Official Transcript of Proceedings

NUCLEAR REGULATORY COMMISSION

Title:	Advisory Committee on Nuclear Waste 176th Meeting
Docket Number:	(not applicable)
Location:	Rockville, Maryland
Date:	Wednesday, February 14, 2007
Work Order No.:	NRC-1440 Pages 1-339

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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE (ACNW)

176th MEETING

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WEDNESDAY,

FEBRUARY 14, 2007

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ROCKVILLE, MARYLAND

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The Advisory Committee met at the Nuclear Regulatory Commission, Two White Flint North, Room T-2B3, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., Michael T. Ryan, Chairman, presiding.

COMMITTEE MEMBERS PRESENT:

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

ACNW STAFF PRESENT:

NEIL M. COLEMAN, Designated Federal Official

JOHN TRAPP

JOHN STAMATIKOS

ALSO PRESENT:

STEVE SPARKS

BRUCE CROWE

EUGENE SMITH

KEVIN SMISTAD

GREG VALENTINE

ANDY WOODS

FRANK PERRY

ART MONTANA (via telephone)

JOHN KESSLER

MEGHAN MORRISSEY

LEON REITER

DON HOOPER

I-N-D-E-X

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

(8:33 a.m.)

CHAIRMAN RYAN: I ask everybody to come to order, please.

This is the second day of the 176th meeting of the Advisory Committee on Nuclear Waste. During today's meeting the Committee will continue to conduct a working group meeting on the igneous activity white paper. This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act, and Neil Coleman is the Designated Federal Official for today's session.

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 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 phones or pagers that you kindly turn them off.

Thank you very much.

I want to just first start by saying we really appreciate everybody's efforts, both our guests and staff and consultants and all for making the effort to be here again today, and we appreciate everybody's patience with dealing with the whether schedule and the tough travel conditions. So thanks very much to all for every effort.

With that, I'll turn it over to Professor Hinze, who is probably going to have a further word on our adjusted schedules, and we'll go from there. Professor Hinze?

MEMBER HINZE: Happy Valentine to you, Dr. Ryan, and everyone else.

CHAIRMAN RYAN: You, too, as well, sir.

(Laughter.)

MEMBER HINZE: I'm sure this is -- you're looking forward to spending your Valentine like this. While I --

CHAIRMAN RYAN: Couldn't think of a better way.

(Laughter.)

MEMBER HINZE: I do want to welcome you to the second day of our working group on the igneous activity white paper. Due to the early adjournment yesterday that was mandatory, and the fact that there are commitments by all for tomorrow, we're going to have to compress yesterday afternoon's activities that we had planned and today's activities into today's work.

As a result, we're going to be shortening some of the talks, but we hope that we will not in any way be -- detract from the discussion and questions that follow them.

We will try as much as possible to stick to the schedule starting where we left off yesterday afternoon. We may have to improvise, because of the availability of the presenters. And so we may not have this quite in the order that we would prefer.

I want to remind you of two things; first, that --

MR. TRAPP: Hello?

CHAIRMAN RYAN: Yes, hello. Good morning. This is the ACNW meeting.

MR. TRAPP: Yes. Is anybody there yet?

CHAIRMAN RYAN: We're all here, and we're in session. Could you identify who you are and where you're from, please?

MR. TRAPP: This is John Trapp.

CHAIRMAN RYAN: Hey, John Trapp. Anybody else on the bridge line?

PARTICIPANT: Yes. The Sanford Nuclear Waste Regulatory Analyses is connected.

CHAIRMAN RYAN: Any anybody else?

(No response.)

Okay. Thank you. Back to Professor Hinze.

MEMBER HINZE: Well, two things I want to remind us of is that we are looking for your comments and revisions on the omission and commissions here. Is it commission or omission in the white paper? And I also want to remind you that we're looking forward to any written comments. We will, unfortunately, need those comments by a deadline of March 1st.

And if there are no other comments to be made, or announcements, we will proceed immediately, and we will move directly into where we left off yesterday, and that is with Eric Smistad from the Department of Energy.

Sorry.

CHAIRMAN RYAN: No, I was going to -- go ahead, if you want to finish your introduction. I was just going to ask one question.

MEMBER HINZE: Eric Smistad is in charge of the igneous activity in the Department of Energy, and he will be discussing the nature and prediction of igneous activity. And in a few minutes, he'll be back with us discussing consequences.

After he has completed his presentation, we would like to open up the question and comments regarding Eric's presentation, as well as that of Jack Davis and John Trapp yesterday. I don't know that we really finished up with that.

CHAIRMAN RYAN: Thank you, Professor Hinze.

One thought that we left rather abruptly, because of the cancellation of the rest of the day yesterday was John Trapp was talking about some interesting ideas. And, John, I know it's going to be difficult to have a dialogue now, but maybe sometime we can put this on -- Bill, on your question and answer period list to talk about.

I was taken by, and think it's important, that we continue to explore the point that John Trapp made that there's a different way of thinking a little bit about these kinds of issues and things, if you're thinking from a point of view of an applicant, the point of view of a regulator, or kind of a strict, more scientific-based view of, you know, ongoing research in an area of interest. And I thought that would be helpful to get folks to talk a little bit more about from their respective points of view.

I think that would be helpful to gain

insight as to how people are viewing and thinking about this, and I think that's an important matter, because on the one hand clearly the NRC staff is interested in the very best science they can gain, gather, and understand, yet their role will not be to defend the science. It will be to evaluate the representation of DOE's presentation of the science.

And I think it's helpful if we get particularly the staff's views on what those differences mean to them and how they think and how they prepare, and what range of issues they look at and how they look at them.

I hope that's a helpful question, John, and others. If we could maybe evaluate that somewhere in the day today, that would be great.

MEMBER HINZE: Excellent.

MR. TRAPP: Yes, this is John Trapp. I fully agree with you there has to be a different way of thinking by the various people, and clarifying that would make a lot of sense.

CHAIRMAN RYAN: Thank you. Bill, I'll leave it to you to maybe pull that in, or others to pull that in as they may want.

MEMBER HINZE: Yes, I'll call on you and we'll get this started at the appropriate time.

A couple of other announcements. I understand that the overheads of Eric's presentation are now available in the back of the room. And also, there is a revised agenda that I hope most of you have already seen, and you can identify that by the fact that the number 11 on it is Eric Smistad giving his views. Eric, it's yours.

DR. SMISTAD: Thank you, Bill. Good morning. I'm probably going to be able to help out quite a little bit with the compression of the schedule. I have very few slides here.

I would like to say, just before I get started, that we recognize that a lot of effort went into this report. I participated in a lot of white papers over the years, and this is a fairly lengthy white paper, and it went into a lot of different areas, a lot of different work by a lot of different folks. And we recognize that it's a lot of work to do these kinds of things.

We felt that at the level that the report was written at it did a reasonable job of capturing the work that we've done through time. It didn't obviously go down into the real depths of our work, but at the level that it was written it did a good job of capturing what -- the work we have done.

We also recognize, and I know that the Committee does, too, and the authors as well, that it is a snapshot. We are continuing to do work. We mentioned yesterday or I mentioned yesterday we are going to be putting out a suite of AMRs towards the end of this FY in a staggered fashion. So the report couldn't capture the stuff that, you know, it didn't know about.

These are high-level observations you'll see today. And I will be providing the Committee with more detailed comments. We have several pages of detailed comments that we've gone line by line, identified the line and the comment. Those were put together by myself, Greg Valentine, Frank Perry, and Kevin Coppersmith. So I've just culled those together in a table. And as soon as I work them through the system, and the project, we'll get those out.

Okay. Just a few -- I just picked a few high-level -- or a few of the comments out of the table that we've generated and put them just into a few high-level bullets here. I think Kevin may have made this point yesterday, but there was a reference in the report about perhaps that the probability may go down as a result of PVHA-U.

The point here is that we don't know where that number is going to go. We haven't got to the -- you know, the cranking stage yet, so we just don't know where that number is going to end up. And I think we just want to make that clear, despite the fact that maybe the authors felt that they had an idea where things may be headed.

