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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE (ACNW)

168TH MEETING

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FRIDAY,

MARCH 24, 2006

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The Advisory Committee met at 8:30 a.m. at Nuclear Regulatory Commission Headquarters, One White Flint North, 11555 Rockville Pike, Maryland, Dr. Michael T. Ryan, Chairman, presiding.

MEMBERS PRESENT:

MICHAEL T. RYAN, Chairman
ALLEN G. CROFF, Vice Chairman
JAMES H. CLARKE, Member
WILLIAM J. HINZE, Member
RUTH F. WEINER, Member

ACNW STAFF PRESENT:

JOHN T. LARKINS, Executive Director, ACNW/ACRS Staff

MICHAEL LEE, ACNW Staff

LATIF HAMDEN, ACNW Staff

NEIL COLEMAN, ACNW Staff

BRIT HILL, ACNW Staff

BRUCE MARSH, ACNW Consultant

ALSO PRESENT:

CHARLES FITZPATRICK, State of Nevada

WES PATRICK, CNWRA

JOHN STAMATIKOS, CNWRA

DONALD HOOPER, CNWRA

ROLAND BENKE, CHWRA

JOHN KESSLER, Electric Power Research Institute

KEITH COMPTON, Division of High Level Waste Repository Safety

I-N-D-E-X

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

8:31 a.m.

CHAIRMAN RYAN: On the record. The meeting will come to order please. This is the third of the 168th Meeting of the Advisory Committee on Nuclear Waste. My name is Michael Ryan, Chairman of the ACNW. The other members of the Committee present are Vice Chairman Allen Croff, Ruth Weiner, James Clarke and William Hinze.

During today's meeting, the Committee is briefed on recent developments in the modeling of igneous activity in the Yucca Mountain area. Specifically, the Committee will hear a discussion from the NRC Staff and the Center for Nuclear Waste Regulation analysis on the hypothetical scenario in which a geologic repository at Yucca Mount is intersected by a volcanic vent, resulting in the dispersal of contaminated ash. We'll also hear from representatives of the Electric Power Research Institute on their most recent independent study related to the potential consequences of an igneous event in the Yucca Mountain region. And lastly, the Committee will discuss proposed letters and reports from this and earlier ACNW meeting activity from this week and previously.

Neil Coleman is the Designated Federal Officer for today's session.

This meeting is being conducted in accordance with the provisions of Federal Advisory Committee Act.

We have received no written comments or requests for time to make oral statements from members of the public regarding today's sessions. Should anyone wish to address the Committee, please make your wishes known to one of the Committee staff.

It is requested that the speakers use one of the microphones, identify themselves and speak with sufficient clarity and volume so that they can be readily heard. It is also requested that if you have cell phone or pagers, you kindly turn them off. Thank you very much.

Do we have any telephone participants?

PARTICIPANT: (Inaudible.)

CHAIRMAN RYAN: Should we wait a couple minutes? Okay. We'll do that. I guess we're going to hook up folks at the Center and we'll just take maybe a five minute pause in the record, so we can set up the telephone connection. Thank you. Off the record.

(Whereupon, the foregoing matter went off the record at 8:32 a.m. and went back on the record at 8:39 a.m.)

CHAIRMAN RYAN: On the record. Okay. Thanks. With that, we'll go ahead and reconvene our record when our recorder is ready. We're back on the record. We have read our opening statement, folks at the Center, and we're ready to begin. So without further adieu, I'll turn the meeting over to Professor Bill Hinze who is going to lead this morning's session discussing the developments related to modeling of igneous activity in the Yucca Mountain region. Dr. Hinze.

MEMBER HINZE: Thank you, Jim and Ryan. We are pleased to have two different groups making presentations regarding the Modeling of Igneous Activity at Yucca Mountain. The first will be by the NMSS staff. Keith Compton, we welcome you and we're looking forward to hearing about the modeling of the fluid remobilization of possible tephra falls in the vicinity of Yucca Mountain. It's yours and welcome you here and we're looking forward to an update on this work which we heard about some 18 months ago for the first time and we're looking forward to hear what progress has been made and where we are at this point. Thank you.

MR. COMPTON: Good morning. My name is Keith Compton. I am with the Performance Assessment section in the Division of High Level Waste. I'm also the Project Officer for the Integrated -- Team termed Redistribution of Radionuclides in Soil. That's actually the group, the management group, that deals with issues of fluvial remobilization and I will be giving the presentation today instead of Don because I wanted to give Don time to be in the lab and be conducting measurements and preparing for field work.

This also had the effect of ensuring that I read the report very carefully. So I'll be giving it. However, I believe that Don and Roland are on the line and can answer technical questions as well as Brett Hill is here. So if there are detailed technical questions that you want to give directly to technical staff we should be able to answer those.

The second slide, the objectives for my talk today, but the first thing I want to do is to give you an overview of the updated framework for modeling igneous extrusive activity. The fluvial aspect, fluvial remobilization, is only a component of this. So I wanted to give you some idea of what the broader context into which this fits, but the bulk of my talk will be on discussion of fluvial remobilization.

Slide three. So going just directly to the overall framework, as you may recall the previous versions or current versions of the TPA Code rely on a fixed single direction for the wind and there is no explicit accounting for redistribution and we had identified in our risk insights a number of areas that could be potentially risk significant and we are in the process of updating and refining the model for account for these processes more explicitly. The goal of this is try to increase the realism in the model to allow us to explore what the impact of these processes could be.

And I should also mention that this framework was initially laid out in the Risk Analysis for Risk Insights Progress Report. The reference to that is at the end. So if you look at I think Chapter 6 in that report it kind of gives the overview of the direction that we're going.

And the overall structure as you can see is shown in what's called the ASHREMOB module and just to step through the process, essentially eruption/disruption of the packages could result in entrainment of waste in the tephra. Following the entrainment of the waste, the dispersal and deposition of the tephra will be modeled by a dispersion model that will allow the wind fields to vary from realization to realization. So it would allow it vary within the realization. So we're relaxing the constraint that you have a single wind field. It would be deposited wherever the wind predicts it will be deposited and there are three sources.

So therefore potentially there are three sources by which the RMEI could be exposed to contaminated tephra. The first is of course it could still deposit directly at the RMEI location and you would have a direct. So that's the direct deposition scenario. As well, if the tephra were to deposit in the catch net base on the Fortymile Wash, then it could be carried by water down to the RMEI area and this is the fluvial remobilization. And then finally, over a larger area if it were deposited, it could be carried by wind to the RMEI location. Then once it's in the vicinity of the RMEI it could then be rescinded, breathed, by the RMEI and get a dose. So basically there are three components in the updated model.

MEMBER HINZE: Can you give us some idea of when we will be hearing about the Eolian redistributing modeling?

MR. COMPTON: We're working on those reports. What I can do is I can talk to Neil and I get an idea of what the schedule for the deliverable of those reports is so we can get you the schedule for it.

MEMBER HINZE: These are so integrally connected that it's much more appropriate, useful, to us to be able to evaluate them together.

MR. COMPTON: Yes. Understood and I think that that's why this presentation hopefully will give you, we can go into some of the details of fluvial so that when we go into the others we can take it a piece at a time. But understood and we'll get that to you as soon as we can. We're eager to get it to you. We just need to --

And I will mention of course that I'm limited to talking about what is publicly available, what we've already published since some of those things are not yet publicly available, not yet published. We're still talking about them and discussing them. We'll get them out to you as soon as possible.

And again, that's the overall context and today the rest of my talk, I'm going to focus on the fluvial, how the fluvial fits into this basically looking into what are the inputs from the rest of the model and what it outputs to the rest of the model.

Next slide. These two figures show a rough idea of the scope of fluvial remobilization. An eruption that penetrated the repository and resulted in entrained waste could result in deposition within the 40 mile watch catchment area. That's shown in the figure on the left and it's maybe a little bit difficult to see in the overhead projection. There should be kind of a yellow outline showing the larger catchment area.

Then if that were to be eroded, it could be carried down through Fortymile Wash and deposited in the depositional fan that's something in the neighborhood, I don't recall the exact number, but something in the neighborhood of 18 to 20 kilometers south of the repository.

MEMBER HINZE: Could you -- I'm going to keep interrupting.

MR. COMPTON: Sure.

MEMBER HINZE: Because it really is most appropriate when we have these diagrams in front of us. Can you give us some basis or justification for the limits that you've established?

MR. COMPTON: To have a -- withdrawn?

MEMBER HINZE: Yes.

MR. COMPTON: It's my understanding, and I will give a cut at it and then I will let everyone jump in before I get too far on. It's my understanding that those were developed based on slope maps, elevation maps, and then the slopes were drawn to identify the base and is Don on the line?

MR. HOOPER: Yes, that was for the fluvial basin. That's all done by general GIS methods.

MEMBER HINZE: That doesn't tell me very much in terms of the justification for them. That's the method that you used to drive the maps. But how were these lines drawn and in particular what is the outer or the southern limit of the depositional area? It appears to be rather arbitrarily drawn and I'm wondering what's the justification for that?

MR. HOOPER: Following old stream patterns and following the contours on the map and things like that.

MR. BENKE: Yes, the general shape of the depositional area was obtained from satellite images. I think the --

CHAIRMAN RYAN: Excuse me just a second. When you switch speakers at the Center, it's important that you identify yourself so that our record here will be clear who's speaking if you don't mind.

MR. BENKE: Sure. Understood. Previously it was Don Hooper of the Center. More recently, Roland Benke speaking at the Center.

CHAIRMAN RYAN: Thank you.

MR. HILL: Okay. This is Brit Hill, NRC staff. I think I can help explain something about what these are.

MEMBER HINZE: Please.

MR. HILL: Are we looking at the depositional boundary or the catchment boundary?

MEMBER HINZE: No, more the depositional boundary.

MR. HILL: There really are two boundaries for the depositional system in Fortymile Wash. The first starts where you can see Fortymile going from an incising system to a depositing system and that occurs about right around the southern boundary of the Nevada test site. So you begin to get aggradation in the fan system.

The original figure, I'm afraid we can't quite see it on here, but there was an outline that showed the topographic extent of the Fortymile Wash fan system that would be the extent of all alluvium that was coming out Fortymile Wash, the boundaries of which was defined by when it would impinge on the alluvium coming out of another drainage. That would extend all the way down to the California-Nevada boundary line.

But if you take a look at the satellite imagery, you can see most that topographic basin is covered by more varnished sediment. This is sediment that has been pretty stable for say the last 10,000 years or so. This smaller box, the triangle that we're using, is the zone of active, most active, deposition is the area of Fortymile Wash that doesn't have varnished sediment. There is not stable surface through there.

This smaller rectangle, that blue rectangle that you see on the figure on the right-hand side represents the zone of active deposition and by active we mean this is where sediment has been deposited within the last 4,000 to 10,000 years. Now of course, there is some sediment that escapes from that general area and goes down a little bit farther towards the Amargosa River, but the volume of that sediment is incredibly small. It's very fine grain and it doesn't look like there's much active sedimentation through that area.

So when you try to make a first pass or a first order model, where are we capturing, where are we depositing most of the sediment that's coming out of that 800 square kilometer catchment basin to the north? Most of that sediment is being deposited within that roughly 24 square kilometer active part of the Fortymile Wash fan. That's defined again based on topography and based on sediment characteristics.

MEMBER HINZE: That's helpful, Brit, but as you're well aware from the exposure scenario, we're particularly interested in the very fine grain components. These are the components that will be most detrimental to the RMEI. So why should we be concerned about the courser grain and the finer grain which are escaping?

