That's lower density strontium titonate. All we did there was we left the higher density in the oven, as we centered it -- centered it long enough.

Now I read in your documentation somebody -- there's some point that says that there's -- what's in there is pressed powders but that's not correct. It's reactive. And that's -- and that is center after the powder has been pressed.

Now these are very interesting shots. We feel we've got the metals nailed, but the ceramics are much more complicated. This is the original powder that went into the strontium titinate before centering.

This is the hardness of the strontium titonate. This is the higher density cerium dioxide. We use cerium dioxide as our actinide dioxide, plutonium oxide, americium dioxides, fluorium oxides.

So we tend to do a lot of cerium dioxide shots, one because the properties are different that the strontium titonate, or we're trying to do our response surface based on properties so sometimes we do shots on things that aren't relevant to any radiological source just to look at the property -- you know the impact of the property variants on the aerosolization potential.

This is high-density stuff. These are -- that's the fracture surface there, that's the fracture surface here. And you can see the grains on the non-fractures surface there, and up toward the top there.

Notice the shape of this. We did that with a hammer, did that with a hammer. You know basic, real simple stuff. That's high-density. This is low-density. This is how -- this is open porosity.

Now, gut feeling, what do you think would aerosolize better, high-density or low-density? I think everybody -- you know these are of a size such that if this came apart it would look like a -- it would be on micron small respirable.

So, the strontium titinate powder was definitely respirable size. It turns out that this has a higher aerosolization potential for strontium titonate.

It very clearly has a density or porosity dependence. For cerium dioxide, there's no density dependence. They behave much differently. That's part of the complication of ceramics.

This is the cerium dioxide, and you can see unlike the nice spherical metal stuff, we've got shapes. We've got grinding shapes in this case.

And if you do see a sphere, that's an aluminum contaminant to the experiment. A little more about the solid fracture failure modes. Compressive wave goes through, followed by a relief wave.

And I've got a little bit of a picture, next slide to show this a little better. And if it's a ductile situation, it spalls. And I've got pictures of that.

Or the compressive wave can be followed by the relief wave, followed by crack propagation. It lifts the cracks apart from each other. That's a different sort of method.

Then there's failure from diviatoric stress. I was happy to hear Britt say diviatoric stress so that I don't have to explain it to everybody, right Britt?

Basically diviatoric stress, and I guess one of the highest places you use that word is in volcanology, the geological -- whatever you guys do. (Laughter.)

So basically it's magic and what it means is it's the non compressive stress, or the non-hydrostatic stress, the bending, the grinding, the -- you know all of this other energy that doesn't do well under mathematics.

But anyway, so we got failure from diviatoric stress. And we consistently as I mentioned observed peak in the same lines -- range that they do when the grinding the stone.

The aerosolization potential, I put that up there to remind me, we are looking at non-radioactive materials here. In an attempt to simulate what would happen if we'd let the strontium titonate age for 30 years, we dealt with proper amounts of zirconium oxide and titanium oxide.

Basically we put some inhomogeneaties in there. We used a mill, got it down very small size and we centered it, and looked at the impact there. Now again, intuition would say radiation aging, that's going to make lots of fluff, that's going to make it come apart easier.

The aerosolization potential, what this

ended up doing, was this ended up reducing the aerosolization potential for strontium titonate down to the low density level, down to where cerium dioxide was.

So it took that high percent -- high aerosolization percent, away. So that was kind of interesting. And part of that explanation is that for brittle fractured crack propagation is a very important phenomenon.

And you can see that cracks would have a really tough time propagating through something that is high inhomogen -- inhomogene -- whatever. And let's see, and then I already mentioned that there's a significant -- unlike metals there's a significant fraction of ceramic particles that come apart.

Now, in an attempt to figure out why strontium titonate behaves so much differently than cerium dioxide, taking a real close look at the actual particles, now these are all in that 2.2 micron peak.

If you look at cerium dioxide, it looks like -- if you -- I don't know you're talking about a steel mill. Did you ever look at the micrographs of the grindings from steel mills?

CHAIRMAN RYAN: No they were much more interested in getting it cleaned up.

MR. HARPER: But if you look at some of the powder metal results, that's starting to look a little bit like things that come out of the mill, things that come out of the grinding process.

Where as this looks like it may be the ceramic failure and contention. We've got a lot more to look at before I can contentitively draw that conclusion.

But if we look -- you know just because of the difference in density dependence, we're looking at drastically different failure modes here. And you can see it in the shape.

These are metals. These are spall surfaces. And unfortunately I didn't bring the varsity picture where I really show a nice spall surface which is from one of those aluminum fragments.

But you can see that the surface is modeled as basically it compressed and pulled apart. And exactly what you think would happen happened. This is time versus pressure on a shock wave

And the compressive part is followed by a relief part, and this is where this and that happen. Who knows where that happens. That's something entirely different.

CHAIRMAN RYAN: But think about the two on the left. Now can we infer that the intention created larger particles? Am I looking at larger conglomerated particles versus those small fragments?

MR. HARPER: No those are from the same stage on --

CHAIRMAN RYAN: On the impact.

MR. HARPER: On the impactor from different experiments.

CHAIRMAN RYAN: I got you.

MR. HARPER: So in that case what we're looking at is we're looking at that 2.2 micron area in the -- stage.

CHAIRMAN RYAN: Got you. Okay.

MR. HARPER: So I homed in on these things so I cut out the reference part. These are similar in size.

CHAIRMAN RYAN: That's fine.

MR. HARPER: And so that's an interesting fact. And I think this is probably obvious by this point, but what matters to ceramics? Properties such as the fracture toughness, the density and speed of sound, and the porosity.

But what also matters is the way that the waves come at you. Is it s shear wave? Is it a compressive wave? Is there any other non-uniformity in the geometry?

That seems to make a different. This is a salt. This is cesium chloride before shocks run through it. Now every other reference bar that I put up there was one micron.

That's ten microns, that's 100 microns. So these powders are big. These powders are big. This is what they look like after you run a shock through the material.

They get smaller. Phase change, here's liquid, and you can see that there's kind of a bubble here at the top. Nice -- they're not nice and round like the other ones. They're kind of clumpy.

There cesium reacts chemically more than the other would. Certainly it's very soluble. There's probably some water involved in that. And let's see, there's also -- I didn't bring it but we get cesium in vapor phase as well.

Now that's what folks at Argon have been doing, some dissection of radiological sources, and this is critical to my extrapolation to real radiological sources.

We need to do the x-ray crystallography here to look at the crystal -- the crystal matrix changes to see how close we are with our conventional strontium titonate and cerium dioxide experiments.

Only reason I put this is you see some chain conglomeration going on. The cerium dioxide powder, when we do powder experiments, if they start out small, they may get bigger in the shock.

There's something called shock conglomeration. You shock -- center shock conglomeration, whatever it is -- it's not conglomeration or centering really.

But if you run high pressures through it you can get bigger particles in the shock wave. You don't have to wait for agglomeration in the fireball.

This is agglomeration in the fireball, however. We can see these nice chain block and things like that. It started out as a pretty highly porous thing -- look's something like that.

And it broke it apart and re-agglomerated it. Now I've been talking little bit about agglomeration. I thought that that might be something that you'd care about.

This is the -- this is -- how many are familiar with roller coaster experiments? So a few, okay. The roller coaster experiments done in the 60's about '62, '63. And they were actual plutonium -- there was one plutonium bearing device for each of the experiments.

And it was -- there's a lot of it that's still classified but this stuff's not. So what they did from there is they did two shots that were of interest to me.

Double tracks, clean slate one. Double tracks is a plutonium bearing device on an eight by eight steel plate. And they put up big balloon curtains and measured, and it was an outside shot, full sized, and measured the aerosol.

And this is the particle size distribution that came out on the double tracks and clean slate one. And ,if you go over here and if you use the thing that's absolutely wrong but say ten percent, below ten percent is respirable.

Go over here, excuse me ten microns. Ten microns you look at about 20 percent of that plutonium device is respirable. This particle size distribution was used in transportation studies, storage studies forever.

However, the -- it turns out that if you do back to the original data and look at it, and this is also stated in the documentation, if I were to look at a volume distributed plutonium activity versus physical diameter, I would follow the above curves.

And it does fine until about ten to 20 microns, at which state -- at which point it goes directly east. And what this means is that up until this point your volume distributed, you might be talking about something having to do with the physics of the actual event, the explosion.

But at this point what you're talking about is you're talking about surface distribution of plutonium, meaning that it added onto the dirt that got into the fireball.

These words are also in the documentation, so it's not just my reanalysis. So in a nutshell, everything up here is dominated by agglomeration on sand that got into the experiment.

Now they were smart, it was a great experiment. They attempted to reduce that by oiling all of the ground around it. They put it on a steel plate, but they still got in there.

And we've also done some outside talk -- tests at the Nevada test site where we've gotten a little bit of that too. So what we do is we try to artificially infuse sand into our experiments.

There's a device going off on top of sand, this one's under sand, and this one's a meter above sand. We're trying to get these, you know a preferential size from that sand into their -- to study the agglomeration effect.

This is a -- this is what bismuth looked like before of course. This is the size distribution, very peaked in the .7 micron size. So this is the effect of the sand on the bismuth size distribution.

You see it's moved over to the right and this is just the cascade impactor. There's a lot of sand in the intermediate range between 30 microns and 100 microns, which also ended up with a fair amount of bismuth.

CHAIRMAN RYAN: On these curves, is it fair to say that when we have sand or other extraneous material that you have a shift upward in particle size?

MR. HARPER: Very definitely. And what's going on is what we've -- we've put the sand up into the turbulent eddies of the fireball and that's where all the agglomeration's going on.

It's not going on five minutes, ten minutes afterward. It's all happening in that fireball. And this is a small grain of sand with this silver agglomerated on it.

So what -- and it works particularly well if you have particles that are of different size because they go different velocities, you know, through -- you can picture it simplification, you know in a gravitational signal.

And one will drop, the others will scatter. But you've got the same kind of thing going when you said turbulent fireballs. And one of the things we do is we developed a method to capture respirable aerosols using aqueous foam.

And basically the same sort of situation's going on there. You know, first of all we reduce a lot of energy out of the fireball in the shock wave. But, in a nutshell, we inject water into that turbulent fireball, and scavenge a lot of the respirable size particles we get.

We get up to 99 percent of the respirable particles. I put this in my -- I don't know. You guys have talked about water, so I put some water in there.

I wanted put this one, this is the last slide. And I thought this might be interesting to you. There were some impact tests done on the fuels for a radio-isotopic thermal generator that had plutonium dioxide in it.

And what's interesting is that ten percent was respirable both for the new -- brand new fuel, and the five year old aged fuel. They didn't wait for 30 years or anything like that but the five year old fuel.

And these are not impacts like what I'm doing, this is a 150 meter per second impact. So it's significant but it's much smaller than my range.

And the general, you know when I'm looking at high-density strontium, I get about ten percent in the respirable range, if I use the magic ten micron number.

And if you magic ten micron number you get a ten percent here for a much lower shock. And the point of this is that the ceramic aerosolization potential is more complicated than just maximum pressure.

You get that diviatoric stuff going on, we've got all kinds of geometry concerns going on. It's more complicated than just matching the pressure, like you can reduce the metal aerosolization.