The report does cite -- I think it's in the executive summary -- areas of disagreement between DOE and NRC, particularly on these topics of multiple vents, dike length, and dike width. Speaking just for the DOE now, we do allow for multiple vents. I believe it's one to three. We have a range. It's heavily weighted towards one, but we do allow for a range. I think the report might have suggested that we were allowing for one.

Dike length -- as far as the PVHA-96 dike length range, and the latest that I've seen from the NRC work, I believe it was 1994, the ranges are very similar. There is a really hefty overlay up in those ranges. So in a practical sense, in an applications sense, there's really not a lot of difference between those ranges.

Dike width, there is some difference. The ranges do overlap between DOE and NRC. But at least in our work dike width is not a particularly sensitive parameter.

And, again, I guess I might want to just comment, I should have said it in the intro, that we only looked at the portions of the report that dealt with our work. We weren't looking into the work of others.

As far as our conceptual model of magna generation, the report did touch on it a bit. It wasn't absent from the report. We felt as a group that perhaps the report could have gone into more detail there. It wasn't do or die, but there are some aspects of our conceptual model that are important, and these are -- this is -- goes at the understanding of these dashes here -- the aspect of waning volcanism, low volumes, low frequency, and these volatile-rich magmas.

There is also -- and I think this came up yesterday in Gene's talk. We, in this AMR -- I've got the reference here -- it's our framework AMR from 2004. We talk about what we feel might be inappropriate links to volcanic fields further north of Yucca Mountain. So this is just one aspect of the report that could have been fleshed out more. It's just a suggestion. It's --

MEMBER HINZE: You are hitting right where you should. Thanks so much.

DR. SMISTAD: Okay. Another high-level comment was on the aspect of PVHA-96 and the timeframes that were emphasized. The report seemed to suggest that, you know, we didn't -- the Miocene was not included. It was included, it was considered, and it was low-weighted, although it was -- the results were heavily weighted in the Plio-Pleistocene.

Okay. Just quick conclusions here. We felt the report did, at the level it was written, capture -- you know, capture the work that we've done through the years. It was a snapshot, and I think we all recognize that. I've just presented a few high-level observations, and I've tossed in a couple of details, I'd say more detailed observations that we had.

There was perhaps a little misunderstanding on the basaltic episodes from our work translated into the white paper, and you can see that here. In contrast to what the report had, at least our understanding of the way it read to us, is that we don't have -- there was no episode between five and seven million years ago.

And in contrast to the report, the Miocene volcanism ended seven million years ago as opposed to the report stated at eight. And this will be in our detailed comments, so, you know, I can get you the line number and all that where we saw that.

MEMBER HINZE: I put a find on those numbers, and I couldn't find where we had said five to seven million years.

DR. SMISTAD: Okay.

MEMBER HINZE: So your detailed comments are essential to us.

DR. SMISTAD: Yes, it may have been -- you may have -- instead of numbers, you may have used the -- yes.

And then, one more, there was the bore hole 23P that Nye County drilled. They encountered a basalt there, and we dated that basalt. The report suggested that there was not, you know, an anomaly associated with that basalt. In fact, we believe there is an anomaly associated with that basalt.

So it's just another example. And, again, we've got that in our detailed comments.

And I think that may be all that I have. That is it.

MEMBER HINZE: Well, thank you very much, Eric. Does anyone on the Committee have questions?

(No response.)

Let me ask a question, if I might, about this dike length. I guess when I look at the document I see that it's rather schizophrenic with regard to that. We talk about -- and people do talk about dikes and the disk shape of dikes, and, accordingly, the aspect ratio. And then, we look at the work that Greg and others have done that we talk about the popsicle stick dike configuration, where the width of -- where the length of the dike is really rather small --

DR. SMISTAD: Right.

MEMBER HINZE: -- compared to the dikes that are commonly described in the literature and in other parts. Can you help us? Can the NRC help us to clarify this issue?

DR. SMISTAD: As far as our documentation goes, I might ask Greg to talk to that. It's his AMR, if -- did you catch the question, Greg, or --

MR. VALENTINE: Yes.

DR. SMISTAD: Okay. Or Frank. Either one of those guys can handle it.

MR. VALENTINE: Yes, the popsicle stick phrase is not mine. That was --

(Laughter.)

We had a paper in Geophysical Research Letters that was published in June or July of last year where we talked about eruptive fissure lengths, and part of the confusion is that in that paper we're talking specifically about eruptive fissure lengths. And that is a reflection of dike length, but it's not exactly the same as dike length at depth.

So we do think that the dikes probably length at depth. We're not sure exactly what their shape is.

MEMBER HINZE: So --

MR. VALENTINE: I mean, the penny shape is something that has been used in sort of theoretical approaches. But in reality, when you have a dike that's rising through heterogenous crust, and so forth, it might be more complicated.

MEMBER HINZE: So a disk-like type at depth is what you're saying? And then -- to a feeder, and then broadening out again into a fissure near the surface? Am I understanding that correctly?

MR. VALENTINE: Well, I think that they probably have a -- to first order have a rounded top, but there might be second order irregularities on that because of heterogeneities in materials. And it's not -- you know, I don't think we envisioned these as being perfect penny shapes either, but they probably do extend deeper than their length. But, you know, the popsicle stick is a little bit of an exaggeration probably.

MEMBER HINZE: Yes, I heard that on your field trip, your field trip with --

MR. VALENTINE: Yes, that was one of the PVHA members that said that.

MEMBER HINZE: I actually used -- it's very easy to visualize, so --

MR. VALENTINE: Right. I actually used -- when I was representing it, I used a stick from a Dove ice cream bar. They're a little bit fatter on top.

MEMBER HINZE: All right. All right.

(Laughter.)

You're picking the right kind of food there.

I think we've got to do -- we've got to clean this up in the white paper, because it -- there is confusion. And I'm hoping that your detailed comments will --

MR. VALENTINE: Right.

MEMBER HINZE: -- help us to do that. I'm going to take a shot at another question, too, if I might. And this -- I was looking around for Gene Smith, and I guess he has escaped on us. But Gene was -- and this deals with the nature, and I don't know if this is appropriate to direct to DOE or NRC or whoever, but this business of the relationship of the volcanoes to the topographic elevation, whether they can occur on topographic highs or not.

In Gene's presentation yesterday we heard quite a bit about the Lunar Crater, the Reveille Range, volcanism as occurring at -- and they occur on the highs as well as on the -- in the basins. And if we move all the way down to Death Valley, I took note of one of the slides that I think it was Bruce Crowe showed yesterday, where the volcano was really up high on the ridge.

And I'm wondering if someone could help us in terms of material for the white paper that would tell us why we might expect to find volcanoes on topographic highs to the north and the south of Crater Flat, and yet the strong propensity for the volcanoes in Crater Flat.

I also realize that two of the eight of the Quaternary volcanics in the Yucca Mountain region are on topographic highs. But I thought a brief discussion of that would be helpful to us in cleaning up the white paper.

DR. SMISTAD: Okay. Yes, I mean, clearly they do occur on highs, and they occur in blocks. I think just in the region, the preponderance is in -- you know, in the lows or in the valleys. But statistically, I don't know what -- you know, what the statistics are, but I think it's that a majority are found in -- I don't know if anybody -- Frank or even the NRC could comment on that.

MEMBER HINZE: I was thinking more in terms of the driving mechanisms, the processes here, rather than the statistics, because we do know they occur in the blocks, they occur in the high.

Maybe Britt or Greg or someone else could help, because I think there's a lot of confusion among the -- I don't want to say the tourists, but there is confusion on this issue.

MR. HILL: Britt Hill, NRC staff. I'd be glad to give a very simple perspective on this issue. It I think fundamentally arises from, if you view magmas as an overpressurized fluid in the shallow crust or a fairly neutral fluid in the crust.

When you get up in that order of maybe five to 10 kilometers in the brittle crust, if you have a significant overpressure in the magma system, to where the magma is actively forcing the rock apart, these variations in topography really create very small variations in lithostatic load.

The magma pathway would be dominated by the local structure and local stress-strain relationships, not by very small horizontal variations in vertical load. So I think the coincidence that we're seeing is that most topographic highs in the basin range have relatively fewer faults than the adjacent basins, which have more faults or have undergone more extension and thus present a more favorable pathway for magma ascent.