MR. HILL: Most of the sedimentation during these large scaled flood events which is what we're trying to model, not the very small events, but the large scale events that move a lot of sediment are in a, I don't want to go too far into the sedimentology, but they're in a hyper-concentrated regime. They're very large flow, very large suspended load flow events. When these come out of the confined channel at Fortymile Wash and hit the depositional fan, most of the sediment is going to be deposited whether it's course or fine grain.

Now there's always some amount of the allutriated fine suspended material that could continue on down the drainage. But if you look at the sediment that occurs in that active part of the fan, what you'll see is there's also fine grain sediment that occurs in this, it's called the proximal zone of the fan. Not all the fines stay in suspension.

The end result, we're just trying to model the bulk process. We're not trying to model a particular size fraction because that size fraction isn't transported in isolation. It's transported as part of a bulk sediment transport process, the fines and the course materials.

MEMBER HINZE: That's helpful. I just want to make certain that you're really incorporating all of the particle size in the mass balance that you're developing.

MR. HILL: The mass balance is for all particulates. It's truly a mass balance approach. We are not explicitly modeling the hydrofluid dynamics of discrete particle sizes. We're not trying to say that the fine particles and the course particles have different transport tracks or that our understanding is sufficient to model those explicitly.

In a way, this is really similar to

airborne transport problem where the model makes a bulk approximation for grain size. We're not modeling explicitly the airborne transport of individual particles. We're modeling the mass of material and it's the same thing here for fluvial.

MEMBER HINZE: I think we understand that. That's helpful. Thank you very much.

MR. COMPTON: That's useful and that actually might help a few slides down the line when we get to that and I'll talk a little bit more about that.

MEMBER WEINER: Could I ask a real brief question please? Where is the RMEI on your diagram?

MR. COMPTON: The RMEI look -- Well, I can't.

MR. HILL: The RMEI would be at the southern boundary of the Nevada test site which is -- I'm try to describe it. It would be right around the right-hand side of that blue triangle, towards the apex of the blue triangle. If you look carefully, you can see an east-west line coming across there which is a road.

MEMBER WEINER: Thank you. That's helpful.

MR. HILL: That would correspond to the

latitude of the RMEI that's in 10 CFR 63.

MEMBER WEINER: Thank you.

MEMBER HINZE: I think that's an excellent point, Dr. Weiner. It would be very helpful if there were some kind of indication of where the RMEI is on this map to assist the viewer in putting this all into a proper geographic frame work.

MR. COMPTON: Sure, and that's something, I actually have to be honest, I was thinking that I would be able to point to it and do my weatherman imitations and say the RMEI is approximately here, but that's correct. That's roughly the location.

CHAIRMAN RYAN: Excuse me.

MR. COMPTON: I think that's the only one I needed to point to. So going to the next slide, Slide 5, now I'm turning at this point from the physical system to moving towards how our abstraction is going to deal with this process. That's shown on this slide.

The abstraction for fluvial remobilization presumes that there will be a constant airborne concentration in the vicinity of the RMEI, but that that airborne concentration would not persist indefinitely. So in our abstraction, there's a number of values, a number of parameters, that we need to calculate and I'll go into these in more detail and explain the parameters.

MEMBER WEINER: Excuse me, Keith.

MR. COMPTON: Sure.

MEMBER WEINER: Constant concentration of what? Tephra or --

MR. COMPTON: The airborne waste concentration is assumed to be constant and that is a function of several parameters. One, it's a function of the mass load. It is a function of the concentration of waste in the tephra in that mass load. It is a function of how much that may have been diluted during transport. So when you get those three things, that gives you if you have a mass load, the activity and the tephra and then how much of that mass load is part of the contaminate of tephra. That will give you the waste concentration. So that gives you the horizontal line.

The extent, the time required, is the time that's required to deplete Fortymile Wash of erodible tephra. Essentially once the redistribution process stops, then the contaminated portion is assumed to go to zero.

CHAIRMAN RYAN: Just to simplify so I understand you, you said waste. You mean radioactive material, atoms of radioactivity.

MR. COMPTON: Yes, I think it would actually be like grams of uranium. I think that's what the output is.

CHAIRMAN RYAN: Grams or curies, either one are the same. It's a constant concentration of radioactive material in a matrix.

MR. COMPTON: Right.

CHAIRMAN RYAN: So you're assuming that there's complete mixing of a non radioactive substrate with the radioactive material and creating the aerosols. That's the only way you get there.

MR. COMPTON: I think so. Yes.

CHAIRMAN RYAN: Okay. That's probably not a realistic assumption, but so be it. I just want to be real clear that the radioactive material is uniformly distributed in the non radioactive matrix.

MR. COMPTON: Right, and that actually I'll probably get to that.

CHAIRMAN RYAN: Okay.

MR. COMPTON: That goes back to the *** 8:59:02 Corporation model is what you're getting at. It's the idea of to what extent --

CHAIRMAN RYAN: And I'm being a little picky because I just want to make sure that everybody is clear that when you say waste, waste actually contains radioactive and non radioactive mass. But I think you said was that the radioactive material is uniformly distributed in a solid matrix of which some becomes particle.

MR. COMPTON: I'm not making any assumption on kind of at the particle size.

CHAIRMAN RYAN: Forget particle size for the moment. Just that there's a uniform concentration of the radioactive material in the solid substrate.

MR. COMPTON: And I think that's driven by a number of things, but that's the entrainment part. That's done by the depositional model because it assumes how much waste would be entrained within the tephra and then it partitions that among it. That's another part of the model.

CHAIRMAN RYAN: Okay.

MR. COMPTON: And it's similar to the approach that we've used before.

CHAIRMAN RYAN: But it's a critical one because it basically makes all the radioactive material available to become particles and there's nothing sequestered in any kind of event that's not in the particles.

MR. COMPTON: In the tephra.

CHAIRMAN RYAN: In the airborne, potentially airborne particles.

MR. COMPTON: Right.

CHAIRMAN RYAN: Okay. That's a bounding case.

MR. COMPTON: Yes.

MEMBER WEINER: Excuse me. Just to clarify. You're using the calculations from the LaPlante and Jarazemba report to determine how much waste, what the density is, what the particle size is, how much is incorporated into the tephra.

MR. COMPTON: This is actually, and this gets into one of the things that we're working on right now. But this is basically used by the tephra code.

MEMBER WEINER: Okay.

MR. COMPTON: And the tephra code --

MEMBER WEINER: It's what is being used by the tephra code.

MR. COMPTON: The tephra code does that incorporation and determines to what extent the material is incorporated into the tephra.

MEMBER WEINER: But you need to put input into the tephra code.

MR. COMPTON: That's right and the only

thing that I'll say on that is that's something that we're still working on getting that report out. But the kind of model that's used in the tephra code is similar to the ash plume model. Brit, is that a fair statement?

MR. HILL: Brit Hill, NRC Staff. We're using the same ring sizes for the high level waste particles as was used in the airborne transport. Now in reality we know that once that material has been incorporated into a volcanic eruption, transported through the atmosphere and sat for some amount of time on the surface, there is likely going to be some modification to the waste form. We do not have a technical basis to evaluate what the waste form will be following transport and deposition from a volcanic eruption.

We use for transparency. We use the same grain size distribution as we have during the volcanic eruption and that material is assumed to be distributed uniformly through the redistributed or remobilized deposit. So when we talk about waste in terms of the mass load, it's concentration per unit area assuming a uniform distribution of that mass through the mass of redistributed material.

MEMBER HINZE: Thank you, Brit.

MR. HILL: One other very quick point, if we have a very thin deposit that includes more dilution of the concentration of radionuclide than in a thick deposit, once you're having a deposit of less than about three millimeters, you're going to be in of course entraining the substrate or the underlying noncontaminated material. So the model is accounting for radionuclide concentration in the bulk deposit and recognizing that thin deposits are not going to have a uniform concentration. They're going to have lower concentrations of entrained material.

MEMBER WEINER: Thank you.

MEMBER HINZE: Do I understand correctly and if you don't mind, Keith, I'll follow up with a question to Brit, do I understand correctly that the assumption here is that there is a uniform distribution of the tephra over the collection zone?

MR. HILL: No, the tephra is distributed non-uniformly based on the distribution patterns that would come about from the ash plume modeling code. So they're much thicker towards the vent and much thinner away from the vent.

MEMBER HINZE: Okay. How is that taken into account in this? Will we hear about that?

MR. COMPTON: Yes. Maybe if I go through.

You're actually anticipating a number of things I want to talk about.

MEMBER HINZE: Okay. Very good.

MR. COMPTON: So hopefully I can.

MEMBER HINZE: Well, let us not get in your way then.

MR. COMPTON: I will hopefully key into some things. The first question, I mentioned before, those were the four parameters that the model needs, the mass loading, concentration of the waste or of the material --

CHAIRMAN RYAN: Again, when you say "waste" I just want to be real clear. You mean just the radioactive material.

MR. COMPTON: It is the mass I think of the grams of uranium and then to get activity you would have to multiply it by the activity.

CHAIRMAN RYAN: Grams of activity is the same as activity. Uranium and curies are whatever you want, becquerels, but it's not diluted in any nonradioactive substrate except for the tephra.

MR. COMPTON: Except for tephra.

CHAIRMAN RYAN: Right. Okay. So it's not waste. It's radionuclide or radioactive material.

MR. COMPTON: Sure, and in particular, I

think the input is the amount of heavy metal basically that's brought in and to get activity, you figure out how much of each particular nuclide is in that.

CHAIRMAN RYAN: Yes, ratio to uranium. I'm with you. That's fine.

MR. COMPTON: So the first of those assumptions, mass loading is obviously a critical assumption. The key assumption here is that your episodic fluvial redistribution events. Those of course occur episodically not continuously. But they are sufficiently frequent so that they sustain roughly a constant mass loading in the depositional area as long as you have tephra available.

However, once the tephra is depleted from the catchment basin, once you've eroded away all the erodible material and you're not supplying contaminated tephra, it's assumed that ambient sediments from the other areas, the unaffected areas would cover that area and therefore you would get no resuspension of contaminated tephra.

Then the mass loading is a function of how resuspendible is the redistribution at the level of activity, so heavy or light activity. I know that's something that would be of great interest, but we don't have that parameter for you yet. So I'll just say at this point that that's to be determined. That's one of the things that is going to need to go into the model. But the assumption is there. The question is just what's the value.

Next slide, Slide 7, I think this is going to the question that was asked about the concentration, what's the mixing in the tephra or what's the concentration that's used. It's a simple assumption. The deposition model will predict. It will actually predict a pattern. It will generate the isopachs for different eruptions and then within the area of Fortymile Wash catchment basin, you would sum up and determine how much waste, how much uranium is deposited within that catchment basin, how much tephra was deposited within that catchment basin and then you would make the assumption that that's uniformly mixed. By the time it gets down to the, by the time it's been redistributed and brought down to the depositional area you would assume that mixing would cause that to be an equal mixing. So the assumption is fairly straightforward.

Next property we talked about, we've talked about the mass loading and we've talked about the concentration. The next is to what extent could you get dilution with ambient sediments. Of course, when the blanket falls, it's probably not going to cover the entire catchment basin. So you're going to get clean sediments as well that are being brought down the wash and the question is what amount of dilution would you expect to get from that.

The approach is fairly simply. The dilution is simply what proportion of the redistributed material is contaminated tephra, so the ratio of tephra to the total redistributed volume. And again this is a simple mass balance approach. You have a certain amount of material that's deposited in the catchment basin. Tephra will erode from the area covered by tephra at its erosion rate. The ambient sediment will erode from the unaffected areas at its rate. They'll be mixed during the transport. So both will be brought down and brought into the depositional area and then finally as I said, when there is no tephra left, you stop that process and you get just clean sediment coming down and depositing on top.