It's about 20 percent under 30 microns, 30 to 40 percent under 60 microns, and I'm talking aerodynamic here with it. And the initial respirable size is rather small, so the impact did liberate a fair amount of that, even though that is centric stuff as well. Any questions?

MR. GARRICK: I have one.

MR. HARPER: Sure.

MR. GARRICK: I think you said earlier that most of your work was on non-radioactive materials.

MR. HARPER: Right.

MR. GARRICK: So this is somebody else's work?

MR. HARPER: Yes. No, this is a different -- this is not in our facility. This is at Sandia.

MR. GARRICK: Okay. Now the -- an earlier slide you had ramifications with respect to plutonium. Was that this work, or was that --

MR. HARPER: That's the other work.

MR. GARRICK: And is that analytically based or was that experimental?

MR. HARPER: That's experimental and extrapolated using this response surface from there and using the properties of the plutonium.

MR. GARRICK: How much of your experimental work is being done to develop analytical bases for doing -- analyzing these kinds of things?

MR. HARPER: A substantial fraction. What this work is for is so that we can begin a -- in an emergency response situation, the fellows that deployed have this capability to estimate how bad an igneous event might be, to help make a decision on what to do about that.

MR. GARRICK: What I'm getting at is that we have pretty good information on what radiation does to materials. I'm curious, knowing that information, could the analytical models that had come out of your experimental work be applied with considerable confidence to irradiated materials, in your opinion?

MR. HARPER: Yes, that's the hope.

MR. GARRICK: Okay.

MR. HARPER: That's the hope

MR. GARRICK: Okay. Thank you.

CHAIRMAN RYAN: Allen, questions?

MEMBER CROFF: You noted at the outset the rather different pressure and time views that you operate in as opposed to some of those things we're interested in.

Can you say anything about extrapolating what you know down to the areas of interest here?

MR. HARPER: That's your job.

(Laughter.)

MEMBER CROFF: Well it's not my job, personally.

MR. HARPER: Well I believe agglomeration is certainly relevant. And if the EPRI presentations are correct then none of this applicable.

CHAIRMAN RYAN: Just a couple of things along the line, Fred. It seems like you said that you know, in an explosive event -- and I recognize your dealing with three orders of magnitude more pressure and so forth -- all the action following an eruption.

And you have determined that based on the materials, the geometries. But, you have what you have what you have --

MR. HARPER: That is correct.

CHAIRMAN RYAN: -- periods of time. And two that, you know, as you just said, agglomeration for post-energy release. That's a huge deal, and that's material.

And trying to look at all the different pieces, you tended to show that relatively small, let's say less than ten percent or some fraction ten microns down, size range, a number of these different events, and explosive events, and so forth.

MR. HARPER: That is correct.

CHAIRMAN RYAN: Is that a fair assumption?

MR. HARPER: Yes, and particularly for ceramics it's difficult -- it was interesting to me that I would use kind of a novel geometry to maximize the pressure.

And I came up with numbers that were lower than that impact test on the last slide.

CHAIRMAN RYAN: That's interesting.

MR. HARPER: So, having -- we don't get huge numbers out of the ceramic stuff. As I said, the ceramics, unlike the metals are smeared over a rather large particle size range.

CHAIRMAN RYAN: Fascinating information. To me the challenge is exactly what you have put to us, is that, is there a way, or how can physics translate it between real high-energy short durations systems, or perhaps a lower energy longer duration event system.

MR. HARPER: And some times, particularly in the ceramic side, if you put the energy in in multiple hits instead of one big one you get more aerosolization.

It's not obvious that you can say oh, I'm in 1,000 or affected 1,000 below so there's not going to be anything.

CHAIRMAN RYAN: Right. And the other point that I made is that sometimes the models and the actual experimental results don't match. I think that's important thing to keep in mind.

MR. HARPER: No, they never match.

CHAIRMAN RYAN: Okay. Yes?

MS. KEEFER: This is Susan Keefer, University of Illinois. I'd just like to comment that there is a resource you might want to draw in it. It's that in the area of geophysics, we study processes from ranging from laboratory experiment scale like this in order to get pressures comparable to the mantle and core, to large meteorite impact craters which are orders of magnitude slower, to static diamond cell experiments.

Generally the products are very different. But there's quite a rich resource of comparables that you might want to invoke on that.

CHAIRMAN RYAN: Thank you. Maybe we can get some of the staff folks to talk in a little more detail about how to tap that resource. Thank you. Ruth?

MEMBER WEINER: I just have one question Fred. First of all, that was a great presentation. What I want to ask is have you done anything with a target, or something that you explode that mixes metal and ceramic.

In other words if -- a composite fuel run, and what were your results?

MR. HARPER: I can't talk about that. But yes.

MEMBER WEINER: Okay.

MR. HARPER: Want me to talk about at home?

MEMBER WEINER: Yes, we'll talk about it at home.

(Laughter.)

CHAIRMAN RYAN: Yes, questions? Other questions?

MR. LARKINS: If you preheat your sample first, and then did it by energy pump, you do get more aerosol form. There is data on PlO_2 matrix that was done back in the early 80s', the formation of plutonium aerosols.

And if it's preheated you get much more energy transferred so you get much more particle. But they tend to agglomerate very quickly also.

MR. HARPER: Are you talking about the plutonium metal or plutonium oxide?

MR. LARKINS: Plutonium oxide.

MR. HARPER: Okay.

MR. MELSON: I was just going to ask you, most of your phenomena that you're generating are super-liquid. I mean, the ones where you're getting the aerosols are way above the melting point material, is that correct?

MR. HARPER: No. No, there's a solid fracture peak that is down in the liquid -- I mean it's down in a small range as well. That's where that grinding comminution kind of a phenomena that I was alluding to occurs.

Then I ended up going back on that and saying well we might have some of the spall issue going on in the surroundings.

MR. MELSON: What I was trying to get at is distinguishing characteristics between this and volcanic phenomena on earth, explosive phenomena where you're using it on the liquids and looking for parallels with our material.

And maybe Bruce will want to comment on that a little.

MR. MARSH: Yes. The problem here has already been readdressed, is that you're working at giga-pascals, and we're down to certainly the megapascal region.

SO it's a very, very different world. However, it is nice to see this is an extreme, the extreme end of things. So, it would be interesting to do similar kinds of abrasion type experiments, fracture experiments, fracture toughness, etcetera, at more realistic kinds of pressures and temperatures.

You know I doubt we would ever get up to these. There is also the phenomena of course of adding this material to a magmatic composition to see what kinds of effects are involved of these things, and what kind of dissolution you could get over, you know, short time periods, with these materials.

It isn't quite clear where -- how to make the bridge, but it's tantalizing. No pun intended.

CHAIRMAN RYAN: We're going to have a discussion session right after lunch so we can maybe cover all three of our speakers. And I'd like to, if we may, move to Dr. Anspaugh's talk.

PERSPECTIVES ON RESUSPENSION MODELING ISSUES

MR. ANSPAUGH: If I can have the first slide please. Some of you may remember me better from the 34 years I spent at Lawrence Livermoore. In 1997 I did move to the University of Utah.

And two things immediately happened. One was that my black hair immediately turned gray, and the second one was that my concept of publication changed dramatically.

And I'll have more to say about that later. We can go to the next slide. Just an outline of what I hope to talk about is the review of what's known about resuspension, and resuspension models.

I'll make some comments on the DOE and NRC methodology. Unfortunately we haven't heard anything about the DOE methodology today. And then I'll finally mention some areas that would have possible analyses that could be used to improve the accuracy and reduce uncertainty in the models.

The next one please. The first question is, does resuspension really matter? And there's some debate about this point. And we believe it's important for accidental situations such as some of these plutonium 238 thermo-generators coming crashing down on the launching board.

But your real concern there is with very short time frames, because resuspension decreases so rapidly that, if you survive the initial cloud, you can almost forget about resuspension.

As a matter of fact, we'll look at that a little bit more later. The other main case of interest is really for reoccupation of territory that's been contaminated many years ago.

And then the situation is a little bit different. You're talking about areas that were not formerly occupied. And also for the clean up and relief situations, it's important to consider in what situations resuspension can be important.

And basically it's only for those radionuclides that do not cross biological membranes, but they can lodge into the lung where they stay for years at a time.

And usually plutonium is our main concern in the application and why all this resuspension stuff was done in the first place. So the next slide.

This is just to remind you what I looked like when I had funding to do resuspension. I still had some hair and a few less pounds. But this is -- I should comment that resuspension is not easy to measure.

And you can see that we designed this gigantic sampler. It sucked in 1,000 cubic meters per hour. And the reason we designed such a sampler was that we wanted to be able to look at resuspension during a time period when there were stable meteorological conditions.

And this, by the way, is on the Nevada test site at the area called GMX where there was a fairly insubstantial amount of plutonium dispersed many years ago.

The next few graph -- slide indicates the other extreme, perhaps. This is taken on one of the islands where resuspension was also an issue. The measurements here are not so interesting because the environment, and also because it was done so many years after the contaminating event.

But, nevertheless, there are measurements of this kind available. The next one, these are Australian aborigines. And I just thought to mention that the individuals' contact with the environment does have an impact on whether or not resuspension is an important pathway.

Now after World War Three is over we may know this as the Las Vegas lifestyle. The next slide please. And also we are concerned about agricultural crops and contamination.

And this is just one slide to remind us that disturbance does make an impact, such as animals stir up material. Of more concern are agricultural implements and so forth.

So resuspension is a complex process that -- it's not so easy to analyze. The next slide. So our past interest on resuspension has really been at very early time in consideration of a need for very rapid evacuation.

And then at late times the consideration of possible reoccupation. So the two situations are different in terms of appropriate models, and what the DOE and NRC are now doing is combining the two situations with a new type of model which we will talk about a little bit more.

So the present approach that's being used is different than what has been used in the past. Next slide. I just want to point out that much of the material I'm going to present has been published recently in this article in the Health Physics of May 2002.

The next slide. Looking at the importance of resuspension, given that IAC is integrated air activity during the initial cloud passage, the deposition we can describe as multiplying that integrated air activity by the deposition velocity.

And then we can look at the re-suspended air activity, which is the deposition times a time dependent resuspension factor. So if we perform these integrals with some kind of a reasonable assumption of what S_f looks like.

We have a ratio of integrated activities that's equal to about one. And this is important because, if you're not concerned about the initial cloud passage, and you didn't think it was important enough to evacuate the person, then certainly and not all that important to worry about resuspension after the fact, because, unless you do some very rapid evacuation, and I'm talking about days, you basically can't stop the exposure to re-suspended air activity.

And I think that's an important point which hasn't been emphasized here. That is the initial cloud package -- passage was not important then resuspension is not going to be important either.

The next slide. Well there are several types of resuspension models. I mentioned resuspension factor, which is a very simple concept that, as far as I know, is invented by Wright Langdon of Los Alamos in 1956.

And it's simply a measure of the concentration divided by the deposition. And there's also a resuspension rate, which is a fraction of the deposition re-suspended per unit time.

This one has not been very popular because there extremely few measurements that are pertinent. And then finally we have a mass loading model, which is the concentration in the air is equal to the concentration in the soil times the airborne concentration, usually in terms of micrograms per cubic meter, and then multiplied by an enhancement factor.

It has frequently been observed that the concentration in re-suspended material is higher than it is in the material in the soil. And again this is largely due to fractionation effects, like that Fred talked about with the activity being associated with the surface of the particles and most of the surface is always on small particles.