We did compare a number of volcanic fields -- Quaternary, Plio-Quaternary, volcanic fields -- in the Western Great Basin. That was in one of our semi-annual reports back in the 1990s. And we found that all of these analogue -- or all of these Western Great Basin volcanic fields overcome a topographic gradient on the order of hundreds -- 400, 500 meters, up to over 1,000 meters of topographic relief.

Now, certainly some, like Lunar Crater, have more of a tendency. The Pliocene Lunar Crater volcanoes tend to be more in the highlands. The few volcanoes we have in the Yucca Mountain area tend to be more towards the basins. But just like we see in Yucca Mountain, the two Quaternary cones -- Sleeping Butte and Hidden Cone -- are up there on a large topographic high surrounded by topographic low.

And I think those may be not the most frequent, but certainly they are not rare exceptions. Those are showing us the large effect of structural control relative to the very minor effect in small variations in lithostatic load. That would be a simple summary from my perspective.

MEMBER HINZE: Let me ask a follow up to that. Is there anything involved with the processes that are involved in like the Death Valley or Lunar Crater/Reveille Range that are different in terms of that which is in the Yucca Mountain region? Are we talking about extension rates? Are we talking about volume of magma?

MR. HILL: Well, there are certainly very important distinctions between the shear dominated Death Valley system and the oblique shear, transtensional dominated Yucca Mountain system in terms of how you can accumulate differential stress in the crust, and how that stress may be accommodated through time.

I don't think -- I certainly don't have a good understanding of the feedback between the accumulation of crustal stress and the relationship to strain for magmatism. But based on the work of people like Parsons and Thompsons, we know there is some relationship. And I think when you compare a place like Death Valley, you would have to be mindful that the stress-strain relationships in Death Valley are fundamentally different in both magnitude and in process from what we see in a place like the Crater Flat Basin.

MEMBER HINZE: Well, thank you, Britt. I really felt that in view of the comments that we heard yesterday from Gene that that -- something about this needed to go onto the record, because of this tendency -- statistical tendency for the volcanoes, at least in the Yucca Mountain region, to occur.

MR. HILL: Very good.

MEMBER HINZE: Greg, please.

MR. VALENTINE: Okay. And I forgot to say who I was the first time. It's Greg Valentine, and, yes, it actually is Valentine.

(Laughter.)

From Los Alamos National Lab. I think our view is overall consistent with Britt's. What we put forward in this paper that was in Geophysical Research Letters is the idea that to first order the location of a volcano of one of these monogenetic volcanoes is determined by the location of the mantle source that is feeding the magma. So that's the primary control.

And structure and topography -- topography on the surface, obviously, and structure in the shallow crust are second order controls, but they can give you a second order effect on where a vent might lie. But the first order depends on the location and the aerial extent of the mantle source, the pocket of magma in the mantle that is feeding the event.

So if that source area, or footprint as we call it, is beneath the topographic high, to first order, the dikes are going to rise and come up through that topographic high. And Hidden Cone, which came up right on the side of Sleeping Butte, you know, maybe 300 meters above the surrounding terrain or something like that is an excellent example of that.

And that is also a piece of evidence that the dikes that are feeding, at least at shallow depth, are shorter rather than longer, because if they were long they would have vented in the low terrain that they -- that would have been intersected first.

So now there was also a paper published by our team, Ed Gaffney and Bronco Demiatic, that also came out in Geophysical Research Letters last summer that looked at the effects of topography. If you have a dike rising and it intersects a topographically low area, that paper actually looks at the effects of conduit localization induced by the topography. So that's additional information for you.

MEMBER HINZE: Thank you very much. We needed to get that on the record. Thanks again.

DR. SMISTAD: I believe you guys have the papers, I think.

MEMBER HINZE: Yes, we do.

DR. SMISTAD: Okay.

MEMBER HINZE: Yes. But I really wanted to get that onto the transcript here in view of some of the discussion yesterday, which is -- really needed an explanation.

Bill or -- any questions? Or Bruce? Anyone else have any questions?

We do have a few moments here. Dr. Ryan, perhaps while Eric is here, and while John is on the line, this is an appropriate time to take up your question about the differing and relative roles of regulators versus the scientific aspects, and so forth. Would you like to start that discussion?

CHAIRMAN RYAN: Sure. If everybody is comfortable that I don't need to restate the question, I'd sure be happy to have, John, you start, or anybody you might want to call on here, or -- John Trapp. John Trapp?

MR. TRAPP: I couldn't quite get the thing -- I'm not getting very good reception for most of --

CHAIRMAN RYAN: I'm sorry, John. We're kind of back to the question I raised earlier, and if you'd like to maybe offer any comment or insights to start us off, that would be great.

MR. TRAPP: I think one of the primary insights I need to put in there is by law, by regulation, whatever you want to call it, our primary focus is "safety." Do they meet the regulations, this type of thing. Yes, we want good science. Yes, we want to do all this other kind of thing. But we are supposed to be assuring that the applicant is meeting the regulation.

And this, at times, may involve conservatism. This, at times, may involve shortcuts, possibly. But if we can demonstrate safety, we have done our job.

CHAIRMAN RYAN: That's an interesting point, John, and I guess I'd offer you maybe a friendly amendment word is maybe it's not shortcut, but shorthand.

MR. TRAPP: I'll accept that.

CHAIRMAN RYAN: Okay. And I think -- you know, I think I appreciate the fact that that means that your modeling or your assessment techniques, tools, or approaches may be different, because you're reviewing something rather than trying to create something. Is that a fair way to think about what you're saying?

MR. TRAPP: Yes, many times it is different. And it is, yes, a shortcut method to get to the safety question answer.

CHAIRMAN RYAN: Okay. John Stamatikos, you wanted to pick up, or Britt? Either one.

MR. HILL: Britt Hill, NRC staff. Yes, I first want to make sure the message is very clear that we do not take shortcuts in our safety assessments. We sometimes help improve computational efficiency, but in looking for public health and safety issues --

CHAIRMAN RYAN: I'm okay with shorthand.

MR. HILL: -- no shortcuts. But it does speak to the crux of our role, is to evaluate the risk significance of information, because risk is the metric that we use for determining public health and safety issues. If we have information that is out in the literature, we have to be able to evaluate the significance, the risk significance of that information, during our licensing reviews, and determine if there is any public health and safety issues with that information.

And that's where for some aspects, if you're trying to look at a performance assessment to come up with an average understanding, treating probability like a parameter, sampling between two end members of a range. If you can convince yourself that all the intermediate points in that range are realizable, then, sure, you can treat it like a parameter and come up with an average value.

But in part of our review, we have to consider outlier information -- information that may be developed after the license application is submitted. And how do we evaluate the risk significance of that information? Clearly, alternative information doesn't represent an average of anything. It represents an alternative conceptual model that exists on is own merits.

The first question we have to answer in our reviews is: does that alternative information have a significance to risk to public health and safety? If the answer is yes, then we have a much harder job. We've got to look at the technical merits of that alternative information and really understand how that represents a range, an average, uncertainties, all those important questions.

If, however, that alternative information does not significantly affect risk, then we have an understanding of it, we have evaluated it in the appropriate regulatory context, and we can address it as part of our review. But we don't have to go into a large level of detail on that topic.

We focus our reviews on the things that are most important to risk. We don't focus reviews on things that don't affect risk. So that's why we have sometimes this different approach in how we're going to treat probability.

MEMBER HINZE: But yet you don't know what the risk is until you've done a sufficient amount of work to perform -- to have the process models, the conceptual models and parameters, and all the rest to feed into the performance assessment.

MR. HILL: I think that's part of the issue. We aren't focused on determining what the risk is. We want to have an estimate of risk. We want to understand what drives the risk equations. But that absolute answer isn't really what we're going for. We're more concerned with what is driving that and what the alternative information may do to that number, so that we can do our job in reviewing the DOE's license application.

I think Tim McCartin has a -- no?

MR. McCARTIN: If you're finished. I just wanted to give one perspective on -- I know John Trapp mentioned the conservative approach. And with respect to our performance assessment model, we have not tried to take a conservative approach. I think what John was referring to is in our review, if an applicant takes a conservative approach, and they comply, we're done.