I'm not going to go into the derivation, but the form on the right, there are some things that I'll draw your attention. One is that there's two ratios in this. One is the ratio of the yields, the ambient sediment yields and the tephra erosive yields. So it's the ratio of those two that drives the dilution and that kind of makes sense. What that's saying in that if the tephra is more erodible, then supplying more material, a relatively larger fraction of that, what's deposited in the depositional basin would be tephra and therefore there would be less dilution.

The second ratio is the area covered by ash to the area of the total basin and again, it also makes sense that the more area that's blanketed by tephra, the less dilution you would get because you would presume that most of that erosion is coming for most of that material is tephra. I won't go into modeling that aerial fraction. The area of the basin comes from the mapping process. The area covered by tephra is an output of the tephra model, the depositional model.

MEMBER WEINER: Are the units of yield mass units or curies?

MR. COMPTON: The units of these, I'm sorry. I should have put those on there. The units of these, and again I can be corrected if I'm wrong, would be kilograms of sediment or tephra per square meter of the basin per time. Is that correct?

MEMBER HINZE: Kilometer probably.

MR. COMPTON: Yes.

MEMBER WEINER: Okay.

MR. BENKE: I'd like to say something. This is Roland Benke at the Center. Keith, the only change would be that for sediment yield in the model is the volume of the sediment per area per event. So that would be meters^3 divided by meters ^{2} to give a unit of a single meter.

MR. COMPTON: Okay. In that case, I'm sorry, then it would be corrected by density.

MEMBER WEINER: Okay. Thank you.

MEMBER HINZE: Go ahead, Bruce.

DR. MARSH: I have a quick question. Bruce Marsh here. In this equation here, Keith, the last term, the area of the basin over the area of the catchment area, the two areas, if they're near each other, then that term is zero. So this whole thing goes to one. So it means that D is equal to one. It means the volume of the ash is equal to the volume of the sediment plus the ash.

MR. COMPTON: I believe -- Let's see. I should be very careful of ever trying to do math in front of an audience. But I think what you're saying is that would go to one. The dilution factor would go to one and, yes, I think maybe that's the key point. The dilution factor is a multiplier on the concentration. So that would represent that 100 percent of the material is tephra.

DR. MARSH: Okay. So the volume of the sediment would be zero essentially to make this consistent in that case.

MR. COMPTON: Right.

MEMBER HINZE: And how are you obtaining the ratio of the sediment to the ash? What's in that?

MR. COMPTON: Based on the relative yield.

MEMBER HINZE: Yes.

MR. COMPTON: I'll go into that. That's part of what Don's work was about was to try and get an idea what the range of that might be. Next slide, I think we're on Slide 9, is the next part is the time required for the flow events to deplete the initial deposit. That's a pretty straightforward calculation and in this equation, it's the number of depletion events that's required times the recurrence interval between them.

Just without going into the derivation, it should be fairly straightforward. The more material you deposit into the basin the longer it might take to erode it, everything else being constant. The higher the ash yield, so the more erodible the ash is, the faster it would be eroded and the shorter amount of time it would take to deplete it.

MEMBER CLARKE: The area of the ash, if I understood what you said in your introduction based on past work we heard in Las Vegas, you're using an airborne release model that has different wind directions. The wind is varying wind directions. Is that correct?

MR. COMPTON: That's right.

MEMBER CLARKE: So depending on when the event occurs, the area of the ash will be different.

MR. COMPTON: Would vary from realization to realization.

MEMBER CLARKE: Yes, so you're running this as a -- Is there a distribution associated with this?

MR. COMPTON: This would be when it's implemented, there you would pick up, you would sample, different values. Again, I don't want to at this point too much into the different models.

MEMBER CLARKE: I understand it might be premature, but I just wondered.

MR. COMPTON: But it would help because I think if you have the whole, everything laid out, no. It might take a long time to go through it, but if you had everything laid out you could take it from beginning to end. But basically you're right. You do a model. You calculate a deposition from that. You get what fraction is covered, how much material was it, how much waste was deposited.

MEMBER CLARKE: So using a distribution of wind directions, you would have a corresponding distribution of areas of ash deposition.

MR. COMPTON: Right.

DR. MARSH: What N in here?

MR. COMPTON: N is the number of events required to deplete the wash.

MEMBER WEINER: I would like to get back to Dr. Clarke's question for a moment. If you are taking into account wind direction, you will have wind that blows in the opposite direction from Fortymile Wash. Does your distribution take that into account?

MR. COMPTON: We intend to put in a realistic wind distribution, so yes. The point is remember, the reason that we're going towards this, is previously we had fixed the wind to blow south at the RMEI all the time. We're trying to get away from that and no, I can't go into more details of that until -- But hopefully we should get a report out to you so that you can understand what that part of the modeling does and I think things will make more --

MEMBER CLARKE: Ruth, I could have asked that question better, but I'm focusing on the area of ash that's within the basin.

MR. COMPTON: Yes, you overlie the two because it's not in the basin does not --

MEMBER HINZE: Brit, do you have a comment?

MR. HILL: Brit Hill, NRC staff. Very quickly, yes, the idea would be that for each realization you sample a wind field based on NTS or Yucca Mountain type data. Some realizations, the wind may be blowing completely away from the catchment basin in which case for that realization there would be no material in the depositional basin and in all likelihood there would be tephra deposited at the RMEI location. So the airborne release would likely have a zero dose for that realization.

MEMBER CLARKE: Thanks, Brit. That's what I was asking.

MR. COMPTON: And again, the one thing that I wanted to say on this slide, again the model is fairly straightforward, high erosion rates or shorter times. More material is larger times. One thing that's worth bearing in mind is the effect of the ash yield on both of these parameters. It works in kind of in opposite directions. If you have higher ash yields, if the ash is more erodible and is eroding faster, you'll get less pollution. You'll get less mixing of that with clean sediments, but it also won't last as long. And vise versa, if the ash is not eroding very quickly, it may be able to erode for a long time, but it would be relatively more polluted. It's just a point that's worth keeping in mind.

DR. MARSH: So the units on Y in the ash -- The units on Y, there must be some thickness or it must be a link scale to make the units match in there.

MR. COMPTON: The units of this --

DR. MARSH: Of Y ash. So it would be mass on the top and you have area.

MR. COMPTON: You have a mass that's determined on the bottom.

DR. MARSH: Yes, so Y is measured in some sort of length of thickness, I guess.

MR. COMPTON: I think of it in terms of an amount of material per area of the basin per time.

DR. MARSH: Okay. Thickness.

MR. COMPTON: I'm not quite sure whether it's --

DR. MARSH: It's a length of some sort.

MR. COMPTON: If it's measured in volume, it would be a length. Yes.

MEMBER HINZE: That's what it is in the equation.

MR. COMPTON: Yes, and then you have to compare that to the mass because you're given then in the numerator you have a mass of ash. You have the -- to make that.

So now I'm going to turn a little bit. That was the abstraction model. I've given you how we get at the four parameters we use in the abstraction model and two of those I gave you the explanation for already. Now we need to explain how we get to dilution factor and depletion time. So we need more parameters, to figure out what those might be and there are four. There's the recurrence interval of the flood events, it basically just converts the episodic flood events to a per year basis, the density of the ash deposits and then the pre eruption settlement yield and the post eruption settlement yield.

Moving right along, Slide 11, the first two are fairly straightforward. The recurrence interval is about four years. So it's based on observations of flood events that there would be a redistributing event about every four years and the destiny of the tephra is based on Cerro Negro positive.

MEMBER HINZE: Is there any consideration of climactic shifts and changes and as we look at this relatively short time period of 30 years?

MR. COMPTON: I'll turn that over to Don if I can to let him answer that. I don't believe that we're basing this reference interval and we're extending it forward. I would have -- So basically we're assuming that that --

MEMBER HINZE: Extrapolating.

MR. COMPTON: We're extrapolating.

MEMBER HINZE: On the basis of the 20 year time frame.

MR. COMPTON: Right. What I would have to do is look through the equations and see how that would play out through the whole equations, in other words, would you have --

MEMBER HINZE: As part of your sensitivity studies?

MR. COMPTON: I would say, to answer your question, I would need to look at that and to see whether that would result effectively in a faster, I presume that could result in effectively a faster or slower yield is I believe the effect that that would have. If it's happening very frequently, then the yield would be higher. If it was happening very infrequently, the yield would be lower on an annual basis, on an average annual basis.

MR. HILL: Brit Hill, NRC Staff. We did not consider any other effects such as climate change in looking at the number of events. We recognize that this is the only observational record we have for flood events of Fortymile Wash, but we are in the position of do some exploratory analyses to see whether or not that is a highly sensitive or relatively insensitive sort of uncertainty.

Recognize that a flood event every four years, it would be difficult to have a much dryer climate say and have those events be spaced out longer and longer. Say that you would want to have an event every 100 years. It's possible, but the information would be a little difficult to do that. Most of the uncertainty we would consider would be for a wetter climate and more frequent flood events.

MEMBER HINZE: Thank you.

MEMBER WEINER: Excuse my ignorance, but what do you define, how is a flood defined here?

MR. HILL: I'm sorry I used that term a little loosely. It is an event that is sufficient to cause flow within the Fortymile Wash drainage system so that you would have active flow at this location in the system. Don Hooper, if you're still on the line, I think you could probably define it a little better.

MR. HOOPER: Yes, this is Don Hooper at the Center. What was basically used for those 11 flood events was just water flowing back to the last flood gate for the basin outlet of Fortymile Wash, the one nearest Highway 95. So there were 11 events over those 30 years recording periods. So that includes the volume flow then moving through that very last stream gate.

MEMBER WEINER: So I can take it that when it's not a flood the water never reaches that last flood gate. Is that a correct interpretation?

MR. HOOPER: Right. It has to be a flow of water large enough to sustain flow down Fortymile Wash. So that means it's a somewhat larger flood like flow of water you have.

MEMBER WEINER: Thank you.

MEMBER HINZE: And the intensity of the flow is assumed to be constant in these 11 events or do you have a distribution that you sample?

MR. HOOPER: These are just measurements recorded at a solitary state and there are only four stations along the Watch. So if at the station that was used, you can't really measure variations in velocity well at all. So, no, it's unfortunately fairly poor of data for a single datapoint.

MEMBER HINZE: So the assumption here is that they're all the same.

MR. HILL: Brit Hill, NRC staff. Yes, that's correct. They're all assumed to be just a single type of a transport event. We're not trying to model hyper concentrated versus normal flow regimes for example.

MEMBER HINZE: Thank you.

MR. COMPTON: And then again, I would mention that when we have the model running we can go look and see what the sensitivity would be to these parameters and determine whether it would be justified to go and collect more data on that.

DR. MARSH: So the duration events are all the same and volume of the events.

MEMBER HINZE: Thanks.

MR. COMPTON: If you to the next state, the pre eruption sediment yield, the ambient sediment yields are estimated conceptually in a fairly straightforward fashion. Essentially how much sediment has accumulated in the active depositional area, over what time period that material deposit and then in order to normalize it to area, what is the area from which it originated. So the values that were used in the calculations were shown here and I've shown previously the upper part of the catchment basin has an area of I believe 815 square kilometers.

So the next, and this is the last parameter that we need and then after we get this one, we can start going back and get into results, is essentially the relative sediment yield. It's the question of once the tephra falls, at what rate is that going to be eroded? Don's process modeling was really focused on getting some insights into the range of relative yields that you could observe following an eruption. This was done using a diffusion-based erosion model. It was parameterized usually slope data and observations at Lathrop Wells. The two dimensional data was transformed into an equivalent 1-D model and that's what was run to estimate the total sediment yields over time.