The next slide. So the resuspension factor has typically been applied at very early times, and the mass loading approach as typically been used at late times.

And another point here is that, if we're really concerned about resuspension at late times, it's much more reliable to go out and measure it, for example, if you're two years after the event, it's much more reliable to simply go out and measure it, and not have to worry about all these models.

The next slide. There have been several times -- types of resuspension factor models proposed. The first one here is really due to Wright Langdon.

The second is a powered function which came from the same roller coaster experiments that Fred talked about. That one is the power function. And something that I actually introduced many years ago was a more complex function where the resuspension decreased as a factor of the square root of the time, plus the constant value, because there was some residual activity.

And later on there was different variations of this. And I'm going to show you an example now of this two component exponential model, plus the final factor.

If we could have the next slide, this shows some early models of resuspension. The one on the extreme left here is due to Wright Langdon, from experiments performed in Project 56, which is a plutonium dispersal device at the test site.

These data, unfortunately, were never published in an unclassified form. But this model was published by -- William. The next one to the right is a formulation that was put forward by Ron Catherine, again based in the old data.

Now what we did in 1970 some was to go out some 15 years after an event. And, according to these models, they should be way down here some place, but the surprise was there was still a substantial amount of resuspension occurring many years after the event.

So this function with the square root of time is something that we formulated just to try and describe some initial high levels, and some initial -- and some later observations.

Then we added this constant factor because at that point we didn't know if it was going to continue to go this way or go that way. The next slide.

And one of the interesting things is that we've seen many, many papers reviewing resuspension but there's hardly any data that's been accumulated on resuspension.

And we go back to the data that was measures many, many years ago. And we did get some new data out of Chernobyl, but it was unfortunate that the Chernobyl data -- the experiments did not get organized at early times.

And so what we have is something that is pertinent to perhaps a year after the contaminating event, which is not very interesting because all the action is mostly over with.

And it is dangerous to apply late time models to early times. Now we have in some recent time accumulated some additional data that can be looked at in terms of resuspension by going through a process of some secondary derivation of what the deposition had to be.

Frequently is occurs that we have no direct measurements of deposition but we do have measurements of external gamma exposure rate. And if you know what the radionuclide mix is, you can in fact determine what the deposition was.

So I'm going to show you a lot of data that is not published that we have recently looked at in terms of trying to acquire more data that could be examined in terms of resuspension.

The next slide. We're going to look at this combined data, and I draw your attention to a few factors here. One is the very rapid decline with time, and also that there is a very large amount of scatter in the data.

But most of this can be explained by looking at the data as a function of time. And also some data sets we'll see decline more rapidly with time than others, and this is due to large deposits of mass which may or may not occur from a volcanic eruption.

So the next slide looks at this data. And you see that we have this very rapid decline with time, with many different measurements showing this effect.

Now many of these measurement points here were actually made at the test site. And many of these later measurements were resulting from measurements following Chernobyl.

And you see we do have a subset here, which declines extremely rapidly with time and stays slow. These are results that we measured following Project Schooner, which was a massive cratering event at the test site, and where we had samplers close in, and the deposition was quite large.

This re-suspended activity divided by the deposit decreased extremely rapidly as opposed to the bulk of measurements where the deposition was not so heavy.

So this does get to something that's discussed in the DOE model about the critical thickness from which resuspension does occur. Now nobody really knows what that critical thickness is in terms of any kind of mechanistic model.

And -- but we can get some idea about it from empirical data. The next slide is similar data, and it shows some different kind of models. This is the resuspension model that I published in 1973.

And you can see that it's extremely conservative in terms of the bulk of data that we now have since then. We designed this to be conservative because we didn't have hardly any data at that point.

This one is a model that was in the LMFBR environmental impact statement. This is the Wash 1400 data. This is a data from England, which is a power function.

And you can see from here that the power function actually gave a pretty good representation here. The next slide looks at a shorter time period now.

We're only out 100 days. And I forgot to emphasize this but these slides are in terms of days, they're not in terms of years.

And so again, I emphasize that this action is all over with in a few days in terms of the high level exposures. And you can see here that this power function, although it looked good on the other slide, it's underestimating the resuspension at these moderate times.

The next slide. What we did was that we took a look at this data and tried to derive a new function, which is shown here, it's a resuspension factor. It starts out at ten to the minus five. It decays with a half-life of about ten days.

And then it comes down to a fairly low level, and has a stabilizing value of about ten to the minus ninth. And in order to attempt to describe this uncertainty, it's shown here as the multiplying factor by ten to the plus or minus one power, which I think is a fairly accurate description of the extreme variation in the data I just showed you.

The next slide indicates, again, that the uncertainty is high for resuspension at any one point in time. Now, I think of broader interest here is the resuspension integrated over a year, for example.

And, of course, if you integrated a very uncertain function over a long period of time, you would come out with some factor that is much less than a factor of ten.

The next slide, I just threw that in here to remind you that not all soils are the same. A good question is what does resuspension look like in something like this.

This, again, comes from the Nevada Test Site, by the way. And, of course, with all these cracks you have more opportunity for material to fall in the cracks, be covered up.

On the other hand, you can see that this material dries out, flakes, and it may actually have more resuspendable stuff. But, without doing the experiment, I don't think there's any model that's going to tell you the results.

The next slide. So the resuspension models are to remind you that the useful models are all based on empirical data. The theoretical models, trying to do resuspension, have been a complete bust.

And there are -- some of may know, there's an entire book published with the title of The Physics of Blowing Sand Dunes. Well, the Physics of Blowing Sand Dunes does not describe resuspension.

And, the physics of soil erosion does not describe resuspension either. So we're kind of stuck here where theoretical models are not sophisticated enough to describe these very complicated processes.

And, for better or worse, we are stuck with the empirical data. The next slide -- I wanted to make some general comments about particle sizes. You know, it's my opinion, which I've looked at a lot of different data from disruptive events.

And I think it's very clear that there's always a mixture of particle sizes. And, any time you have large particles, you've also got a lot of small particles there.

And also, it's more complex than that in that, if you throw a bucket of sand in the air, it does not come down to earth as a bunch of very discrete particles.

And I might remind you that we all did this experiment when we were two years old. But, sometimes we forget the result. If you throw a bucket of sand it more or less comes down as a bucket of sand, not as what you would describe as the aerodynamic behavior of the individual particles in that bucket of sand.

So, gravitational attraction among particles is very important. And we need to remember that. Next slide, please. In terms of human intake, you know, we can describe all these wonderful distributions all we want.

But we need to remember that the only thing that really matters in terms of the dose, is the mean. Next slide, please. I wanted to have a few comments about the DOE and NRC models, and also publications.

I did do a search on pubmed, which, as you know, is oriented -- it's biased toward biological type publications. And I wanted to see what I could get with Yucca Mountain put in there.

And I did get 64 hits. But, unfortunately, only two of these publications dealt with dose assessment, one of them with the NRC model.

But, it did not describe the model itself, on some results. And the only one was a paper by Dave Wholer and Mike Ryan, which basically was attacking the dose assessment in terms of -- 129.

So, obviously most of the articles are associated with geology and hydrology. So I guess I ask a question, do the geologists and hydrologists have all the money and/or all the motivation?

And the second question is, hopefully we can look forward to this situation being rectified, because we heard a description this morning of how we were only going to be presented with data that had been published.

Well, publication in an academic case evidently means something very different than publication to DOE or NRC, because these publications -- as near as I can tell -- have not been peer reviewed or published in appropriate journals.

Next slide. The other thing is the RMEI. I understand you got some strange things in the CFR this, that and the other. But, you know, if the RMEI was really going to be exposed, we could take her out of there.

And, I don't think there's any particular point in worrying about the RMEI. What we aught to be worrying about is the eclectic dose to persons in Las Vegas.

And I think, if push comes to shove, ultimately, that's going to be far more important than a single RMEI that's located in some peculiar position south of Yucca Mountain.

The next slide, the DOE resuspension model, I have a hard time finding it. It is on the website, the Yucca Mountain website. There is a publication that's devoted almost entirely to resuspension with the title Inhalation Exposure Input Parameters to the Biosphere Model.

So, it took me a while to find it. The approach in that document, it is unique, and it is non-traditional. It is a time-dependent mass loading approach, which is not what has been widely published or widely used.

And it is based upon mass loading observed following volcanic eruptions, and it depends heavily on the Mount Saint Helens experience, which I think is -- the paper does make a pretty convincing case that this is an appropriate analysis.

And so I was quite impresses with it. However, it has not been published in the peer reviewed literature. And I think members of the Committee should lean on this people pretty hard to publish this stuff.

Sooner or later in a legal arena this is going to be important. If you look at challenges, the question is, is it a traditional approach? Is it the same approach your peer would use?

Has it been peer reviewed? Has it been published? So, you know, this is not just an academic viewpoint. It's also, I think, important for the credibility, for the legal liability of the analysis.

The next slide, I mentioned I think the model is reasonable and appears to be well founded for time soon after their position. It's not totally clear that the model will describe accurately resuspension over long time periods.

But, I won't say that it doesn't. But it's not totally clear. And I would suggest that the model really should be validated against some radionuclide data, which would a more sensitive indicator of the potential long-term problem.

The next slide. The NRC resuspension model, even though NRC invited me here and sent me a bunch of literature, I don't think I ever received that kind of literature.

And I did not know that this model existed until yesterday. And, at this time, the only thing I have is the handout you all have from Keith Compton et al.

And my comment is that that model appears to be exceptionally considerably -- it has very high mass loading values of an average over the first year, about 33 milligrams per cubic meter.

That's certainly not what was seen following Mount Saint Helens. And so, if those kinds of data were actually observed at some other volcano, I think it's seriously questionable whether or not that is appropriate for Yucca Mountain.

And also, there is an extremely slow reduction of time. It has a half-life of ten years. And you remember that my opinion of half-life at ten days is a lot more likely to describe the true situation.

The next slide, this is just some ideas that might be used to improve accuracy and reduce uncertainty. I think the time sets of data on mass loading for Mount Saint Helens are really truly interesting.

And I must say that, until I read the DOE report, I didn't know they existed. I think some more detailed analysis of that would be very helpful, along with consideration of other datasets to determine what is termed the critical depth, that is that depth from which resuspension really occurs.

It's obvious in one extreme that, if we have a meter of deposit, you're not going to get the resuspension from the complete meter. And, on the other hand, we don't know what that depth really is in terms of any kind of theory.

And so, all we can do is look at the empirical data. Another thing that was striking about the DOE report was how different the mass loading levels are in Spokane, versus other locations.

And frequently we attacked because people say well, you measured resuspension up in the desert, and everybody knows that the mass loading in the desert is terribly high.

Well, it's not true. The mass loading in the desert is very low. And, if you look at a large city, like Spokane -- it's not really that big -- but, the mass loading in Spokane is about ten times higher than it is in the other locations.

So, I think there should be some other consideration in looking at background levels as a function of land use. Some of that has been done certainly in terms of agriculture, plowing, and so forth.

But I think it is another area that could be looked at. I'm really surprised the modeling concept that the DOE and NRC models is virtually the same.

But the parameterization is totally different by orders of magnitude. And so, at some point in time, the DOE and NRC model really aught to be reconciled.

Both of them really aught to be validated against radionuclide data. And I'll just close with one other concept about validation. One of the things that's disturbing about other parts of the DOE model is that they talk about validation about what appears to be on black box against another set of black boxes.