But I would like to make clear that in our performance assessment approach, in the TPA code, we have not tried to put conservatism in it. We have tried to do what John Garrick said, take your best shot.

CHAIRMAN RYAN: Tim, that's real helpful, because I think that's part of, you know, where some of the comment and discussion is -- your comments are very helpful to further explain that, the views, you know, that you're expressing now.

You know, on the one hand you did do a -- kind of a probabilistic assessment of dose to the receptor, and in other cases you've chosen to use a simpler approach for many of the reasons, in part or in combination with other reasons that you've touched on now, Britt, and that John Trapp referred to a minute ago. So it's helpful to hear what is behind that.

Of course, a question comes up, and I think this is what Bill was saying, is that if you do it in the kind of way Tim has done the TPA, you're sort of in the ballpark of a PRA, and you can explore any value over any range you want and see what happens to the endpoints and propagate it through a model.

If you pick a value, you create a burden for yourself, in that you have to really explain why that's a good one, or, you know -- and then, you have the risk that if you -- and maybe it's a small one. If you've thought through the problem correctly, is that you could mask something.

So, and I'm not saying you've done it. I'm just simply saying those are the kind of pitfalls and pluses and minuses. And, of course, by picking a value you get all the things that you've talked about -- simplicity, ease of review, clarity, and transparency, the calculations. There's lots of pluses there as well, so don't feel like I'm being critical.

But it's helpful to hear that and understand what's in play at what point in the different calculations and processes. So --

MR. HILL: I tried to give a very simple example. If you're trying to look at two very mutually exclusive alternative conceptual models, you have two end members, call it an orange and banana, and you want to sample between those two to know what you're going to have for lunch the next day, well, if you sample those two end members and come up on average you're going to have what? You're going to have something that's physically unrealizable.

But if you have enough fruit, you've got a whole basket of fruit, to where your choices aren't simple binaries, you've got this large amalgamation of choices, perhaps the best thing to do is throw it in the blender and take the average and get a sampling of everything. It's a simplistic analysis, but that division of where do you look at this as a binary problem versus something that could be an ensemble average is best illustrated by the two approaches we have right now.

The Department is taking ensemble average by convening the probability elicitation panel, looking at a range of alternative models, having their experts come up with a range of models, each of which incorporates different kinds of aleatory and epistemic uncertainty, and is producing an ensemble distribution that they will propagate through the performance assessment.

We have no fundamental issues with that approach. However, the Department is not the only potential party in this process. There are other parties that may need to be considered, and other issues that may need to be considered, at which point we may be looking at this as a simple binary type process.

Here is the license application with its approach, and now we have some alternative information. If we sample between that alternative information and the information in the license application, what are we really doing? We're creating physically unrealizable states between those two end members of information.

It is much cleaner for us to simply evaluate the significance of that alternative information, in isolation, by comparing it directly to what we would get in the license application. The only time we would need to start worrying about the statistical representation of the alternative information is if we found this would have risk or safety significance from that alternative.

CHAIRMAN RYAN: And the approach you've

just outlined to me really hinges on how well you've explained, or demonstrated I guess, that it is not risk significant alternative information or it is. So that's probably the focal point of that -- of having that approach. Is that a fair observation?

MR. HILL: Right. And the only way we can do that is in the context of the information that's tendered and available as part of a license application.

CHAIRMAN RYAN: We're talking in the abstract, and I know that's down the road, but that's really the key to it, isn't it? I think us understanding that, and everybody that's here understanding that difference, is helpful. I think it will be helpful if we can somehow capture that, Bill, as we talk about this in the white paper.

MR. HILL: If I could interrupt, Tim McCartin will be talking also about the treatment of alternative conceptual models and --

CHAIRMAN RYAN: Later on. I think, John Trapp, you had a comment?

MR. TRAPP: Yes, because Britt started on something which I think needs to be expanded just a bit. While DOE and this whole PVHA is putting together an ensemble, one of the things that Kevin knows and was presented in the first PVHA and will be presented in this PVHA is a breakdown of the various experts, so that you can take a look at the experts one by one by one and see how they got to their conclusions and what their conclusions would do if they were put totally in isolation.

CHAIRMAN RYAN: Well, that helps you in the "is this risk significant or not" assessment.

MR. TRAPP: Right, and we can find out what drove the various experts to put in their numbers and find out if there's something driving that needs to be look at in more detail.

CHAIRMAN RYAN: That's real helpful. And, again, I appreciate just this quick diversion, Bill, for a few minutes to hear these approaches. And if anybody for the rest of the day wants to add their two cents on this point as we go along, I think it would be real helpful to the Committee.

And, John, thank you for starting us off yesterday on getting the discussion going.

MEMBER HINZE: Dr. Weiner, you had comments?

CHAIRMAN RYAN: Thanks.

MEMBER WEINER: I had a comment, and the conversation may have gone beyond this a little bit, but I'm trying to put this into the context of the parameter that you are actually looking at, which is the probability of an igneous event.

Now, when you convene an expert panel, the expert panel comes up with a range and you can either look at the entire range or look at the median or look at some piece of that range. But you also have to remember that that expert panel consists of people, I'm sure who are all experts, but it's the people who happen to be available, the people who don't have too much else to do, the people who want to participate, as distinct from those who don't want to participate.

So you're getting a self-selected group, and they -- the reason you're getting this self-selected group is that you can't simply look at the frequency of events the way we can look at, say, traffic accidents. We have tons of data on traffic accidents, so you can look at frequencies and see -- and say this is the probability. But you can't do that in this instance.

So it seems to me that in picking a point in this range there is a tremendous amount of unexplored uncertainty, if you will, and that -- I think this is -- this is my question about the approach. And I understand that NRC is the regulator, NRC looks at what the applicant does, not -- it doesn't do it ab initio.

But it seems to -- my discomfort is that you have -- and perhaps this is too simplistic -- you have a point that you have picked, and you want to know whether that point is within the range of the probabilities that have been elicited by this expert panel. And, you know, what if it isn't?

MR. HILL: Britt Hill, NRC staff. We have not picked a point that represents the truth or the correct number. We have a number that was based on different considerations of alternative models, and also, as more coincidence than anything else, tends to be around the middle of the range of uncertainty that we think could be supported by various interpretations.

Now, we've communicated back in 1995 and '96 to the Department about the use of expert elicitation in licensing. I think it gets that letter from, I believe it was, Bell to Austin -- gets right to the point of the elicitation provides useful information, it's very valuable. We will give it full consideration in licensing, but it does not constitute the sole technical basis for looking at probability. There will be other information.

So one of the things here on this number, more than anything else, we have to keep in mind that this is a very simple linear number. If you believe the probability should be an order of magnitude lower than 10^{-7}, multiply by .1 for the risk. If it should be an order of magnitude higher, multiply by a factor of 10.

There is no position or regulatory intent with 10^{-7} except that it represents a number staff believes is credible, recognizing that there could be other credible numbers that are equally credible.

CHAIRMAN RYAN: And I think the real challenge is -- I agree with you on the probability, and it's a point you pick. You know, you assign it, and then it flows through just as you describe. But it gets a little tougher if you're dealing with parameters that are uncertain or they're non-linear, and they're non-linear in combination.

So, I mean, that gets to the performance assessment calculations that Tim McCartin talked a little bit about and will talk more about I guess later, and some of the other kinds of calculations. So, you know, I can appreciate that, again, from a license review point of view you can be in the PRA sort of approach that Tim McCartin has taken for dose assessment for long-term releases.

You can be at -- well, this is a value that is assigned and it's clearly a direct scalar and there's no challenge to that. So it's pretty easy to go up or down from there on a result. Or it's really complicated and combinations of factors can swing things in much wider and not clear ways. So that's the tough spot, that last group.

MR. HILL: Right. There is a fundamental difference between what we're doing in volcanism for probability versus what needs to be done in seismology.

CHAIRMAN RYAN: Fair enough.

MR. HILL: For volcanism, there is no relationship between the magnitude of the igneous event and the likelihood of occurrence, unlike in seismic where there is a strong relationship between the likelihood of occurrence and the magnitude of the event.