Just moving to the next slide, this is really where and the model suggests that you would have a period given that the tephra would be more erodible than the ambient sediments. There would be a period of accelerated erosion. You would get a period where you would expect to see more sediment than before the eruption. That would go up as you caught up more and more of the basin and then eventually it would decline over time to the pre eruption yields and that's a pretty common phenomenon.

MEMBER HINZE: It's observed. Right?

MR. COMPTON: Right. Say again?

MEMBER HINZE: It's observed.

MR. COMPTON: Right.

MEMBER CLARKE: What are these units, Keith, on the -

MR. COMPTON: The relative sediment yield?

MEMBER CLARKE: Yes.

MR. COMPTON: Don can correct me if I'm wrong but that would be the ratio of the mass, the delivery rate of the mass after the eruption relative to the delivery rate prior to the eruption. Don, could you?

MR. HOOPER: Keith, the ground to sediment yield actual units on that is unitless.

MR. COMPTON: Right.

MEMBER CLARKE: Right.

MR. COMPTON: It would depend on what term you took.

MEMBER HINZE: It's a multiplication

factor.

MEMBER CLARKE: Okay. Understand.

MR. COMPTON: And the point I guess that I would emphasize on this is that the relative yields are probably within a fairly narrow range. They're not going extremely high. They would be elevated, but they're not going to extremely high numbers.

DR. MARSH: So the key result here really is the time delay or the hold off in terms of this time scale here.

MR. COMPTON: That's part of it and also the range, the value over which that sediment yield range is in the abstraction model because you recall that I had the expressions that showed the ratio of the ambient yield to the tephra yield. This is close to, I'm not going to say right now whether it's exactly the numerical value, but that's an indicator of what that ratio is.

So again kind of past going back to, now I'm going to jump ahead to my results, but again if you have more accelerated erosion, the significance of that is that you would get less pollution because more of that stuff in the depositional basin would be tephra and if you had a lower level of erosion or lower fraction, this is not in this part, but a lower fraction covered by tephra, then you would get relatively more dilution and then again the effects on the time required for --

DR. MARSH: So the key time here, the 25,000 years for example, that's pretty much set by the number of events, the type of events.

MR. COMPTON: I would have to ask Don to explain the basin. I mean that would have to do with essentially how fast the tephra was able to move through the basin and deplete.

DR. MARSH: So the frequently of erosion events in the magnitude of the event.

MEMBER CLARKE: Intensity of the event. Go ahead please.

MR. HOOPER: Don Hooper at the Center, could you repeat the question please?

DR. MARSH: I'm just interested in the decay time here, this 25,000 year decay time, and that should be pretty much set by probably the number of events, the frequency of the events, of erosion and the magnitude of the events.

MR. COMPTON: And the erodibility of the tephra.

MR. HOOPER: Yes, that's set by the modern erosion rate. That's sort of an underlying guidance.

Yes.

MR. BENKE: This is Roland Benke at the Center. In addition to that, it would be dependent on the volume of tephra originally deposited in the Fortymile Wash catchment basin.

MEMBER HINZE: While we're on this point, has there been an attempt made to use the tephra from Lathrop Wells that is now in the soils or in the alluvium as a basis of validating all of this?

MR. COMPTON: I'll take a cut at it and then I'll let them answer in more detail. I think that one of the things used to constrain the erodibility or to set the erosion of the tephra was based on the Lathrop Wells data. The parameters, the diffusion coefficients, were based on the Lathrop Wells data. Don, can you?

MR. HOOPER: This is Don Hooper again. Yes, it was basically based on what was observed at Lathrop Wells. For example, we know that Lathrop Wells erupted 80,000 years ago and there's only a small remnant amount of tephra. So we know that in that 80,000 years obviously the whole deposit has been eroded. So that's one upper bound. But, yes, what was observed in erosion pattern or suspected erosion pattern of tephra at Lathrop Wells was used. Yes.

MEMBER HINZE: Is there any indication of the tephra in the alluvium in Amargosa Valley?

MR. HILL: Brit Hill, NRC staff. No, there's been no evidence in any of the drill holes for example or any of the shallow trenching that's occurred immediately down gradient from Lathrop Wells. This has been a question that we've asked the Department of Energy a number of times, where are any and all exposures of the Lathrop Wells tephra. There is enough of the trace of the deposit to say that the bulk of it was distributed to the north.

MEMBER HINZE: I think there are some in the trenches we saw up in the --

MR. HILL: Approximately 15 kilometers to the north there are some relatively non diluted to very lightly diluted fall deposits that were in trench 8 for example and some several other of the intervening trenches. We also can tell from the distribution patterns that there was a fairly thick deposit about 1.5 kilometers south of the volcano at least 1.5 meters thick. There's a preserve little remnant there.

So it wasn't all purely distributed to the north. Some of it did go south of the vent, but literally there is nothing south of the vent that has been recognized with the exception of that one deposit I mentioned. Nothing is out on the alluvium and also in the surrounding hillsides including where we would have expected very thick, meter thick, sorts of deposits from initial deposition, they're completely stripped off fairly low gradient hills.

MEMBER HINZE: So we can assume that the lack of evidence for Lathrop Wells tephra in Amargosa Valley is an indication that it has been moved out of the valley or is it a matter of not being able to recognize it or not doing enough work to recognize it.

MR. HILL: Another hypothesis might be that it is buried several meters deep. It is possible that there could be appreciable deposits.

MEMBER HINZE: Oh, it hasn't been looked for completely.

MR. HILL: There has been surface excavations on the order of one to two meters out in that area from just general excavation. It has not encountered to the best of our knowledge any tephra. It could be deeper than that though. But the drill holes have not encountered it either.

MEMBER HINZE: You've look at the drill holes in the alluvium for this then?

MR. HILL: I've not personally examined

them, but the lift logs, in this area the basaltic tephra is a very characteristic, very easy to identify, very unusual feature. I think we would reasonably expect during drilling if this was encountered it would have been noted in the geological logs. So to the best we can tell, this deposit is probably 99.5 to 99.9 percent removed in 80,000 years.

MEMBER HINZE: Thank you.

MR. HILL: That's our data point for figuring all this out.

MEMBER HINZE: Thank you. Keith.

DR. MARSH: I had one last question, Keith. I realize that these are the parameters that go to solving a differentiation equation probably for erosion that's a diffusion style equation. Is that how it's for getting this -

MR. COMPTON: These parameters are --

DR. MARSH: I realize the model is probably much more involved than we're seeing here.

MR. COMPTON: Yes, this is a quick summary of what's in the model and then the --

DR. MARSH: Input parameters.

MR. COMPTON: And what the input parameters are, not the values of them.

DR. MARSH: But the key. There must be a

key differential equation behind this. A diffusion for erosion, that's what many people use. Is that basically at the root of this?

MR. COMPTON: Yes, that's the approach. Don, I think you might be able to explain.

MR. HOOPER: Yes. This is Don Hooper. Yes, the procedures of the equations have been used in landform degradation, landform erosion, soil erosion, for several decades now. It's a fairly well established piece of --

DR. MARSH: Yes, great. Thanks, Don.

MR. COMPTON: And again something that I would mention about that is that again when we have the whole model put together what this impacts is the relative yields and once we have some set of results, we can decide whether we're warranted to go towards different types of models or whether we need to explore later.

I was on Slide 15, the Sediment Budget Parameters, this just gives you, shows you, what the numbers were. I will then break quickly and go to Slide 16 and indicate some of the results. Actually this should probably say the abstraction modeling outputs.

These are the parameters on the previous slide were put in the abstraction model to compute the degree of dilution that you would expect or the dilution factor rather and the tephra depletion time and again bear in mind, dilution, it might be a little bit confusing just giving it in percentage terms, but that is the percentage of the deposits that are tephra. So 100 percent means that it's 100 percent tephra.

What you get out of this, I would just kind of look at it in as fairly broad scale. What you see in these is that the dilution, the extent of dilution, looks like it's going to be somewhat constrained. You would not, given the sediment yields and the relationship between the post eruption yield and the pre eruption yield, it would suggest that you would not get a large degree of mixing with clean sediments. By large, I mean factors of 100 or 1,000. But the depletion time is a little less constrained and can vary over larger amounts. But it does suggest that remobilization could supply tephra to the depositional basin for long periods of time. I'll just say long periods of time you might expect to see tephra being brought down into the depositional area.

MEMBER WEINER: Excuse me.

MR. COMPTON: Sure.

MEMBER WEINER: Is this dilution over the entire fan, the entire deposition fan?

MR. COMPTON: Yes, the assumption is that what we actually would compute would be the proportion as it comes into the depositional area what is the relative ratio of the two. We're not trying to sort out as it moves down through the depositional fan, would it change because of different characteristics of the ambient sediment and the tephra. We're not trying to track those, how might it sort out and vary over time. We're just looking at the relative --

CHAIRMAN RYAN: Just a question on that point. If I look at the chart, and I understand the differences in time, just kind of eyeballing it, it's one to one or two to one for mixing, something in that range. And then for the idea that you're not trying to account for any difference between the sediments that are there and what's added, have you explored if that's a reasonable assumption based on just thought experiment type approaches, what if? What if there is a difference and what if there is some preferential behavior over these times particularly these very long times to see if that assumption holds up?

MR. COMPTON: Yes, I think what I would suspect what you might be getting at is the issue of would maybe the characteristics of the two be quite different. So would the particle size characteristics for example of the ambient versus the tephra be different over time?

CHAIRMAN RYAN: Yes, and I appreciate --

MR. COMPTON: And right now, we're not doing that, but that's certainly something that we're thinking about as how much error could you introduce by that and what that would get to is would you be somehow bringing down stuff that's more or less resuspendible. I would suggest that's really you would need to focus on.

CHAIRMAN RYAN: Sure, and I appreciate the fact that you can't. It's very challenging to think about how you would verify any of those thought experiments in the physical reality, but I think it's very important to understand how the results would change if those things were actually shifting one way or another.

MR. COMPTON: Yes. Certainly if you were bringing down, mixing it, if the characteristics of the two were quite different and you were bringing down stuff I would suggest that was much more resuspendible that would have one significance. If it was much less -- You could think about that and you can do thought experiments to figure out what might that, how robust are our results. I mean would they change dramatically.

CHAIRMAN RYAN: Sure, and I guess what I'm asking is that that be considered and that's not the subject of what you're presenting today and I appreciate that very much. But I think that's for us from a risk significance point of view, that's where the rubber meets the road of understanding that.

MR. COMPTON: Yes, and that's one of the reasons that we're still discussing the model.

CHAIRMAN RYAN: Okay. Thanks.

MR. COMPTON: And trying to make sure we all are in agreement before we put those to you. So that brings me to the end of my presentation. I think I'm getting in just before the finish line. The summary is that I gave you an overview of the process model and the key things that I would suggest to take away from that were that dilution with ambient sediments would probably result in some degree of mixing, some degree of dilution, with ambient, but at least at a bulk level, it does not look like it would be large amounts of dilution and the time required to deplete the tephra deposit is quite long. So again the conclusion is that the redistribution could continue to supply contaminated sediment onto this fan for a long period of time. I'll just end with the abstracted, the overall model, of how this all fits together is still under development and we're still working on getting the parameters for the rest of the model and then we'll have to exercise it and see what kind of conclusions we get and what we need to find out and what kinds of things we say this is probably good enough and we don't need to go into more detail and what are the things we need to go into more detail.