I would submit that validating one black box against another black box is not a good idea. We really should be validating against any penetration -- thank you very much.

CHAIRMAN RYAN: Thank you Dr. Anspaugh. I appreciate your comments and your insights. It's very helpful. Any questions?

MEMBER WEINER: I don't really have any questions. I just want to thank you for a very good presentation, and especially for your last point about validation of one black box against another black box.

And I think we need to separate the fact that different models give us similar answers from models that are validated against them. And that's it.

CHAIRMAN RYAN: Lynn, I'd like to pick up on your suggestion about the radionuclide data. You know, you heard their conversation about radioactivity distribution across particle sizes and across a stable part of the aerosol.

Could you give us any insight as to your experiments that you've done, have been involved in, and how that distribution occurs? Is it uniform, is it non-uniform in terms of the radioactivity, distribution, and the mass.

MR. ANSPAUGH: Well, most of what we've looked at, I've already made some comments about it. But, most of the events that we've looked at have been ones that really volatilize the radionuclides.

And, when the radionuclides typically condensed, they really condensed to the surface because the surface area is always on small particles.

You know, I hesitate to say anything about what this particular kind of situation might look like during a volcanic eruption. But I think, in any situation where you have volatilization in place, and condensation, the activity always goes to where the surface area is.

CHAIRMAN RYAN: If there's a separate process for the radioactivity, because they behave independently of the vast --

MR. ANSPAUGH: I think the question is, does it get volatilized?

CHAIRMAN RYAN: Right. Thanks. Any other questions, comments?

(No response.)

CHAIRMAN RYAN: Thanks, we appreciate it. Last up, before our break for lunch, Dr. Keith Eckerman is going to talk a bit about Perspectives on Dose Modeling Issues.

PERSPECTIVES ON DOSE MODELING ISSUES

MR. ECKERMAN: Dosimetry is frequently the last thought -- often to as many folks still in the audience at that time. What I'd like to do is go back and talk a bit about the -- and focus just on inhalation dose modeling, because that seems to be the dominant pathway of concern here.

So, can I have the next slide? When you

look at dosimetry systems, there's really only two gains that you can deal with, and that's the MERD system and the Society of Nuclear Medicine, which really isn't applicable to our considerations here because it normally limits itself to dealing with low LED radiation.

So, you have to look at the system that was set out by the International Commission on Radiological Protection. And you need to, of course, keep in mind that that system is getting -- is a mature system principally set out for protection of warheads.

And, of course, in that case, it has to be applicable to all radionuclides, all types of radiations that might be emitted by those radionuclides.

And this was principally the sole domain of that Commission's considerations -- was workers until the Chernobyl accident. And then it became clear that we had to expand that considerably, things in more general framework.

Next slide. And there has been, with the Chernobyl event, a sort of change in the culture of ICRP. It isn't always evident, especially to a broad set of audiences.

But, the focus is to provide realistic dose coefficients. This is at least the object one is working towards. And I think this is evident in a lot of the recent work that has been in the post-Chernobyl period.

For example, relying a lot more on physiological based modeling. That doesn't mean that we're totally mechanistic in the modeling approach.

But certainly we recognize, as we had to, in addressing age and other gender aspects of dosimetry, that there was a rich body of useful information under that disguise of being physiology.

The purpose has been driven more and more to considerations of health risk. The intent also is to provide meaningful doses to tissues at risk. And so, we're not interested in absorbed dose in an abstract, but actually dealing with what are the tissues and what might be the health risk associated with this.

Despite all of these objections or approaches, we still deal with a set of reference individuals that we address in the dosimetry. So, this is ICRP's current approach.

And the realization of this is not always evident. And so I'm going saga, if you will. Next slide. So, I'm going back. As you saw, the dosimetry, only enters into this whole scheme of things by a number.

Nobody talks to us until they want to have a number to put into a calculation. And, the ICRP 30 and Federal Guidance 11 documents, those are basically models that are three decades old.

Even if it might seem later publication dates, Federal Guidance 11 is later. But it took the U.S. that long to get around to recognizing it. So, this really relies back on things that were set by 1975 in the publication process, the open literature.

If I was running the slides myself, there would be a big X on this. Don't use that. That's not a date. You should bury it, forget it. I've been trying to do that after the last time.

The contemporary data, is ICRP 6872. ICRP has another numbering convention, so the numbers go up faster than the information is really available to you.

Seventy-two is the -- 68 is the worker, 732 is the member of the public. Those are the current documents. There is an extension of that in Federal Guidance 13, which deals with risk as a prime unit.

And so it goes to risk for unit intake of activity. But that's the document you ought to really be considering, especially you'll never get anything -- as Lynn Anspaugh mentioned, getting things into the open literature.

You're not going to get reviewers to bide off on journal publications if you're still working with publication 30. I mean, no reviewer would accept that as an open literature publication.

There's a lot of information that's available to you, numerical data, on dose coefficients, databases, on CDs. Federal -- the ICRP has the data for workers and the public on a CD that they sell.

And you can find a freebie from the EPA on Federal Guidance 13 which does -- although the thrust of that document as printed was risk coefficients, the underlying doses coefficient, they are -- at least for one particle size, they reconsider.

Next slide. Now, it just shows you that information from ICRP's database for workers, public, pick the age you like, pick the intake in inhalation route, particle size.

There are ten sizes there. And that information is readily available from that document.

Next slide. All of this is buried in a set of computational models, of course, because we can't measure doses in organs.

It has to be computed and you have a set of models that deal with the route of intake, whether it is through ingestion through the gastro-intestinal tract, inhalation through the respiratory tract.

Once the intake has occurred, there can be an uptake of the radionuclide from those routes of intake into blood, into the systemic tissues of the body, and finally there's routes of elimination and primary urinary excretion, and fecal excretion by which the radionuclides are eliminated from the body, in addition to radioactive decay.

And then, folded on top of all that, there's going to have to be some consideration of some dosimetric model. And so, this is the framework in which these coefficients are developed.

Next slide. So, going back now, we're going to put the focus back onto the inhalation considerations. The respiratory tract model and update post-ICRP 30 -- so ICRP publication 66.

And these are some of the features of that model. And the intent was to provide a realistic simulation of intake, make the model applicable to particles, gases, and vapors.

In the past we have had two different models that we've dealt with. A common model that should be applicable to workers of the public, calculate biologically meaningful doses in the lung, and provide the route of uptake into the blood, into systemic tissues, it ought to be, or was designed to be applicable to this process of setting protection standards.

That is prospective applications and, of course, interpretation of actual exposures in a retrospective sense. Next slide. And so, here are some of the guideposts along that way.

And I'm doing this to show you that there's been a change in some of ICRP's approaches. But what had to be considered was the effect of respiratory model.

You have to deal with the lung physiology, nature of the exposures, breathing rates, frequency of breath, and so forth, deposition of the particles in the airways of the lung.

How are they cleared and removed from the airways? And all of this, of course, will be useful for just calculating the dose of the lung itself. The, of course, there's the process of absorption of the material from the lung to blood, the dosimetry, and finally there's a recognition, of course, that there's a lot of different tissues in the lung, and there are different cell populations that are taken to be addressed in doing a meaningful respiratory dose.

So these are some of the issues that were examined in that effort. Next slide. And so, eventually you have to superimpose, of course, any compartment kind of model, you see the square boxes, on the anatomy, to deal with these tissues.

One of the new features of that 66 model is the extent at which the extra thoracic airways were considered. The earlier model, if you like, the individual was strictly a mouth breather.

And, of course, it was based on actual aerosol inhalation experiments in which the person had the device in his mouth and breathing. It has now been extended beyond in theory, and some very sophisticated calculation of what is the processes in the depositions if the individual is in fact a nose breather.

So, for the first time, the airways outside the thorax are being considered. And, as you can imagine, this is going to be very important with regard to the influence of particle sizes.

And, of course, you move down the tracheal-bronchial tree and get down to the gas exchange region. Next slide takes away the anatomy and just shows you what this looks like in a compartment kind of model.

And the bold arrows are places where the material is considered to be deposited. And the thin arrows are the routes at which material is being removed mechanically.

So, those arrows deal with getting the material, writing up the mucous escalator, and being cleared into the GI tract. There are two rates at which this occurs, so that is the reason for the second block.

There is also a biological removal by macrophages that are shown here, which the material may be removed to the lymphatic system and basically sequestered out of the system.

Then, in addition to -- so, those processes are entirely dependent on the physical size of the particles, how much is being deposited, those black arrows.

And that's a mechanical clearance. That model separates then and the absorption process. So, if you like going into the wall from these boxes, there's another set of transfers by which the radionuclide escapes from the particle and is absorbed in blood.

The next slide. I want to say just a little bit about the aerosol considerations. When we tabulate numbers, we tabulate numbers assuming a log-normal distribution of the particles.

And so, that distribution -- above one micron physical size, that distribution has a GSD of two and a half. So that's the spread of the distribution.

The standard tables that you see published assume that the density is three grams per centimeter cubed. And we throw in a shape factor for the settling velocity of one and a half in terms of a drag on the particles.

So, there is an underlying of the table data. There is an underlying assumption of these kind of parameters with respect to the distribution of the particle sizes within the aerosol.

On that CD that I mentioned earlier, there are ten sizes of going all the way from .001 micron AMAD which, at that range, is really thermal dynamic processes, it's the fusion that's governing the deposition, up to the ten micron size that has been calculated.

The modeling assumes that the radionuclide is volume distributed. That's just the way the numbers have been calculated. The model can be -- you can easily consider a surface deposition on particle -- on the surface are of particle, as Dr. Anspaugh related earlier.

But, the tabulated data make this assumption. And, underlying that is the -- if you have the mono-dispersed information that is in the 66 document.

It goes out to 100 micron aerodynamic diameter. And, when we speak of these things, we characterize them for the log-normal distribution in terms of the median of the distribution.

There is no -- in that model structure, and all the information is there to deal with any size aerosol distribution that you might want to assume.

You're not locked. We only locked you into the log-normal assumption when we had to implement the model and tabulate those coefficients. But, there's a broader capability available in the model.

Next slide. There's a -- these are just the mechanism of the deposition. They're really governing what happens. At the top three are really what the -- depends on what the diameter of the particle and gravitational settling.

So, this -- and impacts and inertia, things this -- is applicable to the larger particles. When you go down to the fine, very small, under a micron, then you start running into this diffusion process as being a means by which the aerosol or the particle winds up being deposited in the airways of the lung.

And finally, of course, there is some consideration of electrostatic considerations. And one worries about agglomeration and so forth even in the lung within that humid environment of the air spaces.

Next slide. So, basically what is done is, the lung is viewed as a series of filters. And so, on the intake, depending on what -- you're breathing through the nasal passages or oral.

The air comes in, goes on down to the distal alveolar region where the gas exchange occurs. There's a holding at that time. And then you exhale and particles go back up.

And so, the net deposition in the lung is calculated, depends on this scenario. Breathing rate is going to be important, frequency of breaths, and of course, the size of the particles, because, as you'd expect, the large particles are going to get filtered out.

And, when you get deeper into the lung, you'll have a finer distribution of particle sizes. The important thing at the large sizes is, again, these aerodynamic processes that deal with the weight of the particle, inertia and gravity.

And the fines are, of course, going to be governed by the thermodynamic diameters with regard to the fusion. Next slide. So, this is just a quick way to show you what happens.