So that's where we've recognized for many years that a fundamentally different approach, a much simpler approach, is supportable, given the very narrow range of kind of igneous events that we're really looking at.

CHAIRMAN RYAN: And I think -- just one

last point. I'm sorry. You know, Dr. Weiner said, "Is there unexplored uncertainty?" And I think I would refine that a little bit and agree with you, is there unexplored uncertainty that has a potential significance to risk from your point of view in evaluating an LA? That's a little different question.

And by "unexplored," I don't mean -- I guess I view unexplored to be, do you understand how the system works? Have you explored it enough so you know how it is behaving? That kind of thing. As opposed to, did you use one tool over another? I mean, you can use a lot of tools to figure out how to explore it. That's not well said, but --

MR. HILL: If I understand correctly, we are very confident that the information to date, based on many years, decades worth of work --

CHAIRMAN RYAN: Sure.

MR. HILL: -- shows that the probability of the event is truly an independent parameter. There are no other link dependencies in the performance assessment code, except for the timing of the potential event, which would follow a simple exponential type distribution.

CHAIRMAN RYAN: And I'm just setting that one aside. I'm kind of thinking, you know, just in general where it's not so clear, where you have to do a little bit more work to get to the clarity.

MR. HILL: We are constantly thinking about this.

CHAIRMAN RYAN: Sure.

MR. HILL: We would rely also on having the Department explore those kind of relationships, if such relationships occurred. But to date there has been no information to show that there are unconsidered effects in the risk assessment from treating the probability as a simple, independent parameter.

CHAIRMAN RYAN: Again, I appreciate the dialogue. Thanks, Britt.

MEMBER HINZE: Right. Bill, do you want to --

MR. MELSON: I just have a brief comment. Bill Melson, Smithsonian. I kind of look at this from I hope a more impartial view than many people here. Something that has been a great concern to me is, one, the NRC research, as done by Britt Hill and others, is one thing that certainly supports the program in many ways.

However, when they become proponents of a particular point of view that may or may not be popular, and may or may not be correct, I feel they've got a conflict of interest in judging applicants' data. This has been a deep concern. It's one thing to get the perspective you need, but it's quite another when you're co-author on a paper that's controversial. And it is.

I mean, we can argue back and forth, as has been done in writing, you know, we've seen across the sea, letters come in defending positions. I find this not helpful to the project personally, and I don't understand it.

MR. HILL: Britt Hill, NRC staff. I feel compelled to respond to that. There is no conflict of interest in developing a technical basis to understand issues. The NRC has a long history of conducting independent investigations that help it do its job efficiently. Absent a technical basis that we have developed ourselves, we would then rely solely on the information presented by the applicant or information in the literature, which may or may not be relevant to the decisions that we have to make.

I'd like to go on the record firmly stating that just because we have developed an investigation and presented it in the review literature that we believe somehow that that information is our sole technical basis. We are open to alternative information. We give full consideration to alternative information, and in no way prejudice our reviews based solely on the work that we have published, discussed, or developed ourselves.

MEMBER HINZE: Thank you very much.

I'd like to return to you, Eric, and see whether you have any comments on these issues or whether you -- from your standpoint as a license preparer.

DR. SMISTAD: Yes, maybe just a couple of things. I know the NRC knows this. I don't know if the Committee knows it. We do sample a range. It's not a single point pick we make, just so you know that.

Just I guess a couple things from an applicant's standpoint. The words and phrases that John has talked about and Britt has talked about, very similar position we've got. We've got safety in mind. We've got, you know, a high quality application with a sound technical basis in mind, while adhering to the tenets of the regulation. So I think that's important to say.

MEMBER HINZE: Well, thank you all very

much.

Let's move on, then. And apparently there is no representative from Clark County, so we are prepared, then, to go to a representative from the Electric Power Research Institute.

CHAIRMAN RYAN: Bill, just for the record, is that due to the local weather here, or is that they didn't -- or do we know?

MEMBER HINZE: They had no comments from Engelbrecht, but Gene Smith -- I asked him to cover some of the items that he didn't completely cover yesterday in rebuttal, if you will, to some of the comments that were made. And he did not have the chance to do that. But, obviously, they have opted to not --

CHAIRMAN RYAN: If Gene Smith comes in later, we'll certainly give him a spot.

MEMBER HINZE: We will let him go then.

CHAIRMAN RYAN: Okay. All right. Fair enough.

MEMBER HINZE: Dr. Morrissey from Colorado School of Mines, we are pleased to have you here, and we are anxious to hear what you have to say.

DR. MORRISSEY: Thank you. I want to first say thanks to the ACNW for allowing EPRI to provide comments on the white paper.

Well, today I'm going to focus on the event probability and the nature and characteristics as part of the work that we've done.

Some of the contributors to EPRI's igneous events analysis are listed here. EPRI hasn't done a lot of their own calculations on probability, but we have analyzed the PVHA work in the past, and we have adopted a probability value of 1.6 times 10^{-8} per year as the expected frequency of volcanic intersection.

Some key points that we highlighted from the ACNW white paper along these lines are that the volume of basaltic volcanism in the Yucca Mountain area has declined over the last 10 million years, and it represents a very low active zone compared to other volcanic fields. Something you noted.

We agree with that -- that the probability range of 10^{-7} to 10^{-9} is consistent with all the published studies in the past, and it is consistent with the observed rates of the Pleistocene volcanic activity in the area, and the latest drilling results that have been published.

We also highlighted that some -- we agree with ACNW on their observations at the proceedings from the PVHA update proceedings, that we, too, will be anticipating the report coming out in 2008, and we'll be reviewing that.

Some observations that the ACNW noted at the proceedings, too, is that the panelists appear to be emphasizing giving a higher probability of -- that the Pleistocene events are more realistic as reasonable events to consider for what's represented for the future, and that the panelists also incorporated new information that has come out since 1996, and with emphasis on the lithostatic pressure variations.

And some additional comments that we -- that the EPRI has noted at the proceedings is the consideration of waning of basaltic volcanism in the Yucca Mountain region, as noted by Bruce Crowe, and he also noted that there is a repose period after the Quaternary volcanism, and also that dike evolution, because that's a topic of interest, big discussion at the proceedings, and magma genesis.

Frank Spera brought this up. It was a really fruitful discussion among the panelists, and they were very interested in the geochemistry, so I think that's something that ACNW should consider, too, in their white paper.

Now we're going to switch over to the

nature and characteristics of igneous activity, and we highlight a few points that describe what's anticipated in Yucca Mountain. And this is some highlights that ACNW made a point of, and that the igneous events are similar -- future or potential igneous events in Yucca Mountain are very similar to the nature of Pleistocene volcanism in the region with Lathrop Wells being the most probable candidate for an analogue, and that there is -- the volcanism is small volume, and it's typical of what has been occurring over the last million years. And that's commonly related to residual pockets of magma triggered by tectonic movement.

And we also highlighted the fact that ACNW noted that Valentine and Perry -- we just had a nice discussion about the fact that dike movement observed in the Yucca Mountain region is fairly vertical with limited lateral propagation. So it's showing that these dikes are coming up from depth fairly fast.

And another point that --

MEMBER HINZE: Change that to a Dove bar stick now.

DR. MORRISSEY: Right, exactly. Yes, it's a Dove --

MEMBER HINZE: From a --

DR. MORRISSEY: Yes, yes, yes. You can -- yes. Put that on the record, that it's a Dove bar stick.

And the fourth point we highlighted was that the potential Yucca Mountain magmas are wet, cool, explosive magmas, as opposed to hotter, more fluid magmas. And this is observed in Pleistocene lava flows at Crater Flats and Lathrop Wells.

We'd like to take the opportunity to summarize the history of EPRI's conceptual model. And these are -- this is contained in these three internal reports. I'm not sure if the ACNW has their 2006 paper, but we can make sure you get that.

What we started out in 2004 is we did a review of the geology in Yucca Mountain, and we -- in our report we describe in detail the physical volcanology that the Los Alamos group has been publishing for 20 years now. And we recognize that Lathrop Wells is the best analogue, and I believe that's considered by DOE and NRC, and that the Lathrop Wells represents -- you can -- represents the type of eruption activity that is anticipated, starting off with a fissure eruption, fire fountains, and aa flows.