CHAIRMAN RYAN: Just a thought if I may on your second point, you know 600 to 127,000. It's three orders of magnitude. Do you have any plans to do thought experiments to explore that range? And let me ask the second part of the question. The time to deplete the tephra over those time frames, I would guess, I'm not a geologist and I've said that before and I'll say that again, but I would think that a lot more processes might be involved that might add new materials or take away new materials or somehow modify the physics of what's going on over a time span of three orders of magnitude. Over 100 years, I can think about things being fairly constant, but over those other time frames, I'm just wondering how you're going to explore that. Is that a fair question?

MR. COMPTON: It's a fair question. The question is if your results suggested that that was a very important parameter and that it makes a big difference as to whether it's 1,000 years or 10,000 years or 100,000 years, then it's something that you might need to look at. But it's hard for me to predict right now what that's going to be because it's a function of all the other things that are going into the equation.

CHAIRMAN RYAN: And again I know we're looking ahead a little bit. So I appreciate the fact that you can't answer it today. But again, I think things that help you examine the range of possibilities not using fixed values or not using the assumptions appropriate for a short time frame would be interesting to us and I think helpful to defending what ultimately your case is that you end up with.

MR. COMPTON: I think once we get the model running and then interpret the results, we might be able to come up with kind of a story that the model is coming out with. I mean here's what we're conceptualizing as to what's happening and does that make sense. Does this thing make sense or are we out of line?

MEMBER HINZE: You're tantalizing us.

MR. COMPTON: I am. Hopefully, I've left you eager for more.

MEMBER HINZE: Neil, did you have a comment as a follow-up to Dr. Ryan's?

MR. STAMATIKOS: I don't want to interrupt, but this is John Stamatikos at the Center and I do have a comment.

CHAIRMAN RYAN: I'm sorry. We were kind of stepping on our comment from the Center. If you could tell us who you are again, and then maybe get a little closer to the speaker phone. It was just a bit garbled.

MR. STAMATIKOS: Yes, this is John Stamatikos and Mike, I want to address your question if I could.

CHAIRMAN RYAN: And just for the recorder's benefit, John, it's John Stamatikos.

MR. STAMATIKOS: And the current landforms that are out there are actually quite stable for periods of time well beyond 125,000 years. So there are some good studies. For example, there were cosmogenetic studies that were on surfaces where the Ghost Stand fault was exposed on one of the ridges of Yucca Mountain and shows slope stability, sort of current type slope stability, that have been there for about 325,000 year.

CHAIRMAN RYAN: Okay. That's helpful and again, I think those things help as you integrate those in your model and the structure of what you're analyzing. It's helpful for us to understand those assumptions which I appreciate. Thanks.

MR. STAMATIKOS: Right.

MEMBER HINZE: Thank you, John. Let's open. If you're through, then let's open. We have just a few more moments. Let's open this out to make certain that we have questions from the Committee.

DR. MARSH: I have one quick question. So if we compare the example that you showed on Slide 14, so I can be on the same page here, onto your 16, I guess we would be over in Case 6, 24,500 years that would be.

MR. COMPTON: I don't recall exactly all the parameters that were used to generate this one. So I want to be careful about saying that that is Case 6.

DR. MARSH: The times would be similar.

MR. COMPTON: This, I would say, that this time is within the ranges of what the abstraction model is generating. We could go into -- The report

I think described it in detail and that's publicly available and we could look into all the parameters used to generate this particular curve. But again, for what I would suggest is what you get out of that is kind of range of relative sediment yields and the basic conclusion being that if you have a lot of tephra eroding, you're probably not going to get a lot of dilution and you would have to have very low tephra yields to get a lot of dilution.

DR. MARSH: So when it's laying around too, following up on Mr. Ryan's comment here, when the stuff is laying around from 5,000 to 100,000 years, for example, you don't consider any chemical weathering.

MR. COMPTON: I don't believe we consider it.

DR. MARSH: Degradation of particles and stuff.

MR. COMPTON: I think what you're saying is would the kind of erodibility characteristics change over time. Would you get stabilization of it by different -- I don't think we're looking at that over this process.

MEMBER HINZE: The primary factor here is going to be wind erosion.

MR. COMPTON: Yes. Could it be depleted by a different weathering process?

MEMBER HINZE: And that we're not hearing about yet.

MR. COMPTON: Right. Unfortunately not.

MEMBER HINZE: Let's go to Dr. Weiner. Ruth.

MEMBER WEINER: Thank you. I have a couple of questions and I apologize for all the interruptions I've been doing. As I understood you to say before when I asked where the RMEI was, the RMEI is at the apex of that deposition fan.

MR. COMPTON: Approximately.

MEMBER WEINER: Approximately. Then what is the significance of looking at the dilution of the tephra over the entire fan since that's downwind from the RMEI or downstream from the RMEI?

MR. COMPTON: I think what you're getting at is what the source in the breathing zone of the RMEI. What is the source of that material?

MEMBER WEINER: Exactly.

MR. COMPTON: Is it coming from upstream? Is it coming from downstream? Is it coming from very far away? Is it coming from very close? That's something that I don't think that we have nailed down completely yet, but I would say it's the area. I don't know exactly what.

MEMBER WEINER: So you're assuming from your model that it's coming from the whole area some way.

MR. COMPTON: It's coming from a variety of sources. Again as I said before, in the overall model, you have a source from the initial deposit if there were an initial deposit. You would have kind of an aerial Eolian source that's coming from a large area and then you have something that's coming from a closer source. The relative weights of those is something that we're still looking at.

MEMBER WEINER: I do have a question about those as long as you brought it up. Do all those sources affect the same RMEI?

MR. COMPTON: Yes, the RMEI would be assumed to be potentially, I mean depending on if there's not a direct deposit there, then they're not going to be affected by it. If the contaminated tephra --

MEMBER WEINER: If the person just stands there and is affected by all three of these sources, there's no time. It just all concentrates on this one maximally exposed individual.

MR. COMPTON: Again, the question would be what is the weight that you assign to the different components.

MEMBER WEINER: I see.

MR. COMPTON: That's one thing we haven't quite found yet.

MEMBER WEINER: Okay. My one --

MR. BENKE: The Center. I wanted to touch a little bit on that question.

CHAIRMAN RYAN: I'm sorry. Could you tell us who you are please?

MR. BENKE: Sure. This is Roland Benke from the Center. I just wanted to add a comment to the question by Dr. Weiner. In a general sense, just what Keith said about the three source regions applies, but what you could consider that the receptor would be breathing would be a regional mass load that could come from resuspension of contaminated deposits under foot or could come from a couple of miles away as the wind may blow them in.

MEMBER WEINER: Thank you. My one other question is looking at your chart on Slide 16, do you have any idea what the mechanism is, and this may be a unfair question, do you have any idea what the mechanism is that causes this particular dependence of distribution of time? In other words, you seem to get a 40 to 50 percent tephra right at the beginning for each case and then the mean amount, it's the low amount and the mean amount you have a very tight distribution toward the early years and it doesn't even go away very much. I was just wondering. Have you looked at the mechanism why this happens? Do you have any idea?

MR. HILL: Brit Hill, NRC staff. There's a real simple explanation for that. This is very low sediment yield system and the amount of ambient sediment is reasonably well constrained. So there isn't a lot of uncertainty on that and by the same perspective there isn't a lot of uncertainty. There's probably only two orders of magnitude variation in the total amount of tephra that you have available within this system.

So the reason you're focusing on roughly 50 percent dilution is kind of a natural consequence of the generally low sediment yield in the basin, the generally high sediment yield, the high tephra yield coming off a tephra deposit and a fairly restricted range of tephra volumes that you can potentially have out there. So at the tails of the distribution, yes, we're seeing a lot more of the variation, but about the mean, there isn't too much variation because the uncertainty of the key parameters is restricted to a fairly narrow range.

MEMBER WEINER: Thank you.

MEMBER HINZE: Allen.

VICE CHAIRMAN CROFF: Mike asked a lot of my questions.

MEMBER HINZE: James. Bruce.

MR. COLEMAN: Neil Coleman. Is that one working? Neil Coleman, ACNW staff. I had a comment on this active fan area. Keith, your Slide 11 listed 11 floods from 1969 to 1998 and there was a comment there that seven of these floods exceeded 1/10th of a cubic meter per second. Now there's been a little more documentation on Fortymile Wash.

For example, the 100 Year Flood has been estimated at 430 cubic meters per second. The 500 Year Flood at 1,600 cubic meters per second and the 1969 event has been estimated at around 100 cubic meters per second. This is well beyond what you're showing.

Two of the events that happened in Fortymile Wash and the Amargosa River reached Death Valley and the 1969 event produced a shallow lake in there. Other events occur that don't even reach

Highway 95. They deposit sediment in the wash and then the biggest events come along and they wash that material out.

MR. COMPTON: That's out of the deposition. What you're saying is get past the deposition.

MR. COLEMAN: They just carry it on down through the system to the fan, what you refer to as the active fan, and in many cases beyond. Now as Dr. Hinze pointed out earlier, your Slide 12 shows that the active fan terminates and you assume that no sediment leaves the area in any flood. That means you have underestimated the two biggest floods in the period of record. When you only have a small number of them to work with, they really deserve some special attention.

These would have carried dramatic sediment loads beyond the so-called active fan and the fine grain silts and clays could be transported the farthest. Silts range from 4 to 62 microns. This is the size range of greatest concern in health physics. So my comment is I know it was mentioned earlier that you didn't want to get into too much fluvial transport. I think Dr. Hill mentioned that. But I think when you look at these largest floods and information about the estimated 100 year and 500 year record it deserves some looking at.

MR. COMPTON: I think one thing to kind of bear in mind is look at what the abstraction model, what the basis is, and what the basis is is that you get a flood that is bringing down enough material in a mass load. Now if a lot of the rest of it just keeps going past from the model point of view you're suggesting are you getting enough deposition in that area kind of periodically to keep that well supplied with tephra and that's the basis of what we're looking at. So I see your point, your point being you could have something that comes down and just cleans everything out I think is what you're suggesting.

MR. COLEMAN: Well, these largest events need to be considered in that light.

CHAIRMAN RYAN: One of the interesting ways to think about that again is in thought experiment. What's the probability of an event washing out the material every, pick a number, 50 years, whatever the right number is from the -- specialist.

MEMBER HINZE: Upper storm.

CHAIRMAN RYAN: Upper storm or something and I think that kind of exploration better informs however you cast the model. So that's the kind of thing I think we're --

MR. COMPTON: And it's there. You would also look at are there in the interim between those 100 year storms, those things that continue to display. So you have to look at all the --

CHAIRMAN RYAN: Could you pull the microphone over please?

MR. COMPTON: Turn it on. Right. So the point being I think you have to look at --

MEMBER HINZE: Our time is fleeting, Keith, but I have a couple of questions that I really would like to ask and one is you have treated Fortymile Wash here and I don't know. Are you going to apply this kind of modeling to the remove of ash that might go into Crater Flat and be carried down into Amargosa Valley and to a RMEI located there? Are we looking beyond this catchment basin of Fortymile Wash?

MR. HILL: Brit Hill, NRC. No, there is only one RMEI and that is prescribed at the southern boundary of the Nevada test site above the highest concentration of radionuclides in the groundwater plume. So we are not considering other locations for these sorts of calculations including the depositional area around Crater Flat.

MEMBER HINZE: As you have mentioned earlier, we do have evidence that there was strong tephra deposition in Crate Flat from Lathrop Wells.

MR. HILL: That's correct.

MEMBER HINZE: And I'm wondering whether any of that could reach down into the same depositional basin within this range where you could as Roland has mentioned have the wind the mass loading to the RMEI.