I left the data all in terms of AMAD. When you're down at this size, this is probably 00306 thermal dynamic diameters for that small one. Deposition in the -- this is an adult member of the public.

So, he has a time budget that he's allowed to sleep. And, unlike that worker that was in ICRP 30, he didn't work there -- he didn't sleep during his work shift.

This guy has a time budget associated with it. In the ET region you have very fine particles. The tortuous path of going through the nose in that structure, which is there to addition the air and so forth, results in things diffusing into the wall.

You get a high deposition as the particles now gain a little bit of mobility to stay with the airstream. The deposition drops. And then, when you go back up to larger sizes, they are going to be captures in this ET one region.

The deposition in the thorax is this other curve. And, of course, if they are filtered out above, they're not going to get deep into the lung. And this will, of course, strongly influence what's available for deposition or subsequent dose to the lung tissue, as well as systemic uptake.

So, particle size can be a critical parameter. Next slide. Now the modeling that's been done is through -- considers the absorption of blood as a two stage process.

That is, you've got to the particle, the activity, is really viewed as being carried along by the particle. And, it may be a minor constituent of the particle.

And so, to get the radionuclide in a state in which it can be taken out by the blood, you first have to get it away from the particle. And so, there's a dissolution step that's considered.

And, once it has escaped the particle, then it is available for uptake to the blood. So this is where the chemistry starts in. And it's not just the chemistry of the radionuclide, it's the chemistry -- the chemical form of the particles that were inhaled themselves.

This process is viewed as being in competition with the mechanical clearance that I showed you earlier and the biological clearance. And the model was actually formulated with the expectation that this process of absorption, we might be able to represent it by some set of functions.

The next slide shows you a couple. So, this is the sort of picture that's now being used. The particles are deposited in an initial state. Some of them are absorbed rather quickly through blood.

The activity is absorbed rather quickly from the particle and is available for uptake in blood. There is a consideration, if you like, of a transition to a transform state.

And then a little later this material will appear as being available to the blood. This process has been implemented with the fault absorption parameters now, F, M, and S.

This is not the old clearance class

business that you saw before, which didn't differentiate the absorption aspect from the mechanical clearance.

But these are referred to as types, fast, moderate, and slow. Fast things such as iodine and cesium go in at a rate of 100 per day, as location for a coefficient.

And S's slow down appropriately. So, this mimics the common observation in the inhalation that there not only are there fines associated with almost any aerosol, but we always tend to see some material coming in the blood rather quickly, then a slowed down delay transfer to blood.

And that is mimicked by these absorption types. Next slide. I just want to touch -- I just picked up the actinide model because this is another change between publication 30 and the newer data.

The actinides tend to be loosely grouped together in terms of their behavior in the skeleton. And they are referred to as bone surface seekers, that is that when they are taken up from the blood in the skeleton, they are taken up along the surfaces.

That's where, of course, the new bone is being formed or bone is being eroded. That's where those processes occur. Unlike the -- like strontium and radium, the material, these nuclides, once they are deposited on the surface, they tend not to move into the volume of the bone.

And, as in publication 30, they were left here forever on that surface. Well, the newer model actually considers that there is some movement of material into the volume of bone.

One way you can think about that is if you lay down new bone on top of the surface, which is where the new bone is going to be formed, you're going to basically burry the deposit.

And, in fact, the newer information and radiographs, and so forth, clearly indicate that we'll see varied deposits strictly as a line source of plutonium, say, in a bone.

However, as we know, an analogy with the waste consideration, this volume of bone can be called upon with age to erode away with the skeleton with the body's need for calcium.

And so, they can be absorbed from the volume and brought back to the surface and be available for recycling and so forth. So, one of the big distinctions on the actinides is the fact that, on the newer models, we considered this process of burial.

That gets it the alpha part away from the critical tissues within the skeleton that we were concerned with with respect to bone cancer and with regard to leukemia induction.

And so, basically, that alpha event is a bit of wasted dose if it's in the volume of the bone. The next slide shows you, just to keep you up to date, we've got some proposed changes in the plutonium model.

This stems out of a great deal of work that had gone on over the years since we first published that model and ICRP adopted it. This is a model with the changes that are actually before ICRP.

Now, responding to some information that's been in the literature over time, and the newer data, as well as the work we've been involved with, with looking at the plutonium workers at the Myac facility.

And some of this is a little bit of window dressing. There was criticisms of the model in the open literature because there is some potential for some fraction of the activity to actually wind up being deposited in the volume and away from the surface.

It's very small, there's no real significance. But, the model was picked up and criticized for that. We have made a simplifying assumption with regard to this compartment in the soft tissue in the original model.

And that resulted in the blood curve not being very realistic. And that was picked up in the literature. And so, we've had to make a change there.

Also, the liver -- the tissues involved here are really liver skeleton. And we made some changes with regard to the later data suggested a little different partitioning of the material between the liver and the skeleton.

However, there was also an indication that, in order to keep the fecal excretion rates right, there needed to be more explicit considerations of -- pathway here.

The other aspect that was driving some of this is, in fact, to deal with disease states. And, among the workers, there's a high appearance of liver disorders.

And so, one of the objectives here was to modify our reference model a bit to be able to look at the significance of liver disease on that population.

Next slide. All right, let me just go back to the americium example and go back to ICRP 30 and show you what happens here. ICRP, all you had in those documents was an AMAD of one.

That level of educational tool that defect INP allowed you to look at different sized particles. That model is so simple that all you needed was a triplet of numbers to actually look at some different size aerosol sites.

But, here's the -- when you open up Federal Guidance 11, and you're only going to find a class W number for the americium. The effective dose there is 1.2-10 to the minus four.

If you go to 72, and state that I can't do any better than this one micron, here's the numbers for F, M, and S. You see, now we said that the behavior of the americium, at least for the members of the public, is probably more dictated by the nature of the particles, the aerosol itself that it's attached to.

So we no longer limit ourselves to one consideration. And you can see that there's a fair bit of difference on the order of magnitude if you go out.

The other thing that's interesting is the common thought in people's mind that the insoluble form is going to be the most hazardous. And that kind of falls to pieces here with regard to the americium.

F is rather mobile in the -- and, there's -- and, of course, from the ICRP CD-Rom, you can get the corresponding dose coefficients for the ten micron data.

And, again, you can see that there is -- there can be a substantial amount of conservatism depending on how you enter these tables and decide what's the applicable number that you ought to be really dealing with.

And, of course, it would be very important to understand all of these things that you folks have been talking about with regard to the aerosol and the -- that we might be dealing with here, as well as how the activity is really distributed in that aerosol.

Next slide. So, just to make a quick statement with regard to uncertainty, dosimetry often gets tagged with a lot of uncertainties mainly because a lot of people, when they do have a choice of coefficients to use, they'll wind up taking the highest one and not doing their homework.

And so, we get hung by people saying, you know, we've got lots of conservatism in the calculation. There's a couple things to keep in mind.

There's a lot of ongoing work in trying to deal with all these issues. But, biological variability, model uncertainty, and parameter uncertainty are often blurred in the literature.

And so, you have to be careful when you go through the literature. Most people don't even tackle this one of deciding whether it's an appropriate model or not.

And, you get into these exercises that we all fall prone to, of comparing one model against another. But you've got to go back to the basic information and look how that model is derived and it is a basis for.

Our application is really to a reference individual. So, we're setting aside a lot of the biological variability. We recognize that it exists and so forth.

But, that's -- we've defined this character that we're going to deal with with regard to his anatomy and physiology. And so we often set aside that.

That doesn't mean you can't explore these relationships or understand where their referenced individual resides. But, if you get chasing biological variability, it's a tough road to go.

Let me say that we tend to like to actually turn the problem around and talk about reliability of the coefficients. And, we like to fall back to thinking of what was the quality of the information?

What was the quality of the information that we had available to develop a model? And, of course, that's all over the place in some cases. It might come as some surprise, but the plutonium model, there's a lot of good information.

It has been well studied in animals, in man. And we know a great deal of physiological information with regard to the skeleton and so forth.

So, we've got actually recent information from injection studies and so forth that we're folding into that updated version of the model. We've got a good set of data.

Americium -- unfortunately the datasets here are not as strong. They haven't been really mined as well. That's part of the consideration as well.

Now, for these radionuclides, I'd say that the effective dose, you know, we're probably talking about an order of magnitude confidence in those numbers that we're given.

We've got some elements of conservatism in certain places. Sometimes this is like integrating. Instead of temporal integration, you're integrating over tissues.

So, these things tend to wash out a little bit. And so, that's not too bad. They're probably overestimating bone cancer risk and leukemia risk in these models.

I expect that we may, within the ICRP framework, we'll probably change some of the dosimetry. Now, if you think about alpha dosimetry, it's a bearcat to deal with.

I mean, you're asking to look at cells that are at risk. And, not knowing exactly -- and dealing with a radiation that's only got a 50 micron range in tissue.

So, it depends a great deal on what kinds of assumptions you make with regard to where the cells at risk are, and what where is this radionuclide that is decaying.

I think that's partly the issue that's here, is that we probably have been pessimistic and conservative in the way we have set those calculations up.

I think that's the last slide. Yes. And so, I think what's neat is, there's a great deal of capability in the newer dosimetric information. We can deal with a lot of the issues at more depth than what has been done in the past.

But, it's going to really take people working -- not just going into handbook and grabbing the numbers. You've got to work it through a little bit.

And we can address a lot of the aerosol kind of issues that we pull out in the dosimetry, once you folks get your arm around it. Thank you.

CHAIRMAN RYAN: Thanks Dr. Eckerman. Are there any quick questions for Dr. Eckerman? We'll break for lunch. We'll have a roundtable when we come back at one.

MR. MARSH: I just have one.

CHAIRMAN RYAN: Yes, one question, please Dr. Marsh.

MR. MARSH: Keith, what about the -- between the science of drainage, for example, and air going down the lungs, you know, half a liter to maybe two liters a day of drainage between the sinus and the stomach.

So, you get deposition of the sinuses and down the stomach. What about that path, rather than the lung path?

MR. ECKERMAN: I didn't give you the details on the ingestion. And, of course, that's what we're thorough concerned with there. And, there is a new, more detailed model of the gastrointestinal tract that's being put together at ICRP where we probably -- soon putting up on the website for comment.

Probably we need not covert, so it's probably going to be after the year, which will deal with some of those issues. And that's another case of probably conservatism in the ICRP method, as to how we dealt with the contents of the organs irradiating the cells at risk.

So, there will be more details on that, in consideration of -- we still have a bit of trouble getting, like all of us have, the same problem you folks have, of getting a hold of some of the parameters for some of these models.

We can conceptualize them and so forth, but it's difficult to get the parameters. So, there's where that culture change is a little hard, because, in the light of lack of information, there is a tendency to go on the conservative side.

And so, you've got one piece of data that you're going to apply to the human.

CHAIRMAN RYAN: Thanks. Any other quick

questions? Well, we're doing pretty well on time. We'll reconvene at 1:15 for our panel discussion.

(Whereupon, the above-entitled matter went off the record at 12:00 p.m. and went back on the record at 1:15 p.m.)

A-F-T-E-R-N-O-O-N S-E-S-S-I-O-N

CHAIRMAN RYAN: All right, I guess all of our panel members and participants are in place. I want to thank all the speakers this morning, Dr. Harper, Dr. Anspaugh, and Dr. Eckerman, for a very interesting talk on various aspects of the dose calculations associated with an igneous event.