One thing that we found interesting was that -- the fact that the lava flows there in Lathrop

Wells are aa. They don't see the pahoehoe thin hot flows that have been modeled over the years by DOE and NRC.

So something we started considering is: is this a typical type of lava flow in the region? And I have this table from the ACNW who did a similar thing. You notice that many of the -- less than a million year old lava flows, they are very limited in extent, and most of them are characterized by thick, rubbly flows.

Well, when we looked at this, we started seeing it's inconsistent with hot, 1200-degree, low viscosity magma. So in our 2004 paper we tried to resolve this by trying to understand cooling mechanisms for dikes coming up. So we looked at the Pollard and Delaney model and the Kerrigan model, and we -- ACNW also noted this, too, that there is nothing -- the character of the erupted lavas in the region of Yucca Mountain that would suggest any behavior of this nature, that they're highly mobile lavas.

And on the contrary, the lavas from the Cinder Cone scattered throughout the region are exceedingly limited to less than a kilometer in radius. So we agree with this. We had agreed with this. And when we saw that most of these lavas in the area were described as aa flows, we thought, well, there is no pahoehoe in the traditional understanding of basaltic lavas. They come out as pahoehoe, and as they pool and crystallize further away from the vent, they become aa flows.

Well, this didn't seem applicable to Yucca Mountain. So to resolve this initially, because we still were stuck with this 1200-degree eruption temperature for basaltic lavas, which is well -- in the past has been well accepted. So in 2004 that's the temperature we were dealing with, and we're trying to resolve. And so we were looking at these different cooling mechanisms.

So at the end of that year we came across the Nicholis and Rutherford paper, which they did the experimental petrology decompression experiments, and they estimated temperatures -- eruption temperatures for the Lathrop Wells Cinder Cone to be 975 to 1010 degrees C, which seemed to us more consistent with what you see at the lava flows that come out at Lathrop Wells and other Pleistocene volcanoes there.

So with that, we went on to consider what in our consequence scenario -- we wanted to understand, okay, we see aa lava flows at the surface. These are the type of flows we expect to go into the drift. This would be of a degassed magma coming in, intercepting the drift. So we looked through the literature to find viscosity as a function of temperature, and we came across Lore and others' viscosity versus temperature curve.

It also included the liquid -- the temperature versus -- the higher temperature versus viscosity as published by Morase and McBirney in '73, and Shaw and various other groups. So this is a continuation line. We were interested in these higher temperatures to obtain viscosities for our consequence scenario.

So this is why we picked this one, because at the time it was available we were considering degassed magma, basaltic magma coming up and intercepting the repository. So this is why we chose the Lore and others' diagram.

In our 2005 -- in our 2006, we have done a similar approach. We have looked at the Roscoe -- help me out here, Bruce.

MR. MARSH: Yes, Roscoe. That's it.

DR. MORRISSEY: Yes. Their viscosity as a function of crystalinity and temperature. So we have adopted that in our 2006 model, which you probably have not seen yet. So a key point here is that we agree with this approach that the ACNW has taken that to use the viscosity to get a better idea of what is the viscosity values of Yucca Mountain basalts, this is a better approach, and we will be adopting this in our 2006 and in our 2007 work.

We also want to highlight this two-dimensional model of the magma column that ACNW has come up with. It shows the viscosity of magma -- in the magma column as it's coming up and degassing and crystallizing. We did a similar approach, but we considered things in three dimensions in our 2006 paper, based on work that Valentine -- that Greg and others have done with the more updated, detailed analysis of the Lathrop Wells deposits.

Here in the early cone-building phase we have Strombolian activity, fire fountain activity, and we also have contemporaneous aa flow coming out. Later on, it becomes a sustained column. The cone-building is this finer material, and we also have aa lava flows coming out at the same time.

So, in our analysis, we're considering not just in the 2-D but also in the third dimension along the length of the flow. So we are -- we are -- we'd like ACNW to consider maybe a component analysis, too, of the material that comes out. If you look at the deposits -- and this is work from DOE in 2004, where they described the component analysis such as looking at the pyroclastic deposits, looking at the percentage of sideromaline crystals, tracolite -- tacolite.

And they also give you some understanding of the magma -- the type of magma coming out, its crystal content, its degassed content. And also, if you consider reaction rims in amphibole phenocrysts that Nicholis and Rutherford describe in their paper, they have also constrained that the magma at some point has resided at depth below the repository depth for several days. So it could be degassing.

So not only do you have variation in the column, but also in the lateral extent, too. So I guess our point here is that when you consider the assumptions of all drifts will be filled with a specific type of magma rheology, well, that is probably not going to happen. So if you're -- in more detailed analysis, you would consider if a dike did intersect a series of drifts, is it really going to be the same type of magma all the time?

Okay. So we're advocating that the intersection along the dike, it could be a thick, tacky magma, to a bubbly magma, or a fragmented magma that could go in. So something to consider.

Move on, back to the ACNW paper, is we also agree here that volcanic conduits would be significantly smaller. The diameter would be significantly smaller at 300 meters below the surface at the repository level, and that would minimize the number of waste packages.

We also mention this in one of our -- in our 2004 report, that we believe that the conduits and dikes that we -- that a minimum width or diameter would be two to four meters. And this is something I believe has been observed in the work by DOE recently, that conduits don't extend that far down to 300 depth, especially the small volume magmas that are coming up.

And that the models suggest that the dikes should be -- okay. Here is something, too, that -- when you look at this picture, and Kevin Coppersmith was discussing this the other day about extrusive event definition. If you consider a fissure that comes up and it intersects the repository, well, if conduits are developing from the top down, and if you're counting one or two or three vents as events, you're double-dipping so to speak. Because the dike has already intersected the drift, that would be a single event. And this is going back to some probability -- the probability analysis.

So it's something to consider, that maybe these three events are actually one event, because you already had a dike that went to the repository. And these conduits may not be reaching far enough down to even interact with repositories, so something to keep in mind.

Another area that we'd like ACNW to consider is adding criteria for analogues. Here we paraphrase many comments that ACNW had made about analogues, and we agree with it that the selection of analogues is an important part of all the analysis for characterizing features that we need to quantify in the probability analysis and the consequence analysis.

We gave a paper at the high-level nuclear waste meeting in May of 2006, and we could provide you that -- with that, too, right now. Since we don't have that much time today, I'm not going to go over the various analogues.

But consider the criteria here. This is -- DOE had promoted this, too, in one of their talks at the PVHA update proceedings, and we follow that. But we also think that -- feel that you should consider the magma composition, along with the water content and the crystal mineral assemblage, too, because if we look at traditional basalts -- many people feel that Hawaii is the analogue, but it is not. If you consider the mineral assemblage and the composition at Yucca Mountain, it's very different than what is in the -- than what we see in textbooks and all.

So this is some analogues that we described in our high-level nuclear waste paper or some analogues to consider that may be not appropriate for Yucca Mountain in areas that have been used to quantify in the parameterization of probability models and consequence models. And some of the -- these are a few that -- Kevin Coppersmith had a nice, long list of appropriate analogues that we -- using this criteria, they fit as good analogues.

So to summarize, broad agreement -- we are in broad agreement with the technical analysis and implications made by the ACNW, although we're waiting for the final conclusions like everyone else.

Some items for ACNW to consider with regard to the PVHA update -- the waning of basaltic volcanism in YMR, dike and conduit evolution, magma genesis. And in regards to the nature and characteristics of igneous events -- and these are relevant to the probability and the consequences -- are to add analogue criteria, the rheology, how it varies laterally and as well as vertically in the magma column.

And then, the depth of crystallization during potential eruptive events, and that is more looking at the component analysis. And also, the conduit diameter at depth, that it may not extend -- these very large, wide conduits, because they are such small volume magmas that are anticipated, that they may not develop into wide conduits at the depth of the repository. So --

MEMBER HINZE: Thank you very much, Meghan.

DR. MORRISSEY: And we, too, will provide a detailed writeup --

MEMBER HINZE: Okay.

DR. MORRISSEY: -- with lines and all, yes.

MEMBER HINZE: So they will be expanding upon these or will there be additional --

DR. MORRISSEY: Yes, we'll -- yes, yes.