MR. COMPTON: Are you asking about the significance of that for ALN (PH) remobilization?

MEMBER HINZE: You will have it for fluvial as well because there is also the possibility of fluvial remobilization out of Crater Flat and into Amargosa Valley and I don't know if it locates specifically at the RMEI but in the proximity of the depositional area that you have. It's just a thought that I'd like to make certain that all of this is complete and you do, too, of course.

I guess my second question is what is DOE doing on this. How does your work compare with DOE and have there been any technical exchanges on this topic to exchange input parameters and evaluation of their parameters?

MR. COMPTON: I think that we're, we certainly haven't had since I have been the project officer technical exchange on this topic. I think that right now we're trying to develop our independent analysis of this and that's what I'm presenting right now. I'm probably not in a position right now to compare our results against what DOE's results are. But we're certainly keeping up with whatever they are publishing and putting out.

MEMBER HINZE: Are they using a mass balance sediment, mass balance approach, as you are?

MR. COMPTON: Don, can you?

MR. BENKE: This is Roland Benke at the Center. I can add a comment or two. The publicly-available information from the DOE was reviewed as part of the key technical issue agreement process and there are letters from NRC to DOE on that indicating an additional information need.

The most recent DOE analysis model report of this process is not publicly available, but I believe requests for it to be made publicly available have been sent to DOE and may be in the fiscal year. The AMR could be released to the public and NRC can review and comment openly on it.

MEMBER HINZE: Thank you, Roland. John.

MR. TRAPP: John Trapp, NRC staff. There isn't much more to say on that. Yes, there is a report that is nonpublicly available that we're trying to get which tremendously changes the way that DOE has looked at this. So when this becomes available, we'll be reviewing it, see how it affects, but right now, there isn't anything we can talk about.

MEMBER HINZE: We would like to stay alert to when that becomes available as well.

MR. TRAPP: The request has been in for a little bit over six months.

MEMBER HINZE: Thanks very much.

CHAIRMAN RYAN: Thanks, John.

MEMBER HINZE: Keith, if there is nothing else then, we thank you for your presentation. It was helpful to us, tantalizing in many ways. We're looking forward to more information as you complete the entire context and conduct your results and your sensitivity studies.

MR. COMPTON: Thank you for your questions and thank you for your patience when I wasn't able to talk about things.

MEMBER HINZE: And thanks too to the Center, Roland, Don and the rest. Thank you. Can we proceed ahead then?

CHAIRMAN RYAN: Sure.

MEMBER HINZE: With that, we move to the second presentation of today. We have with us, I guess I won't say once again, but we welcome John Kessler who is going to bring us up-to-date on their most recent analysis looking at the consequences of an intrusive igneous event at Yucca Mountain and, John, we welcome you here and this is a topic we have great interest in and have commented upon. So we're looking forward to it.

MR. KESSLER: Thank you for inviting EPRI to give us a chance to discuss some of the work we did last year and, yes, I'm back again like a bad something or other. The next view graph please.

I'd like to begin by acknowledging the people that really did most of the work on this report that I'm going to be discussing. Mick Apted from Monitor Scientific led the work. Megan Morrissey, Marcus Bursik, a lot of the igneous work regarding what an intrusion looked like and what kind of magnitudes are we talking about. Fraser King worked on the effects on magma intrusion on waste packages and Matt Kozak also from Monitor Scientific did the performance assessment for us on this.

Borrowing liberally from view graphs from

DOE whenever they do show up on the TV, never mind. Obviously, this intrusive release pathway is just one of the --

CHAIRMAN RYAN: Could you pull it just a little bit closer to you for the recorder?

MR. TRAPP: Sure, Mike. Is that better?

CHAIRMAN RYAN: Yes, it's fine.

MR. TRAPP: Okay. There are several event scenarios that you're all aware. You just heard about part of an extrusive scenario with fluvial redeposition of the tephra. What I will be talking about is the intrusive release pathway. Next view graph please.

Just as kind of a reminder, last year I believe Matt Kozak spoke to you on our igneous extrusive scenario work and just to remind you that our reasonable expectation case for that was zero release. We felt we weren't going to be failing waste packages with the reasonable kinds of eruptions to expect and that work was documented in the report that's listed here on this view graph and it is publicly available at the hideously long website address at the bottom there. Next view graph please.

So now onto the intrusive release. Again thank you, DOE, for providing some real nice graphics for us. What we're assuming for a conceptual model here (1) that we have magma coming up into the drift, (2) that we would expect the magma to enter the drift to some degree, and (3) that it will expect some sort of interaction. There will be some sort of interaction with the waste packages and there could be as is shown in No. (4) some kind of thermo-hydro-mechanical-chemical combined effect. Next view graphy please.

Good, it showed up fast. This is EPRI simplified cartoon conceptual model of some of things we thought about in terms of mechanisms and trying to divide this magma that might be entering a drift in various zones of influence so to speak or how might waste packages react to the magma. What you see here is we looked at are there thermal-mechanical impacts in the waste package. Are there thermal impacts? Are there chemical impacts for those couple of questions? We also looked at some of the impacts on the tuff itself which is discussed in the report but I wisely choose not to go into all of that today, knowing you would probably be behind already.

So let me just describe these three zones that you see in color on that view graph. We have the internal red zone which is where we actually see or are assuming magma fills part of the drift. Then we have some blue zone that will require definition which we do in the report which is some zone immediately beyond where the magma may enter the drift. Yet there is damage to the waste package and that's something we spent some time in the report discussing and trying to decide what extent that zone is and I will be talking to you about that. Then the green zone is far enough down wind, however you want to call it, of where the magma has entered that we think that the temperatures are cool enough, the gases are sufficiently non corrosive by the time you get that far down the drift that we assume essentially no damage and that beyond the green zone or in the green zone we would assume that the waste packages would follow a nominal release scenario type of effect. Next view graph please.

I think I've said some of what's on here. So that red zone is what's immediately adjacent to the rising magma dike. We're assuming that the drip shields are disrupted or displaced in some way and essentially for our modeling we just made them disappear. The waste packages in this red zone are fully engulfed by magma and that we assume that the Alloy 22 and the cladding are failed fairly quickly due to the very high temperatures that would occur in that red zone.

In the blue zone which I'll talk about in some length here, at this point I'm kind of summarizing but I'll go back and rewind and tell you how we got to define the blue zone by looking at various mechanisms, we're talking about significantly elevated temperatures, something above, in the range of 300 to 400 degrees C and corrosive gases as well. In this zone where the magma has not intruded, we would assume the drip shields are intact, but the temperatures are high enough such that the waste packages and cladding we assume have failed fairly quickly and I'll describe the mechanisms we considered so that we can define that blue zone and kind of back define it saying where is it that we would expect waste packages and cladding to fail quickly, how much farther down the drift from the magma is it.

Then that green zone where peak temperatures are something less than about 350 degrees C we make arguments to suggest that there's really no significant effect on the long-term EBS performance in that green zone for performance assessment. I'll discuss briefly how we reached that conclusion. Next please.

So the big question is how large are each of those zones. They are going to be functions of things like the magma ascent rate, the viscosity of the magma entering the drifts, temperature of that magma, the extent and magnitude of the hot corrosive gases that may be moving on down the drift ahead of the magma front itself. We also looked at how and to what extent can waste packages fail in these various environments, what's the nature of the radionuclide release from a failed waste package and then of course ultimately what are the incremental dose consequences and in this case we did compare it to the nominal scenario. Next please.

Okay. On to starting to try to answer some of those questions, magma ascent, we feel it's going to be representative of a hydrous alkali basaltic magma that's found in the Yucca Mountain region. Our understanding is that some of the previous DOE assumptions were that the ascent rate was maybe between .01 and 10 meters per second. With fairly high magma temperatures, temperatures that high imply you're going to have relatively low crystallinity and importantly fairly low viscosity, meaning if you have something with that high a temperature and that low a viscosity, the opportunity for the magma to move way down the drifts is pretty significant.

There was some work done by Nicholas and Rutherford in 2004, however, that suggests perhaps the magma is going to have a very different kind of characteristic. They provide information that suggests the ascent rate may be more at the low end of that range that DOE assumed and more importantly, the magma temperature is significantly lower, probably in the 975 degree to 1010 degree C range. In that range, the magma is going to be highly crystalline and highly viscous such that it will more of a rubbly flow than a fluid flow. Next please.

Again, I'm making a long story short here and there's more on the report on these issues, but in terms of the extent of the red zone, we view it as a plug flow and an aa-type flow, very rubbly, very high viscosity with viscosities up in that 10^5 to 10^7 Pascal seconds range at these expected temperatures of 975 degrees to 1010 degrees C.

As it moves down because it's very near the point it's going to go totally solid, it's going to freeze rapidly and we would expect that the magma will not get that far down the drifts. We think the extent of the magma engulfment will be on the range of zero to three waste packages on either side of the dike because of this highly viscous, rubbly, fairly low temperature magma that could be entering the drifts.

MEMBER HINZE: Excuse me, John. Can I ask you a question there? Is that based upon calculations? Is that based upon analogs? What is it based on?

MR. KESSLER: You're taxing my abilities here, Dr. Hinze, but I believe it is a combination of available data as well as calculations as to what a fairly wet magma that's an alkali-basaltic type magma would do as it ascends from depth up to the surface in terms of temperatures and viscosities, things like that. So it's a combination of the two.

The other relevant factor there is that the dike might intersect something like one to 20 drifts as it comes up. It's another assumption we made. Next.

The tricky one was the blue zone. What kind of range of blue zone environments are we talking about and how do we go about defining it? So a good chunk of our report really is going through all the mechanisms, the extent for interaction with the waste packages as well as the tuff and the rock around it, how far downstream or away from the magma front would that zone where the waste packages and cladding would rapidly degrade are. So how do we go about this?

First of all, we said that there could be some hot magma sprays or ballistic particles very near the dike but those effects are likely going to be limited to waste package surface heating and not much more than that. What we had to do was temperature was the key here and we came up with two different estimates of the thermal history. We looked at what we called the partial fill geometry and the cutting through geometry in terms of what does this magma look like and how might it affect temperatures in the near field. Next view graph please.

So let's start by defining that partial fill geometry. Sorry, the type is so small here. What you'll see really in this case what we assume is the magma is entering the drift from the left and that the dike is somewhere well off to the left such that the temperature or the heat is just from the magma that's in the drift itself. The waste package you see is really a long cylinder there. We assume that because of the very close spacing and good radiative heat transfer and conduction heat transfer that we could treat those waste packages as sort of an effective long waste package in terms of conducting heat down the drift away from this magma front, but yet that the dike that could essentially heat a whole plane of rock way off to the left in this view graphy, it's significantly far away that we didn't consider that in this particular case. Next view graph please.

Okay. Now the cutting through geometry is now we have that magma plane that's sitting right at the left and what we wanted to do was see how much of a difference there is in terms of what kind of temperatures we would expect in front of those different kinds of magma, one where it's just the magma in the drift itself and the other one where we have this whole dike full of magma where we have essentially a very hot plane or source. Next view graph please.

So we did some TOUGH2 evaluations to determine what kind of temperatures and relative saturations and things we'd have down the drift. We benchmarked it against a more detailed study that was done by Lore where the temperature was held at 1010 for five months and then allowed to cool. The Lore study is in the red and our TOUGH2 model is in the blue just so we could benchmark against something to our analysis.

So onto our results now for these two

cases, this view graph here shows you the waste package and gas temperature histories for the partial fill geometry. There are a couple curves here. So you have temperature versus time in both of these curves and what this is I have for various distances in front of that magma plug that's entering the drift. You see that within about one meter of the magma plug we have temperatures that rise after a couple months up to about 800 degrees C and then they fall off. That's the waste package.