And I'd like to open it up for panel members to make comments or ask questions of any of the three speakers so that we can reach for the goal of maybe thinking about all of these talks first individually, and then what don't we see in the aggregate as patterns, or comments that we can make coming out.

If I may, could I start on this side with Dr. Marsh?

SESSION THREE ROUNDTABLE DISCUSSION

MR. MARSH: Sure. In terms of the presentation today, the thing that I'm struck with we picked our processes to analyze in great detail. And, being that this is such an integrated and far-reaching spectrum of processes that we're talking about, maybe generations, and things moving up to actually hurting people, inhaling particles and things that all this -- pull apart.

It is absolutely critical that we identify the right processes to investigate the detail. And what happens, of course, in these kinds of situations, is that we come at this with a certain expertise.

And we apply our expertise to looking at the process. And we work on a problem that we can recognize. And it may not actually be relevant. And our approach may not be relevant.

And there's the danger that we don't look at the integrated process in identifying which aspects of it are the real critical aspects that we should be looking at.

So, there are areas or boundaries where one group has to assume the results or needs results from another group. And they started off and go forward with those.

The key is we need bridging across these areas. And, when the advice is given to modify or to look more broadly, or to look more deeply, it should be taken in many ways.

But it's hard to do that, because it comes down to what people are able to do. So, the single thing I think that is missing in many regards is that process that I would consider enormously critical that could involve direct experimentation, down to even the level of taking a small -- one of our small cylindrical pellets, putting it in a tephra column.

Let's say the laboratory runs -- fluid times fashion, tumbling these things in there in tephra and seeing what happens to them in terms of what were the fines produces.

In geology we're now pretty proficient at crystal size distribution and particle size distribution theory. So, there's a common ground here.

It is very interesting. We have log-normal distributions we see in the rocks in terms of -- of the crystals, and also in terms of volcanic ash.

So, I think there are aspects of this problem -- serious aspects -- that fall through the cracks. We reach out for a number, and we get a number.

And people go with it. But, the fact is that we actually have to -- as we're doing here -- learn each other's expertise a bit. And we talk to each other, it is amazing.

We understand each other at various levels. A lot of this involves transport, physical processes, transport, and uptake, and things like this.

And all the conservation -- are very well known by all the fields involved. And so, we can actually get them in the same age, I think, without much difficulty.

But an effort has to be made to have the same people in the same room for some period of time, and not in necessarily a formal statement where people are given position papers and things, but actually get that and try to focus and say, let's move the spotlight over here a little bit and let's try to solve this problem.

And so, I think that's a major issue here. I think we have a real strong probability that we could really embarrass ourselves here, as scientists, engineers.

This is a problem that, if we did an excellent job at it, it could set a precedence for the unforeseeable future. I'd like to see some more of this integration from the earliest time to the late dispersal time, to the uptake.

So we really understand each other and really ask the pertinent questions. Just in closing, I would like to say, I spent my entire career working event processes and physics.

This aspect of this is that I will no

longer be able to tell my mother-in-law that what I do is still a practical job.

(Laughter.)

CHAIRMAN RYAN: Thank you Bruce. Bill?

MR. HINZE: Well, in addition to the "I" word, I think we could also use the "I" word for the importance. And I don't mean to steal John Garrick's, but, we have a large number of processes involved here with parameters.

And we worried about -- we tend to get slotted into our own particular favorite parameters. And we're just not very able to consider all of these in the kind of detail that perhaps we'd like to, from a scientific viewpoint by the time we reach decisions.

And that means we do have to find out which are the most important. I hear, for example, a lot about size and mass in terms of the remobilization and dosimetry?

And yet, I don't hear, as a geo-scientist, I know how important the shape factor is. And I didn't hear that until Keith brought that up in one of his slides in those considerations, I think was the title, in which he had a shape factor of 1.5.

That seemed like a very conveniently rounded off number. Excuse me, Keith. But I wonder where that came from and how important that is, and how that really -- how much of a parameter we can enter into our modeling of the shape, because shape can be very important, not only in terms of the settling, but also in terms of pickup static charge, etcetera.

These are the kinds of things that we have to focus on that, as Bruce has put it, we don't want to embarrass ourselves. We can't just accept the standard of values.

And we have to question them, but we have to question them within the framework of risk. I have been privileged to sit in on a number of the igneous activity technical exchanges.

And, one of the great things that is coming out of this meeting is the fact that we are finally paying attention, perhaps too late, to these problems associated with the distribution re-distribution and dosimetry. I'll leave it at that.

CHAIRMAN RYAN: Thank you. Bob, any comments?

MR. BUDNITZ: No.

CHAIRMAN RYAN: Mr. Garrick?

MR. GARRICK: Well, not many, you'll be happy to know. One of the things I wanted to say is that, what I'd like to see is things happen that add credibility to the dose calculations.

I'm sort of reminded in the reactive view, 70's and 80's we took our risk assessment to off-site consequences and calculated dose, and so forth. And, when the NRC got involved, they sort of stopped doing, for reactors at least, dose calculations.

And reasons were not given. There is a much greater confidence in the calculations that lead up to the source-term than there in the calculations for the dose, even though the mandate is for the NRC to protect the health safety.

But they tried to do this through, and in achieving low core damage frequency than demonstrating the off-site doses. So, think the challenge seems to be to do things to make the dose calculations have credibility.

What is saw this morning was particularly encouraging in that regard. Although, I think that the calculations that are being considered here are probably not taking full advantage of the technologies that exist.

One specific example is dispersion models. There has been a tremendous amount of work in dispersion models in dealing with the dynamics and with the changes in direction of wind, changes in wind speed, changes in stability factor, as a function of the atmospheric conditions.

And I didn't see anything of that nature. So, I think we've got a lot to do to establish confidence and credibility in the dose calculations. I think we know where to do that.

And we've always known that the health effects models are suspicious in terms of being able to have high confidence in them. So, there are a lot of uncertainties associated with it.

And, the point I tried to make this morning is that, if we fix a lot of variables, and fix a lot of processes, and decide that we want to do a comprehensive uncertainty analysis of one or two variables and one or two processes.

We may be just kidding ourselves by the fact that we mask the real uncertainty by all the fixes we've made. I just feel there's a lot of opportunity there to improve the credibility of the dose calculations.

And I didn't see a manifestation of A, people taking advantage of what we already know and what we've already done, particularly with respect to dispersion models, and B, in the kind of thinking that we've been trying to inject into these types of analyses, namely consistent treatment of uncertainty and ability to propagate that uncertainty through the model. And I think that's pretty much my comment.

CHAIRMAN RYAN: Just to kind of expand on that, John, when you say propagate through the model, would we really propagate through the entire system model?

MR. GARRICK: Well, certainly do what is reasonable. You know, it turns out that, of course, in the dose calculation, a lot of things are prescribed.

And I kind of object to that too. I kind of would like to know what the experts really think the dose is, prescriptions not withstanding. But, I know of its limitations.

And those limitations have to be addressed by considering the fact that we don't know as much about them as we like. And the way in which we address that is we assign them a little more uncertainty than the others.

But, I think some sort of consistency check so that the reader and the audience realizes that, if you are fixing things and so forth, that they know what it is.

And, of course, they did that in the analysis. But, what we didn't see was what the real impact of that was. And that's what -- it's sort of like we saw in some of the early performance assessments.

We saw language like there was no uncertainty with respect to the solubility of some of the actinides. But, when you read the fine print, the reason there was no uncertainty is they assumed it was constant.

Well, that doesn't take away the uncertainty. And so, what we need to do is simply expose and make clear what we're doing and why we're doing.

And, if we're doing uncertainty analysis, put it in context.

CHAIRMAN RYAN: Thanks. Dr. Melson?

MR. MELSON: I thought Keith Eckerman's talk was very enlightening for me, because I knew very little about it, which is really the beginning. We make it the end.

But I think his comment should have been at the beginning. I mean, this is what people want to know. What's going to get in my lungs, or what's this going to do to me?

And, I think this meeting has been really good in linking these things in a way I've never seen. Usually dosimetry sections are separate. So, we're busy talking about the volcanoes, and they're talking in another room about dosimetry.

So, I thought this was an excellent way of doing it, in looking at we need to be very considerate about these very small particles. So, in a way, this refines the models of volcanology coming up with.

So, in that regard, I was happy when I learned that eh UO_2 assented. Ben Harper pointed out that, even though it is powdered, it's -- when it goes into the reactors.

And that's an important consideration for those of us interested in the fragmentation. And I'm not talking with the powder, but with a more coherent, solid particle.

And I think, generally speaking in terms of the modeling, I think Britt and others made it very clear that we're modeling a very complicated phenomena.

And we're like an infant just beginning to walk, I believe, in some areas. I think Bruce made that very clear with his comments yesterday about how the magma rising has a very complex number of processes -- it only will solidify in a place that's -- water is degassing.

It's very hard to model. To keep leaving out these things and making very simple models, we're not going to get the truth. It won't really tell us what's going to happen.

And yet, to deal with reality is extremely complicated. But, I'd like to say what I see here is movement in that direction. We've all been very generous in the criticism of everyone's models.

And people are going home to think that -- you know, I hope that they will use it. I want to comment on the erosion rates just very briefly, because a number of us noticed that, in Don Hooper's presentation, he was giving an erosion rate of 15 centimeters a years, which is .05 meters.

When we go through the calculations, that's about 150 meters in 1,000 years. It's getting to the point where these rates are -- now these are on slopes.

And I'm not sure what the slope rate was. And you're welcome to comment. Yes, that's what I'm asking about because I thought that was extremely high.

MR. HOOPER: Yes, Don Hooper from the

Center. Are you looking at erosion rate or the precipitation, the rainfall rate?

MR. MELSON: I thought you said this was the erosion rate on the slopes.

MR. HOOPER: Okay.

MR. MELSON: What I heard was -- and you can correct me. You stated flow rates of hillside erosion. And I wrote in parenthesis after that .5 meters a year.

MR. HOOPER: The three to 30 cubic meter per square kilometer per year, that's a sediment yield. That's more of the erosion rate.

MR. MELSON: Well, what was this .15 meters per year?

MR. HOOPER: That should be the annual precipitation.

MR. MELSON: All right. Well, I must have misunderstood you. I'm glad to hear that. Because we would have to be concerned, if that were the erosion rate, of exposure of the repository within a few thousand years.

But, it is true that the erosion rate was not low in these areas. As a matter of fact, one of the highest erosion rates that's been record is for the region -- these dry areas, because of the lack of vegetation, are extremely rapid, much more so than our eastern erosion rates.

So I think, you know, I'd be concerned about how long any load would really last. Now, the other thing is, the casualty variation that was modeled is a critical part of getting at the dose.

How much of this is going to be released and what will it be? And, the one thing we talked about over lunch was the possibility of modeling -- not modeling, but actually doing canister relationship to -- experiments, versus -- because there are places where there are large batches of magma where one could begin to do that.

I'd rather recommend that specifically. I'd say whenever we can come up with ideas about how we can test our numerical modeling, we must do that. Numerical models need to be constantly tested.

About that testing we don't know about the initial conditions. We don't know how well they're going to work. And yet, because they have numbers, it seems so rigorous or so believable by people.

So, there is that danger in numerical modeling and need for experimentation wherever we can do that. What I think we experience here more often than not is the need for transparency in regard to our own communications with each other.