MEMBER HINZE: Well, that would be great. If we could have those, that would be very useful to us.

DR. MORRISSEY: By March 1st.

MEMBER HINZE: Committee, questions?

Ruth?

MEMBER WEINER: I hope this is a fair question, Meghan. But you were hear yesterday, and you heard Chuck Connor say that there was some problem with event -- with definition of an event. I wonder if you could comment on that. Did EPRI have any problem with definition of an event or --

DR. MORRISSEY: Well, I agree with Chuck that it definitely has to be well defined in terms -- for each calculation. So if it's going to be consistent, the PVHA-U, the update panelists, have to have a consistent definition, yes.

MEMBER WEINER: Thank you.

DR. MORRISSEY: So you, too, should make -- include that in the report.

MEMBER HINZE: But that may vary from one group to the other. Is that correct?

DR. MORRISSEY: Pardon?

MEMBER HINZE: But that may vary from one group to the other?

DR. MORRISSEY: I would --

MEMBER HINZE: In other words, DOE may have its own event definition?

DR. MORRISSEY: Right.

MEMBER HINZE: The NRC would have a

different -- may have a different event --

DR. MORRISSEY: Right.

MEMBER HINZE: -- definition?

DR. MORRISSEY: Right.

MEMBER HINZE: Or are you saying --

DR. MORRISSEY: I would -- as done in the PVH, in the first calculation, I believe they had -- they all defined and they were consistent with their definition. So I believe you should have the same event definition.

MEMBER HINZE: Further questions?

MR. MARSH: Meghan, in earlier EPRI reports you had -- you showed calculations, really, with steam blasts and other -- the pyroclastic phase and there are fairly heavy-duty numerical results they look like. Are you doing any more of that? Is there any more of this coming or --

DR. MORRISSEY: Yes, we updated -- because we noticed that we used some lower temperatures and some of the values that we chose were on the low end, so we redid those calculations. They're in our 2000 -- no, they were in a draft of a Journal article that we can send to you, too. There were updated versions.

MR. MARSH: So you mean low end -- what do you mean by "low end"?

DR. MORRISSEY: Well, not -- the temperatures were not the temperatures that we were using in our 2007, okay, the 1010.

MR. MARSH: Do these calculations --

DR. MORRISSEY: I think the numbers went down to 900, but that doesn't make a remarkable difference.

MR. MARSH: Okay. Does this treat the process -- the transition from water saturated, non-visculated magma to the --

DR. MORRISSEY: Well, those we were doing our own analysis and looking at the type of work that Andy Woods and others did, and looking at things in two and three dimensions, seeing how the -- if you brought the crack in in vertical dimension, and what are the dynamics in terms of a vertical crack intersecting a horizontal drift.

And that's what we were -- that was part of our study, and we found that you get the high pressure concentration at the top of the drift. And then, if you -- if you extrapolate to the work by DOE, by Ed Gaffney and others, who showed, too, that you can get -- open up a crack and you can start sending fluid up the crack, it eliminates some of the -- alleviates some of the pressure buildup.

So it was -- our approach was to find -- to look at various factors that influenced the pressure history in the drift when magma interacts with the repository. And I can send you that paper as well if you're interested.

MEMBER HINZE: Further questions or comments? Dr. Clarke?

MEMBER CLARKE: Just a clarification question, Meghan. On your second-to-the-last slide, you have analogues. And you have a group that is called uncertain analogues. I'm just wondering what you mean by "uncertain." They may not be?

DR. MORRISSEY: Well, yes --

MEMBER CLARKE: They definitely are not, or --

DR. MORRISSEY: -- according to the criteria, there is some aspect of those that do not fit the criteria. So we would say --

MEMBER CLARKE: It means some of the criteria, but not all, is that --

DR. MORRISSEY: Right. Exactly, right. So, for instance, Grant Ridge I believe is in there. It's volume is three or four times that of expected volume for an expected eruption at Yucca Mountain.

MEMBER CLARKE: Okay. I understand.

Thank you.

DR. MORRISSEY: Okay.

MEMBER HINZE: Dr. Sparks?

DR. SPARKS: Yes. Make a note. I just wondered -- I'd like to just discuss one issue, which is -- I developed in my talk, which is the rheology. And I think the one point is that the Nicholis and Rutherford temperature estimate is based on the equilibria of -- the stability of hornblende down at high pressure.

DR. MORRISSEY: Right, right.

DR. SPARKS: And so that is not a measure, I don't think, of eruption temperature, because of latent heat effects. And the consequence of latent heat effects is that the magma, when it crystallizes as a consequence of degassing, can erupt as a significantly high temperature, and, therefore, lower viscosity.

So I would suggest that if one was looking for analogues, which is I guess the point of my talk, you'd look at other trachybasalt volcanoes, like Eldfell and Etna, where you can measure the temperatures and you can see that they are somewhat higher.

DR. MORRISSEY: Right.

DR. SPARKS: And you can also, at least in the case of Etna, you can measure the rheology, and you find 10^5 Pascal-seconds would be an upper limit.

So I would certainly -- one comment on the white paper in that context is you showed a diagram from the white paper showing viscosities of 10^9 Pascal-seconds in the conduit. I would suggest that those are not credible viscosities for these sorts of eruptions.

DR. MORRISSEY: Right. As I said that, we're -- the science of basalts is still evolving. We've learned a lot over the last 20 years. Okay? So it is a higher -- we -- EPRI feels it's a higher viscosity than previously thought. Okay? So 10^5 Pascal-seconds we believe is a reasonable value. Okay? Much -- more appropriate than what is said in the past by DOE and NRC as 10 to 10^1 to 10^2.

DR. SPARKS: I would certainly agree.

DR. MORRISSEY: So it's more consistent with what you see in the field, which is what we've always been basing our ideas on is what you see in the field, is that consistent with how we're going to model it. Okay? So --

DR. SPARKS: Okay. Now, I agree with that.

DR. MORRISSEY: -- yes, the work you've done is -- I appreciate the work you've done, and the same with Bruce Marsh, all the work on crystals and viscosity. It's the right approach, the right direction.

DR. SPARKS: I'd just sort of make one further comment that you might want to consider in developing the models for deeper down, that in the very fast flows there might not be time for the crystallization to take place, and you see --

DR. MORRISSEY: But they are degassing, because what you -- at Lathrop Wells, this is something we -- it's a good discussion. You don't see the pahoehoe, the degassing, the basalts that come out with, you know, some residual water coming as such. Okay? You don't see that.

So there is some component of the magma that is coming out, that is reaching the surface, that is degassed. Okay? That is starting to crystallize. That's higher viscosity than what we --

DR. SPARKS: Yes. I agree with that. The point I was making was that you see evidence, I think, in scoria and things like spatter --

DR. MORRISSEY: Yes.

DR. SPARKS: -- of when you've got the

sort of intent --

DR. MORRISSEY: Oh, yes. Yes, yes, yes.

DR. SPARKS: I'm not talking about --

DR. MORRISSEY: Oh, yes. Absolutely. Yes, yes.

DR. SPARKS: I'm not talking about the degassed magma coming up.

DR. MORRISSEY: Right.

DR. SPARKS: I'm talking about the fast gassy flows --

DR. MORRISSEY: Correct.

DR. SPARKS: -- that they can flow so fast that --

DR. MORRISSEY: Oh, yes. Yes. We're not discounting any effect of the pyroclastic aspect of, yes, you would see -- how that would come back into the magma column.

DR. SPARKS: That's what I'm saying is the 10^5 Pascal-seconds would be a good upper limit for the sorts of viscosities you might get in the sort of --

DR. MORRISSEY: Well, we're looking --

DR. SPARKS: -- system.

DR. MORRISSEY: Our approach was really looking at the magma that would -- that comes out as lava. Okay. So that's where we were really emphasizing our work in the past with the higher viscosities. Okay?

MEMBER HINZE: This is a good discussion, and thank you, Steve and Meghan. I think we're going to hear more about this and have a chance to discuss it in even greater -- and let's plan on doing that.

DR. MORRISSEY: Okay. Very good.

MEMBER HINZE: Are there any other --

DR. WOODS: Can I make one point?