The gas history is somewhat different in the sense that the gas is pretty hot right there at the edge of the magma but you see they both cool off such that by the time you're down 11 meters down the drift the peak magma temperature, I apologize for the yellow, you should never do things in yellow, is dropping off by a few hundred degrees there and by the time you get down about 114 meters the peak temperature is below 200 degrees and the gas temperatures are in the lower right one. Next view graph please.

This one is pretty busy now. This is for the cutting through geometry.

MEMBER HINZE: The gas history, this is internal gas.

MR. KESSLER: No, this is the gas in the drift.

MEMBER HINZE: In the drift.

MR. KESSLER: In the drift, gas temperature in the drift. Okay.

DR. MARSH: Air temperature.

MR. KESSLER: Yes, air temperature. Thank you. The next view graph has them combined. You see in the legend the top four. WP means that the waste package temperature at various positions away from this magma, a dike in this case, the planar source of magma, as well as the gas temperature in the drift which is what's labeled as the drift for the four in the length. And what you see is that temperatures are higher as one would expect in terms of positioned away from that magma source because now you have a whole plane of magma rather just the magma in the drift.

Conceptually, we assumed that what we get for temperatures is probably somewhere between these two cases just depending on where you're at down the drift. Now we're looking at a range of potential conditions that would exist in that blue zone, just beyond where the magma has filled the drift, but close enough to the drift that we have significantly high gas temperatures and waste package temperatures due to conduction mostly from where the magma is.

Next view graph shows some waste package isotherms for the two geometries. This is starting to get at really being able to define how long are those two blue zones either side of the magma and what you see is that for that top blue curve 200 degrees C isotherm goes out to about 80 meters ahead and takes a couple years for it to start cooling off once you have that dike in place.

But of course, what we're interested in is at the higher temperatures, what are the extent of those zones in terms of damage that might occur to the cladding and the waste package. You see that by the time you get up to 400 degrees C unfortunately in the hard-to-see yellow there, for the partial fill case, you're talking about maybe no more than 40 meters ahead of that magma front and for the cutting through case maybe about 50 meters ahead of the magma front. By the time you get to 500 degrees C, that extent is on the order of 30 meters ahead of the magma front for some fairly short period of time.

So now that we have some idea of what kind of extensive high temperatures we have, we go on now to think about the magma waste package temperatures in terms of the mechanisms. Next view graph.

MEMBER HINZE: Can I ask a question?

MR. KESSLER: Yes.

MEMBER HINZE: Can't we have both of these scenarios?

MR. KESSLER: Yes, they both exist together.

MEMBER HINZE: Right.

MR. KESSLER: In the sense that the dike --

MEMBER HINZE: Feeding in the super positioning of this.

MR. KESSLER: Right, it's a sort of a super positioning and we decided that we would be overanalyzing if we tried to get any fancier than what we've already been doing here in terms of taking these two cases and we admit to being a bit subjective here, but we wanted to look at ranges of what we would expect for temperatures to come up with ranges of estimates of effects and I really think that's about all that we're justified in doing in terms of available data and analysis here.

MEMBER HINZE: Good show. Thank you.

MR. KESSLER: Okay. On this view graph, I've quickly listed what's a fairly lengthy discussion in our report on the different kind of waste package and magma interactions we considered. The first one is erosion that is in that red zone. You may actually have the magma scraping past the waste packages, really literally eroding the Alloy 22 right off the waste package. We felt that that mechanism was fairly unlikely and that the magma flow is slow and limited in time. So we felt that mechanism really wasn't going to contribute much to waste package degradation.

Thermal sensitization on the other hand and the next one, enhancing subsequent aqueous erosion could be consideration. Frazier King in the report talks about this requirement, temperatures in the order of 600 degrees C or higher which occurs only for a short distance down the drift but nevertheless we could have some waste packages that are thermally sensitized now such that when the water does return to the drifts, we could have much higher or relatively higher degradation rates of that Alloy 22 than where the Alloy 22 is not thermally sensitized. There is one mechanism that could cause earlier waste package failure.

Next one, corrosion due to magnetic gases, we looked at what the gases were. Indeed they can be corrosive. We felt that a lot of those gases are going to wind up going into the rock rather than depositing on the waste packages themselves. Things cool fairly rapidly and what we felt was that we would have minimal localized corrosion due to the magmatic gases, something on the order of about 1/10th of a millimeter to maybe one millimeter and I have them reversed here. At about 1.1 meter down the drift, we have the 1.0 millimeter depth of corrosion and it drops off to about 1/10th of a millimeter when you get to about 55 meters down the drift. Please make that correction. Sorry, I have it backwards there.

So what we determined was that really shortens the overall waste package lifetimes only slightly. We did take it into consideration when we did our waste package failure distributions for subsequent performance assessment analysis.

The next mechanism was a creep that when you get these waste packages up to maybe 400 degrees C or so or higher that we expect the waste packages to creep since there's no magma holding them in place. We would expect it needs something like 30 percent creep to rupture a waste package for Alloy 22. Nevertheless, the creep rates when you get up fairly high can be such that we can't rule out that creep would occur and what Frazier concluded was that creep failure is possible for five to ten waste packages on either side of the magma plug.

MEMBER HINZE: Those temperatures related to creep, are those based upon tests of Alloy 22?

MR. KESSLER: Yes. Yes, there is available Alloy 22 creep data and strength data or similar high chrome, high nickel/chrome, alloys that we took that from and that's in the report, that kind of information.

The last one is rupture due to over-pressurization. Obviously, if we're heating up the gas inside the waste package, the pressures could rise and it's possible that we could have rupture due to over-pressurization and just to give you an idea of the kind of analysis we did there, the next view graph please is one of, I think I have, two or three on this pressurization. So the first thing we did was we took a look at how high might the internal waste package pressures go versus time at various positions downstream or away from the magma front there and you see that's what that figure is, the internal waste package versus time and how it decays at various positions away from there. Next view graph.

So we ran that into a mechanical model and looked at how much that waste package might get strained at the lid and deformed due to the internal pressurization. In this case, we looked at an internal pressurization of 0.69 mega-Pascals. That's 100 psi and the deformed shape there is exaggerated by a factor of 50. What's important to see is the strain, the little red zone there around the lid, that we started to consider in terms of how much strain do we think the Alloy 22 could take at that point. Next view graph please.

So here is our model versus available data, Dr. Hinze, just in this particular example. What we had was we looked for if we had a strain limit of 0.2 percent, that's that 0.2 percent offset, or perhaps compare that axial stress on the outer lid to 90 percent of the ultimate tensile strength and looked at the axial stress on the outer lid for a stress concentration factor of about 12, that stress concentration factor at the high end of what we think could happen, but again we're looking at the shape of the weld, how stresses might actually concentrate locally at that lid and the stress concentration factor is fairly high, so we did what we could to raise that curve, of course what we're looking at is where does that intersect, at what kind of temperature is that and then we can say if we have temperatures above that then we could have fail due to overpressurization which is what we assumed. Next view graph.

So to summarize the extent of these red and blue zones, for the red zone, the second row across, again that's waste packages fully engulfed by the magma, we think the extent would be anywhere from zero to 20 meters away from the dike plane. That would represent a total number of waste packages on both sides of the dike of zero to six waste packages because the spacing for each waste package is like 5.5 meters is the length of a typical waste package. We would assume the cladding has failed in that region.

For that blue zone that I talked quite a bit about where we decided that the waste packages would experience significant thermal impacts such that the waste packages would fail relatively quickly from a geologic perspective, that extent would be something like 37 to 66 meters from the end of the red zone and that would include a total number of waste packages on both sides of the dikes together of something like 14 to 24 waste packages and again we assumed in that zone that the cladding was failed.

The green zone where we said the temperatures were cooler the volatiles were around but for a very short period of time. We didn't expect there to be significant alteration of the waste package degradation rates relative to the nominal scenario where you don't have an igneous intrusion and that the cladding remains intact.

Now transitioning into some modeling of releases and then getting into the performance assessment results. I'm going to give you two examples here of our release rates in those zones for two radionuclides.

The first is iodine-129. What you see is that the blue zone has the highest release rate because we're not assuming the salts or the magma that's covering. The waste packages in the red zone allows any kind of hold up of the iodine-129 as it comes out. You see the red zone actually has somewhat of a delay and that's due to the effects of the magma actually protecting for a while or delaying the release of the iodine-129 through that engulfing magma as it gets out of the EBS.

And the green zone follows right on top of the N or the nominal case there in terms of release rates after the time of waste package failure. So that's the kind of general release rates that we're going to plug into our TSPA code. Here's an example for iodine-129.

Next view graph please for neptunium-237. Similar trends here again. In this case, the neptunium does absorb a bit on that magma which again causes the delay before reaching that peak in the red zone. But of course because the waste packages and the cladding are failed, the release rates are much higher than for the green zone or the nominal case, significantly higher and that's captured in this view graph for neptunium. Next view graph please.

Finally, throwing it all together in a pot here and looking at some conditional doses and I want to emphasize when I say conditional doses, these are probabilistic doses taking into account the range of parameters but assuming that the igneous event occurs. So it's conditional on the igneous event having occurred.

And these are doses to the RMEI at the compliance point, the 18-ish kilometers downstream and what you see here is assuming that in this case all of the drifts that the dike intrudes are completely filled. So essentially we have 14.4 percent of the drifts that are red zone and what you see are two peaks, first due to technecium and iodine. The next one is due to the actonides that in the range of 1/10th of a millirem a year up to a couple tens of millirem per year, maybe about 20 millirem per year for this kind of case where you might the magma completely filling all the drifts. This would be similar to our analysis of DOE's assumptions where they assume the magma is fairly viscous and all of the waste packages fail along the entire length of the drift that the magma intersects. Next view graph.

This is our expected value case, however, where we have F-Red or the fraction of the drift that's in the red zone we think is really more like five percent or 0.05 and the fraction of the drift that's in the blue zone is really more like 20 percent in our particular case and I'll show you a sensitivity or two. Again you see sort of the same shapes, but what you'll notice is that the highest peak is way out in time and that is due to the nominal case. What we're suggesting is that the nominal case still provides the highest peak dose for our expected value case where we have limited red and blue zones compared to assuming the drift that the dike has come up to, has intersected, is completely filled with magma. Next view graph.

Excuse me. Let's back up one before we get to this worst case one. What I want to point out is at the bottom there. We said "To rival the nominal case peak dose, in other words, to get that second peak up as high as the nominal case peak dose, we had to roughly triple the depth of the red and the blue zones which we think is sort of at the upper end of what we would expect in terms of the effects of the intruding magma. Just to kind of give you an idea is how large or how much of an effect does there have to before even for this conditional dose case that the conditional doses rival the nominal case. Okay, in the worse case --

MEMBER HINZE: Help me. What does the 0.15 refer to then?

MR. KESSLER: That 0.15 says that we're assuming that 15 percent of the length of a drift is in the red zone.

MEMBER HINZE: Okay.

MR. KESSLER: And that 60 percent of the length of the drift is in the blue. So we only have 25 percent of the drift that wouldn't be affected in that case and we're saying it takes that much for the doses to rise enough to rival the nominal case dose.

In the worse case, conditional dose I have, next view graph please, is that blue zone. We don't have it covered with magma. The waste packages and the cladding have failed and if we assume that all the drip shields are not functional, again really tying a lot of arms behind our backs here, in this case we can get peak conditional doses on the order of a couple hundred millirem for this particular case. That's 14.4 percent of all the drifts are in the blue zone. They have all failed. There's no drip shields. For the conditional dose case, we get up in the hundreds of millirem.