And if we leave sometimes and misunderstood each other, imagine what the public, as you were pointing out, John, must have in understanding what we're doing.

And that call for transparency is a call for doing the job right. So I would encourage wherever we can take some of these processes and make them transparent, and make them available to the public so they can understand what's going on, whatever that takes. I think that's about all I have to say.

CHAIRMAN RYAN: Okay. Thank you.

MR. JOHNSON: Thank you. I just have a couple of very brief comments. One is to amplify on what Dr. Marsh said. And you'll hear more of it on my wrap-up talk at the end of the afternoon.

What I really think we need is some sort of framework to tie all this stuff together. It may help provide the transparency that we need to communicate with each other to make sure that things aren't falling in the crack, and make sure that we understand what each other is trying to say.

And I'll amplify on that a little bit later. Just a comment on something that John Garrick said relative to the NRC using surrogate measures to really measure the safety of the commercial reactor.

We know that when we do that and when we look at what's important based on these surrogate measures, we get different answers. And there'll be a different answer if we regulate it versus the public health effects.

So we have a real danger of biasing our results in our direction of going if we don't use the final end-states as the measures were focusing on.

CHAIRMAN RYAN: Thank you. Yes, Bob?

MR. BUDNITZ: Yes, I realize I wanted -- I passed before -- a couple more comments. I realize I want to say something to reiterate something I said yesterday.

And that is that, as I said, we -- the Department of Energy -- are going to submit our license application in about three months. And, as I said, we worked as hard as we can to make that post-closure analysis for this new forum.

We've also worked diligently to make sure that it addresses the Yucca Mountain review plan, which is the NRC's criteria for reviewing what we sent in and ultimately, we hope, agreeing that what we did is adequate.

And that contains acceptance criteria and various requirements for data and models of the like. So, when you see what we've done, you have to understand that we've done that with those two things in mind.

And then I just have to be sure to say that our models, just like a model of any analysis ever done for anything, is always an abstraction of reality.

There's no such thing as absolutely precise, and accurate analysis of anything physical. It's an abstraction. The abstraction is for that purpose.

And, just to give the opposite example, the best analyses of the response of the Golden Gate Bridge, near where I live, to earthquakes, is not an absolutely accurate analysis.

But it's way more than adequate for the purpose, which is to assure the bridge would be okay. The same thing is true of the analyses of a large aircraft -- I'm going to fly in one this afternoon -- in the face of huge turbulence.

It can't handle that exactly, but we know that those analyses are more than adequate for the purpose. And we feel confident the plane is safe enough.

And that's going to be true here too. So, please don't expect, because it wouldn't be fair to expect that the analyses you'll see in our license application are realistic in that sense.

They won't be, and they can't in the sense that a purist would look. But they're going to be adequate for the purpose. In fact, they're going to be more than adequate for the purposes, we believe.

And we hope that you will review them. I don't just mean the NRC staff, the ACNW, but others in the community will review them and understand them in that light.

That said, we -- like everybody else -- expect that over the years, as more is learned, the analyses will improve and become more realistic. Even though they'll be adequate for the purpose we'll reduce and we'll have even more confidence that we understand things.

And I've listed to these last two days in that light, to see if there are things that we could learn to help us improve the analyses which we believe are adequate for our purpose, to help us make them have higher confidence and to reduce some of the uncertainties, and to understand the differences in everyone's models so that we can feel even more confident that we're on the right track.

CHAIRMAN RYAN: Thank you. Any comments in this session? Questions for the speakers?

MEMBER WEINER: First of all, I wanted to thank the speakers. I thought this morning's session was really wonderful. And I am very sensitive to doing models myself.

I'm very sensitive to this notion that, as Bob just said, we have to find some road between what is adequate for the purpose, and what is truly accurate, because we can't model into the distant future with anything approaching accuracy.

And, I just had a couple comments to make on the comments that were just made. And one of them is, I think, to Dr. Melson. I think we're all sensitive to the question of communicating with the public.

But, I think there is a danger that we run into. And that is to confuse transparency with oversimplification. And I would encourage the people who do the communication with public not to do that.

What we hear at this meeting is so much more informative than much of the public information, that there should be some way to incorporate that information without oversimplifying it to the point where it's not informative anymore.

And I think that's something that we should strive for. The only other comment is -- and this comes off of everything we heard this morning and everything we heard yesterday.

We need to always bite the bullet and look very carefully at where we are being conservative and where that conservatism may be excessive.

CHAIRMAN RYAN: Let me at this moment throw it out to the audience. Are there any questions or comments from this morning's session, or any technical matters?

(No response.)

CHAIRMAN RYAN: Going once, going twice. Staff comments, questions? Yes?

PARTICIPANT: I just wanted to make the observation on the things that we're not sitting in this meeting -- the technical exchange has been great.

It is essential, and it is useful. But, based on this meeting and other meetings like it before, I believe that this is left out. We haven't heard anything from the technical experts to DOE.

You know, DOE is going -- license application in three months. The other thing, to say

-- right now that might be useful for them. And, in the next six to nine months they are going to review the license application.

Some useful, such as -- I think some are very useful also.

CHAIRMAN RYAN: Okay. Thank you. Anything else? Another comment? I guess I'll offer just a summary comment or two. It seems to me that there are a couple of themes that have come out today.

One is, it's always good to hear the vigorous technical exchange, particularly in areas where you don't have expertise. I usually learn something everyday that happens.

And these meetings have been certainly an example of that. I think the theme that has come out to me is we have to be mindful of and careful about compartmentalization of different disciplines.

I think if we do that we tend to lose sight of the big picture or the risk informing aspect of the entire system. So we have to be careful.

And I take to heart the comment that we might really like our own ology a whole lot better than the other ologies. But our ology may not be important, even though we want it to be.

So, we have to be mindful of that. I

think this morning's session in particular was an opportunity for us to see aspects of science and information that transcends some of the ologies.

You know, whether you're health physicist or volcanologist, you can sure appreciate the importance of particle size and resuspension. So, with our presenters this morning, Dr. Harper and Dr. Anspaugh, we heard insights into particle size and resuspension form different points of view sort of independent of this process we're evaluating today.

And that's most helpful to hear. Similarly, we all used dose conversion factors from one handbook or another. Some of us even have to use part 61's ICRP two, which is 50 years old for low level waste.

So, you know, there's a broad range of dosimetry. And I think it is very important to recognize and learn to understand some of the variabilities that Dr. Eckerman told us about as well this morning.

So, I'm mindful of Dr. Melson's comment that integration and somehow sort of putting this on a system level and risk informing it in a detailed way, as Dr. Garrick and Dr. Johnson have pointed out, might be a way to take some advantage from our couple of days together.

So, that's something we need to think about. Any other comments, questions, observations?

(No response.)

CHAIRMAN RYAN: Well, with that, I think we can successfully close our roundtable discussion and our working group meeting. I want to thank all the participants and speakers from yesterday and today.

I do recognize that this close to the LA there have been constraints on speakers, both from the NRC, and for others that may have wanted to participate or offer comment.

But, you know, this late in the game that we are close to the end. So, for whatever reasons folks didn't make comment, we certainly recognize and appreciate that.

So, with that, I think we're up to our two o'clock. We're a little bit ahead of that session. Presentation by stakeholder organizations or individuals is more than welcome.

We have not had any formal request. But, I think Judy you mentioned you might like to make some comments. And I welcome you to make those at this point and anybody else for that matter that wants to have an opportunity to comment is more than welcome to speak as well.

PRESENTATIONS BY STAKEHOLDER ORGANIZATIONS

PARTICIPANT: Thank you, this is Judy from the Nuclear Waste Task Force. You were talking about being this late in the game and this close to the license application and constraints on people.

And I guess the first thing I would like to say is that this committee meets in order to advice the Commission. And, I would hope that you would advise the Commission that the DOE is probably not ready to submit an LA on the basis of the things that you've heard here and perhaps other things that the rest of us have seen too.

We're not really going to be able to get really compartmentalized. But, the Department of Energy is just restarting its second expert panel because they have finally just started to collect some new information on volcanism.

And, the report from that panel isn't due until mid-2006. So, new information would come in in the middle of a licensing review, I guess, according to the current schedule.

And it would be different if this was sort of an accidental thing or something that just happened so that new information came in. But, it's being planned that way.

And it seems to me that the NRC staff is going to have a difficult time. And it's all sort of like a setup for DOE. All of this stuff should be completely done before there is a license application.

This seems to be some kind of a race. And I'm not sure why it is or what the prize is. But, I see it just being done wrong. Another thing that I think is really important when it comes to volcanism, because volcanism is a major failure mode for a repository if one is to be a Yucca Mountain, is to determine actual doses, to know exactly what the dose is to the people in the Amargosa Valley, or wherever it is that the wind heads with the ash.

And, John was talking about having confidence and credibility. And I think that's where it starts. You can do probability analysis, and you can do risk calculations and all of that stuff.

But I think, beforehand, you should know what the actual dose is, and everybody should know what it is so that it becomes clear and transparent because we know exactly what we're talking about and then how it's being used later.

Volcanism is a major failure mode for a

repository. The other one would be corrosion of the canister, if the canister somehow didn't last. That would really impact the waste isolation.

But certainly so would volcanism. And there's a huge unknown and uncertainties that surround it. But this is vitally important, the more information that's known.

The report has been finished. And I think that we're just at a beginning stage on a lot of this stuff. And it's inappropriate to be at this stage when, in fact, they're talking about being at the end.

So, those are the comments that I would give. Thank you.

CHAIRMAN RYAN: Thank you very much. Any other comments or questions? Yes?

MR. SMITHSTEAD: This is Eric Smithstead, DOE. I wanted to perhaps clear something up from yesterday on the probability discussion that went on.

I didn't want the Committee to walk away perhaps having a false impression with what the Department is doing in that regard. We embarked on this program of flying an aeromagnetic survey, which we have results on in the drilling program in PVHA.

We're not doing this to establish our basis for LA. We've done that with PVHA. This is a confirmatory endeavor no different than any other confirmatory endeavor that would -- that we would embark on for areas that are important to performance.

So, testing and this sort of thing will continue. It doesn't stop at 12/04. There is a requirement for a confirmation program in the regulation.

So, that's what this is. This isn't going back and saying oops, and reestablishing a technical accuracy or basis for probability.

CHAIRMAN RYAN: Thanks very much. I appreciate your comment. Anybody else?

(No response.)

CHAIRMAN RYAN: I guess with that session being relatively short, our agenda calls for a panel and committee summary discussion. I guess we just had that.

If any panel members or committee members or speakers -- Dr. Harper, or Eckerman, or Anspaugh, do you have anything else you'd like to add? Please do so now.

If not, I think we're up to Dr. Johnson's final summary. Oh, we have one.

MR. HINZE: Well, I'd just like to go back briefly to yesterday morning when John Trapp was asked the question of where do we have a chance of modifying uncertainties?

And I think his answer was concerned with what we were discussing this morning. And, as I look at that, I see that there are a large number of uncertainties that remain and that, in my view, there has been limited attention paid to these.

So, I think John's comments are very much -- with what we heard this morning, that we can anticipate a decrease in the uncertainties in this whole area of re-distribution, re-mobilization, and doses.

And, I think that we should encourage. Or I believe that the Committee should encourage the Commission to put resources into this, because one of the very important points here is to reduce this uncertainty and increase that credibility.