The take-home line is at the bottom there. The peak probability weighted dose, remember because we're talking about the probability of an igneous intrusion occurring, that dose rate is going to be much, much less than the nominal case. We had some general discussions about how you convolute that probability for assuming it occurs in time alla the way that DOE does it. But we're confident that when you finally get away from the conditional dose case and do the fully probability-weighted dose case that the dose risk contribution is going to be small even from this bounding case. Last view graph please.

So our conclusions are that the extent of magma intrusion into the drafts is likely to be quite limited, maybe something like zero to six engulfed waste packages. Adjacent to those engulfed waste packages, we may have something like 14 to 24 waste packages that will likely fail early due to those high temperatures and various effects that we looked at. The probability-weighted dose rates due to magma intrusion scenario are less than the nominal case. So if we combine that with our earlier igneous eruption work, we reach the conclusion that the igneous scenarios don't appear to be as significant or I guess I should probably say a dominant contributor to the overall dose risk. And the report on this igneous intrusion is available again at that hideously long website that's available and it's shown at the bottom of that view graph. Thanks.

MEMBER HINZE: Thank you, John. That was an excellent presentation. We'll start with Dr. Weiner.

MEMBER WEINER: I just have a couple of questions, John, because I'm going to defer most of my questions to Dr. Marsh who could ask them better. What do you mean by failed? Do you mean everything goes or the cladding fails, the package fails, stuff is available for mobilization?

MR. KESSLER: Good question. We have not assumed that the waste package disappears and that the UO_2 pellets are sitting there on the bottom of the drift. What we've assumed is that there is a failure along one of the lids and that water has to migrate, diffuse, in and around the lids. The cladding is split, but it has to enter then into the cladding and diffuse out of the cladding and then on out of the EBS. That's what we're assuming for our source terms model.

MEMBER WEINER: So roughly what fraction of what's contained in any fuel rod do you assume gets out or a fraction of the total inventory, whatever, because under that scenario, you're not going to eliminate everything, you're not going to release everything that's the waste package, are you or aren't you?

MR. KESSLER: We assume and we're revisiting this a fairly short waste form alteration time in the sense that takes UO_2 to go to U-308 or something maybe is a couple thousands years. If we assume invection, I talked more about a diffusive release pathway, but if we do have invective flow through there, our alteration times are such in that for the higher ranges of flow rates we can actually assume we've released 100 percent of the inventory.

MEMBER WEINER: Okay.

MR. KESSLER: And of course, it just depends on what we assume for the amount of release.

But of course, we're factoring neptunium solubility and things like that. So it's a range. Sorry for saying it depends.

MEMBER CLARKE: Excuse me, Ruth. Can I follow up on that?

MEMBER WEINER: I still had one more.

MEMBER CLARKE: I just want to clarify what you two just said to each other. Once the waste package is damaged, then you're into a situation much like a nominal release. Is that it?

MR. KESSLER: Much like a nominal release, yes.

MEMBER WEINER: When you speak with of conditional dose, does that include some kind of conditional probability term?

MR. KESSLER: Yes, really it's saying --

MEMBER WEINER: So it's a risk.

MR. KESSLER: It's a risk, right. It's a conditional dose risk still. The only thing we haven't factor in is the probability of the igneous intrusion event occurring.

MEMBER WEINER: Yes.

MR. KESSLER: Again, we're saying assuming it occurs what's our dose risk with the distribution of parameters we have for everything else that would result.

MEMBER WEINER: So using the risk triple, you're saying assuming that it occurs what is the scenario of this particular --

MR. KESSLER: Right. What are the scenarios? What are the ranges of neptunium solubilities? What are the ranges of invection that could occur, all of those things and the probabilities? That's still all factored into these view graphs, these conditional dose risk view graphs that I showed you.

MEMBER WEINER: And my final question is are you planning to consider the new DOE TAD package liner? Or would that make any difference?

MR. KESSLER: At this point, we don't feel it would make any difference for this kind of analysis at the degree of sophistication that we did this analysis. We're assuming and again obviously we'll have to wait to see what gets designed for the TADs that the Alloy 22 waste package overpack will look similar to the existing waste package disposal container that DOE's designed. The TAD is just the inner canister. Of course, there could be differences. That could change our analyses on things like overpressurization and creep and things like that. But right now, we're not planning to change anything from what we've done here based on the TAD concept which is still very nascent.

MEMBER WEINER: Thank you.

MEMBER HINZE: Dr. Croff.

VICE CHAIRMAN CROFF: In the opening parts of your presentation, you used the word "assumption" a lot. I think with respect to overpressurization you had some subsequent analyses you showed us. What about for example the cladding failure? How much is that? Is that just, I'll call that, an arbitrary assumption or is there something behind it that causes you to believe that the cladding will fail, the drip shield will be displaced and this kind of stuff?

MR. KESSLER: A good question. Due to time limitations here, I skipped over quite a bit of the detailed analysis of the waste package and magma interactions. We do have a lot more detail in the report.

An example of your cladding question. Yes, there are data available that suggest the cladding is going to rupture when you get up to temperatures above 500 degrees, 600 degrees C, something like that fairly quickly and that it can creep a temperature somewhat below that. Creep rates when you get down to like 400 degrees C are very slow, but does creep slightly.

EPRI has a very active program, in fact, cooperation with NRC research and DOE on looking at those kinds of cladding properties. So, yes, there's a lot of data there. I talked to Dr. Hinze about where we got the assumptions about the magma properties. So, yes, we do have a basis. It's not just guessing at some things behind these mechanisms that I just didn't get a chance to get into in the discussion here.

VICE CHAIRMAN CROFF: Okay. Thanks.

MEMBER HINZE: Dr. Ryan.

CHAIRMAN RYAN: Thank you. John, thanks for a good presentation. Just one further clarifying question on the dose curves.

MR. KESSLER: Yes.

CHAIRMAN RYAN: I assume we're looking at the mean value of your realization.

MR. KESSLER: Yes.

CHAIRMAN RYAN: Okay. Not the 95th percentile.

MR. KESSLER: Correct.

CHAIRMAN RYAN: The 50th percentile.

MR. KESSLER: Yes, those are the means.

CHAIRMAN RYAN: Okay. Great. Thanks.

MEMBER CLARKE: Thanks, John. Very nice presentation. Dr. Hinze asked you earlier about the cutting through and the partial fill geometries. Are they both in the analysis?

MR. KESSLER: Yes.

MEMBER CLARKE: This is a result. These doses are a result of both of these scenarios.

MR. KESSLER: Right. For example, the ranges of the red and the blue zones come about because again this is the arbitrary part that we're making some assumptions about how we would superimpose these two cases and what that might mean for the real extent in the case where you do have a magma dike but you have some plug ahead of it. It's somewhere in between these two cases and that's why we started applying, one of the reason we applied the ranges along with we're not exactly sure when we're going to have creep failure and when we'd have overpressurization failure. I think there are obviously uncertainties there. So that's where are our subjective expert judgment came in to come up with those ranges.

MEMBER CLARKE: Okay. And then just to clarify what I asked you earlier, once a waste package is damaged you have a water-borne release into a transport model through the groundwater pathway to a receptor.

MR. KESSLER: Yes. Sorry I didn't make that clear for you.

MEMBER HINZE: Bruce.

DR. MARSH: Yes. One of the key ingredients of this analysis is the calculation or assumption of this high viscosity.

MR. KESSLER: Yes, it very much is.

DR. MARSH: And you base this on Nichols and Rutherford's experimental work.

MR. KESSLER: Yes.

DR. MARSH: Their experimental data just to refresh you a little bit shows that they determined using the assemblage of minerals they see in the lava, for example, Lathrop Wells, that that was an equilibrium at about 200 mega-Pascals, about two kilobars, five or six kilometers down in the earth that had three or four percent water and it had a fairly low percent of crystallinity and in other words, in the crystallization range -- that was down at maybe 10 or 15 or 20 percent crystals. So the viscosity at that point, 975 to 1010 for example, would be really low with that amount of water, three to four percent water. It would be like maybe 100, maybe 500, but it would be pretty low, really low, maybe even a little lower.

Now if you take that and actually translate it right to the surface, one atmosphere, that melting range of course changes. The melting range at 200 mega-Pascals is much lower because it has water in it. As it degases, the melting range actually enlarges and it goes up to higher temperatures. It stays about the same wetness and things.

So there is a major question. In other words, if you take that temperature that they gave, roughly 1000 degrees and use it to calculate the viscosity on the surface in the melting range, you end up that it's really near the point of being 100 percent solid. So you would get these number like you show here, these high values. However, there is a major question here in terms of when the magma actually moves from that point of five kilometers down to whatever to the surface, exsolves gas out of it. Water is actually is zero solubility near surface. The temperature and pressure trajectory that that thing takes is actually somewhat open to question in terms of what's going on.

So a better thing to do would be to know that trajectory and that's somewhat of a difficult issue in some ways. But the other aspect is is to have some other controls by looking at the magma on the earth's surface knowing something about the temperature that actually came out, not using Nichols and Rutherford's data at depth, but actually using a geo-thermometer and looking at it in detail on the surface like the -- for example.

I didn't see any of this in your report and I wasn't quite clear.

MR. KESSLER: Right.

DR. MARSH: There's a gap here. I could understand how you could get to that number based on the scenario I just took you through, but there are significant uncertainties in this and things that we don't know as yet and I didn't see those in the report. Are there other things that you took under consideration here?

MR. KESSLER: No, I think that, I'm sorry. Meghan's not here.

DR. MARSH: Yes.

MR. KESSLER: Meghan Morrissey who contributed this piece to the report because anything I say, I'm going to get myself in hot magma real fast here. So I don't recall either a lot of discussion, justification, for what we came up with. We assumed some certain trajectories. I don't recall how much we talked about the uncertainty in those trajectories.

Can we go back quickly to Slide 25 please? Thank you. One thing we did do in this one. This is our uncertainty case. This is the case where we assumed every drift that gets intersected by the dike is 100 percent in the red zone and so this is what we got for our conditional dose risk versus time case and you see that we do have a peak in a couple of tens of millirem for the conditional dose case.

So when I asked myself we could go back and sharpen our pencil on this, maybe it is more fluid. We don't know the trajectory just like you're saying. I look at this view graph and of course, I have to be very aware of the uncertainties in this view graph, but I would say that if I believe in this view graph and it's a conditional dose, I would say if I multiply by the probability of the igneous event occurring I knock those doses down to less than a nominal case and then I have to ask myself why I would want to sharpen my pencil on this particular issue.

But I agree. There is uncertainties. We didn't talk about them as much as we should have. But

I'm not sure whether it's worth exploring I guess from a performance assessment standpoint.

DR. MARSH: There are obviously worse -- If you're going to use that as an example to start off your analysis, it's worth knowing well. So it's worth taking a look at I think. Thank you.

MEMBER HINZE: Any other questions? Staff. If not, we have reached our limit of time and you've really helped us and done an excellent job of getting your points across very succinctly. We appreciate it and it's very helpful. We'll be exploring your document in a lot more detail I'm sure.

MR. KESSLER: Thanks again for the opportunity to share it.

MEMBER HINZE: Mr. Chairman.

CHAIRMAN RYAN: Thank you, Bill. We'll take a short break and reconvene at 11:00 p.m. with our letter writing activities. We'll conclude the record here or do we need to -- We'll conclude the formal record here but we'll reconvene at 11:00 a.m. Off the record.

(Whereupon, at 10:48 a.m., the above-entitled matter was concluded.)