And so I say that, from what I've heard in the last day and a half, that John is right.

CHAIRMAN RYAN: Thank you. Other comments?

MR. GARRICK: I just want to not leave the impression that all I've got to say is critical. I think this has been a terrific working group meeting.

The presentations of yesterday and today were really informative. And one of the reasons you can be critical is you learn a lot that you didn't know and you get some real insight as to the status of where we are with some of the analytical process.

And I just didn't want to walk away from here without acknowledging that. I think the ACNW should be complimented on these kind of activities and these kind of working group meetings.

They are terrific. And I want to see this particular format replicated with any other oversight group. It is very creative for you to do this. And I hope it continues.

CHAIRMAN RYAN: Well, again, the credit in large part is due to your leadership up to three weeks ago.

MR. GARRICK: Well no, it came before me.

CHAIRMAN RYAN: It's a great format. It does allow for a lot of exchange in a pretty efficient way over a very short period of time, relatively speaking.

So, I agree. And we do learn a lot that helps in our letter writing process to hear a variety of views. And, thank you for your comment. Any other comments?

Dr. Johnson, do you want to summarize for us, please?

EPILOGUE REMARKS

MR. JOHNSON: Thank you. Can you hear me okay? First of all, I would like to thank the Committee for inviting me to come here and participate in this meeting.

I've enjoyed it thoroughly and learned quite a bit. In preparing for the meeting, I tried to absorb as much of that information as is possible.

It is an impressive amount of information, very interesting. It confirmed my appreciation for the geo-sciences, and dosimetry folks, and the materials folks.

And it confirmed my belief that I should stick with developing probabilistic frameworks and leave that stuff to you experts. But I certainly enjoyed it.

If I am contributing something to the discussion again -- I think Dr. Marsh hit it on the head. What seems to elude me when I read this material and hearing some of the discussion here, is a lack of a framework that really ties all this stuff together.

If you ask me what's important, I'm not really sure. We have a lot of very nice analyses, very detailed analyses. But I'm not sure how to identify what's important.

I believe that this triplet notation that you have heard about, this triplet formulation is a path forward to help build this bridge that could help us communicate between our disciplines.

It's more than just answering the three questions of what can go on, what's the consequence, and what is the likelihood. It is really a perspective that starts with a probabilistic framework.

Let's embrace the uncertainties and understand them. Let's identify where there are lots of uncertainties. But, more important, let's look at where those uncertainties are important in making decisions and providing information to the decision makers.

It is top-down approach. It really allows us to dig down to see what's important. A little bit of the history of the concept. I first became aware of it in the late 1970's when the crew that John Garrick put together for identifying risks that commercial power plants.

He was applying it in risk assessments. Subsequently it was published in 1981. So, the concept has been in the open literature for quite some time.

I believe, if we go back further in that, we would see that the basic concept I actually in John Garrick's thesis in the -- was it the Miocene age?

(Laughter.)

MR. JOHNSON: I get those geologic ages mixed up. But, it has been there for quite a while. And it has been applied in a large variety of application from the commercial to the power program, to the chemical program, space, transportation, DOD, and marine applications.

Just to briefly divert on an example in one marine application that I'm familiar with, we looked at a retired supertanker, if you will, off the coast of South America.

It's a tanker that has been through its useful life. So there are some structural questions about how this thing is going to withstand seas and storms, etcetera, etcetera.

It is use to accept crude oil from a number of undersea wells. So, it holds about million barrels of oil on this huge tanker. Now, on the tanker they have built a chemical plant, a refinery.

And, of course, there is 100 or so people that live on this thing. And so, it was a very complex problem to look at. The decision makers were interested in the workers' health, a number of environmental issues.

Probably about doze metrics were of interested to the decision maker. And, of course, it involved a large spectrum of technical experts that needed to understand jet fire from chemical plants or the operations associated with offloading and on-loading sort of things.

Like I said, we have a chemical plant sitting on top of a million barrels of oil with the workers living right in this thing. It is the risk framework that the triplet approach offered that really helped us, in my opinion, put this thing together in a coherent form.

It allowed the different disciplines to talk to each other. I'm confident that, if we were to consider using this type of framework for the Yucca Mountain facility, it would open up the communication channels.

I won't say it will solve every problem, but I'm confident that it probably would. The fundamental characteristics of the triplet, again, it's at the beginning a very probabilistic framework.

We seek out where there is uncertainties, and we want to understand what those uncertainties are. It embraces the uncertainty. We understand that that's a key element in the decision process, and is useful information to pass on to the decision makers.

It is scenario based. Now, for most applications, we've seen it start with an initiating event or a set of initiating events leading to a number of steps to a spectrum of end states, consequences.

In the case of Yucca Mountain, you would probably start with a set of initial conditions and step through a set analyses, processes that would lead to the dose to the public.

It does provide a structure to integrate various components of the analysis. And it uses all the available information. A lot of the material I saw was deterministic in nature.

There's nothing wrong with that. That's a proper way of doing things. This type of framework would embrace that information and bring it into the analysis as evidence.

And, just as an observation, in 1998, NRC Commission -- paper on misinformed the regulation, specifically it adopts to this triplet formulation in their definitions.

So, what's the risk of Yucca Mountain? Well, I had a hard time answering that question with the material I've seen. It is rather segmented, even as this meeting has been segmented in terms of the volcanism, some of the trans-work mechanisms, the materials properties, and the dose.

We really need to consider this question holistically and not look at the problem in these segments, as it can be broken up. The information after all that decision makers -- which I think is the NRC Commissioners -- needs to understand is the probabilistic expression of the dose to the public.

And we need to formulate our answer along that line. So, what's important? It is tough to say what's important. We know that there's a lot of uncertainties, a great number of them.

The question is, which ones of those are really critical for us to understand the question of is this reasonable to license? And, again, a coordinated, integrated approach is the way to approach that question.

It sounds like -- of course, it is dangerous to conclude on the only course of two days or so and a week or two of studying the material.

But, it looks like one could say that the hazard associated with the volcanoes appearing at the site is reasonably well known.

Like I said, I'm not an expert in this area. But, if we're talking about a factor of ten uncertainty in the frequency of these things occurring, that doesn't seem to be so bad for events that are in the ten to the minus seven type of range.

On the other hand, the question is, are there any uncertainties associated with those analyses that would kind of drive us into a new regime where we're outside of the confidence range of where the experts are really trying to tell us what the numbers are.

It gets back to the importance of really articulating as best we can the uncertainties associated with the various parts of the analysis and where that uncertainty comes from.

The other thing that seems important, just form what we've heard in the last couple of days, is the source-term associated with those scenarios. In particular, it seems like there's a divergent opinion or philosophy on how to treat the canisters, for example, if it were to interact with magma, in what form the radioactive material might appear in.

It seems to me, again, just one the surface, just on the few days of studying this, that if I were to chase on particular issue, the source-term is of the most interest in the sense.

Certainly underlying science associated with any risk assessment has to be sound. There's no question about that. But, there real question is, how accurate does the information that our models produce or the science produce, how accurate does that have to be?

I understand what Bob Budnitz was talking about. Basically we need to make sure that the information that's contained in our various pieces of the analysis and of the analysis as a whole is in a sense good enough.

Is it good enough to yield high quality information to the ultimate decision makers? Uncertainty is a large part of that information. It doesn't make sense to invest resources to reduce uncertainty just to reduce uncertainty.

It makes sense to invest resources to reduce uncertainty where it matters. In the papers I've read before I came here -- and I think we've heard it here also today, it seems that I read that -- there was only a handful of isotopes that are really driving the picture here in terms of public health effects.

That, to me, was a tremendous insight after reading a number of things about volcanoes and transport, etcetera. It seems like here it got down to something that I could almost get my hands around.

I think we heard this morning how that kind of insight is kind of filtering back into some of the analyses. But, it really would make a lot more sense, I think, if those little snippets cross the disciplinary boundaries a little bit more so that we focus on what's important.

Or we could challenge that conclusion. Maybe there are other isotopes that are important. Again, that's outside my realm of expertise. I suspect that, as we look at different scenarios, and certainly as we look at different timeframes, that small collection of isotopes that are important might change.

The next question I asked myself is, what's the relationship between performance assessment and what we're envisioning as a triplet application.

That's a tough question for me to answer. I have not really obtained a full appreciation of what's in a performance assessment. So, I'll answer it with some uncertainty myself and recognize that it's a part of the process here.

It is my observation that the performance assessment is in a sense a bled of deterministic and probabilistic analyses. It doesn't seem to tie the sequences, if you will, to some sort of initial conditions to the ultimate question of what's the dose to the public or the exposed people.

And it's not a top down construction, at least it doesn't appear to be in my mind. Those are, to me, the kind of critical observations, and ones that I think a triplet formulation could supplement this viewpoint and make the good sciences going into it more readily available and useful to the decision makers.

In doing my homework for this meeting, I tried to look for what I could find easily about performance assessment, about Yucca Mountain. One paper I ran across was by Leon Reiter.

It was a paper he presented recently in a PSAB meeting, entitled What Role for Performance Assessment. And I'm quoting from this paper without permission. So I'm totally --

MR. GARRICK: There he is, get his permission.

MR. JOHNSON: I told him already if I misstate your thoughts here in any way, please correct me. But, I found it to be a very instructive paper for me to get my hands around the concept of performance assessment, etcetera.

The thing I want to pull from that paper is the fact t the identifies several advantages for performing a performance assessment. These are that the performance assessment allows for the integration of many models and large amounts of data.

The performance assessment takes into account the interaction of different models used. And it takes uncertainty into account. More interesting to me was the disadvantages.

Its highly integrated nature and complexity and obscure those elements which drive the results. Its highly integrative nature and complexity can seem to limit -- can obscure the limitations and assumptions.

And, its highly quantitative nature and complexity can lead to false impressions of accuracy. I think those are any observations that are appropriate for any risk assessment, quite frankly.

But, the philosophy that we're talking about when we talk about the triplet formulation really addresses those issues head-on. We embrace the uncertainty, we include it in the analysis.

We use it as an integrative tool so we hopefully do not obscure the interactions between the disciplines, etcetera. I did want to steal one other part.

It attributes to Hammings from his 1962 book on numerical methods of complexity. Hammings says that the purpose of computing is end sight, not numbers.

I think that's a very powerful thing for us to remember when we do any sort of assessment like this. In conclusion, I just wanted to bring up the concept of the triplet.

It is more than the three questions. It's really a philosophy about how we approach a complex problem. We treat it probabilistically from the beginning.

We treat it end-to-end. We don't try to break it into segments that might obscure the results. And we strive to create a top-down assessment that's supportive of decision making and communication between the different disciplines.

I think such a formulism would be very much complimentary to the performance assessment in existing analyses to date. Thank you.

CHAIRMAN RYAN: Thank you very much. Let's see, that brings us to closing remarks. I think we have been around the room and the table a couple of times here in the last couple of hours.

I don't know if I have any detail to add to that. So, unless there are any specific comments, I think our letter will certainly reflect the content of the meetings in the two days, and some of the summary comments as we prepare our thoughts for our lettering session that will occur in our next meeting in October in Bethesda.

So, with that, I'd like to bring the formal working group meeting group to a close.

(Off the record.)

CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: Advisory Committee on Nuclear Waste

Docket Number: n/a

Location: Las Vegas, NV

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and, thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

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