MEMBER HINZE: Please. Andy? You'll need to get to a microphone. Right there, or we can use this one.

DR. WOODS: Yes. Andy Woods from Cambridge University. Just on this issue of the viscosity, if, as in the -- I guess the white paper report or in your presentation, you have these very high viscosities and very wet magmas.

DR. MORRISSEY: No. I was trying to say that they're not very -- the part of the eruption that we were considering with our higher viscosities was the output of lava, so that magma -- I'm not generalizing anything. I'm saying one component of magma that's coming out that we're most interested in is, what's the viscosity of the degassed magma that comes out as the aa lavas? And that's why we --

DR. WOODS: Well, if I can just continue --

DR. MORRISSEY: Okay.

DR. WOODS: -- with my question. I guess my point of concern is, if originally these magmas at depths have higher water contents, and they have these high viscosities, is there an issue about how the -- following the degassing was -- the decompression in the solution, as magma ascends to the surface, how those volatiles actually separate to produce a continuous high viscosity liquid phase, rather than what you might expect, which would be a much more fragmented dispersion of fine liquid-solid fragments.

DR. MORRISSEY: Again --

DR. WOODS: And I guess I'm just -- I'm curious how you can have a continuous phase of degassed magma, if it's such high viscosity, because the separation speed of the basalt gas --

DR. MORRISSEY: I agree with you. We're not talking about that. We're not talking about that high viscosity at depth. We're talking about viscosity of degassed magma coming up at shallow depth to become aa lavas right at the vent that yo usee at Lathrop Wells. So we're trying to extract that 300 meters down.

When does that degassing occur? Okay. So we're just saying that we don't feel that we're going to have these hot, low, high temperature magmas coming up and reaching the repository at 300 meters and flooding it. We believe that, yes, they are coming up as hot, low viscosity magma from depth. That's the only way you can get it up.

But at a certain depth below the repository, you have degassing exsolution. You have a lot of things going on that we truly don't understand yet. But because what you see at the surface is these very thick lavas that are coming out as aa lavas that do not flow very far, where does that occur?

We're just assuming that transition is occurring at some point, at one point, below the repository, so when these magmas are coming up in that degassed state, and they fill it -- they reach the repository, the type of degassed magma we expect is more of the aa type than a pahoehoe type. That's all we're saying.

Yes, we envision, too, that we're going to get these less fluid, hot, pyroclastic type rheologies going in there, too, but there are not these fluid flows going through. That's --

MEMBER HINZE: Thanks very much, Meghan.

Any other points that need to be made? Bill?

MR. MELSON: Yes, I'd like to address a comment to Steve on latent heat and crystallization. You have to have crystals to have that effect be meaningful, correct? These rocks are nearly apheric when they come out.

DR. SPARKS: My understanding from the descriptions is that they are -- there is a large number of microphenocrysts, and from I think Frank Perry's and Greg's work, so there's a lot of crystals in the lava flow.

And also, my understanding is that the -- when you look at the scoria -- this is information that I was discussing with Britt that -- which is fairly typical, the scoria is, then there is less crystals in the scoria. But there's still quite a lot of crystals.

So if you take the thermodynamic equilibrium, as the one end member, you should have a lot of crystals, and, of course, everything from complete to sequilibrium, no crystallization at all right through to equilibrium. So my understanding is there's quite a lot of crystals in the --

MR. MELSON: Well, there's also quite a lot of glass, and I don't know that we know the answer statistically to how important the latent heat is, unless Frank can tell us.

DR. SPARKS: Right. But you can't have it both ways. You can't have it coming -- I mean, I agree with Meghan actually that the extrusion viscosities at the surface are quite high. I'd put them at around 10^5 Pascal-seconds at the lower end of the suggestions that EPRI made.

But if you fail to crystalize, and you erupt a super-cooled melt and produce a glass, if you take that as the end member, you're going to have a very low viscosity melt, relative to the crystal-rich one anyway. So those -- what seems to be observed is that these things do crystalize quite a lot as they ascend.

MR. MELSON: Well, liquid becomes a glass. It's a solid, not --

DR. SPARKS: Yes, that's true.

MEMBER HINZE: Well, let's hear what Frank has to say about what is actually in the rocks, which it might be good to hear. And then, I'm going to call a halt to this discussion, and then we'll pick this up again. I think we have some other talks on this same subject matter, and then that will be going into it in more detail.

Frank, what do the rocks look like?

(Laughter.)

MR. PERRY: Frank Perry from Los Alamos. We've done point-counting and looked at these lavas carefully. The lavas are rich in microlytes. I would say a few tens of percent, maybe 30, 40, 50 percent. And some even have like a trachytic texture with the microlytes. These microlytes are typically a few tens of microns to 100 microns.

The phenocrysts, which are very sparse, two to three percent, go anywhere from 500 microns or half a millimeter up to one millimeter or two millimeters. The scoria has less microlytes. I don't have a good number, but it's maybe up to 10 percent microlytes in the glassy scoria, but several tens of a percent in the lavas themselves.

MEMBER HINZE: Thanks very much.

And thank you, Meghan and E-P-R-I, or EPRI. May I ask, is Engelbrecht in the audience? Yes? Engelbrecht, we passed over Clark County earlier this morning, because there wasn't any -- wasn't a representative.

This is your chance to have any say, if

you wish to, regarding the nature and probability. And if Gene Smith is here --

MR. von TIESENHAUSEN: I expect Gene Smith to show up later. He might have some comments. I'm not a volcanologist, and I will keep my mouth shut on this subject.

MEMBER HINZE: All right. Well, okay. Let the record be noted that you were given the opportunity. Okay? Thank you.

CHAIRMAN RYAN: And I think, Bill, if I read the agenda right there is a slot later in the day.

MEMBER HINZE: That's correct, yes.

CHAIRMAN RYAN: Can we come to order there, over there on the left? There's another slot for Clark County if --

MEMBER HINZE: Yes, right.

CHAIRMAN RYAN: -- Gene does show up. And, again, recognizing we have weather problems and travel questions, and hopefully he'll be here with us. But thanks, Engelbrecht, and thank you, Bill.

MEMBER HINZE: We will -- we'll take a 15-minute break to 10:15. At that point, we'll pick up with a discussion by Bruce Marsh, followed by one by Art Montana, if I understand correctly. And then, we'll have a discussion.

(Whereupon, the proceedings in the foregoing matter went off the record at 10:03 a.m. and went back on the record at 10:20 a.m.)

MEMBER HINZE: Our next speaker is Dr. Bruce Marsh from Johns Hopkins University talking on, really, a follow-up to some of Meghan Morrissey's discussions.

Before Bruce begins, I would like to make a comment regarding Meghan's presentation, if I might. And that is that there were some views expressed that some hypotheses suggested that these were ACNW views.

ACNW feels that these views are viable views that need to be considered, but these are not ACNW views as such. And I just want to make certain that we're all on the same page, and I will take responsibility if there is any misreading of the white paper in that regard. And I will assure you that that will be rectified in the final version.

It's kind of a minor point, but to us in ACNW, I consider it to be a very major point.

CHAIRMAN RYAN: Bill, thanks for that clarification. I might just add and remind everybody we're exploring the range of views that folks have and have expressed in our role to give the Commission a summary of the range of views.

MEMBER HINZE: That's correct. Right.

CHAIRMAN RYAN: Now, we may have specific comment or observation, but we're not trying to determine what the right view is.

MEMBER HINZE: Right.

CHAIRMAN RYAN: We're exploring the range of views. So thanks for that clarification.

MEMBER HINZE: That's correct. Right. Bruce's handouts will not be available until early in the afternoon, but they will be available at that time.

With that, Bruce, it's yours.

THE MAGMA/REPOSITORY/CANISTER PROCESSES IN BOTH ERUPTIVE AND INTRUSIVE SCENARIOS AND IMPLICATION FOR RISK FROM IGNEOUS ACTIVITY AT THE PROPOSED YUCCA MOUNTAIN REPOSITORY

DR. MARSH: Good morning, everyone. I want to talk and explore some of the boundaries on these processes we have. We have been talking a lot about wet and dry magmas and magmatic processes in general.

We have seen things from Gene Smith talking about it deep in the mantle to talking about tomography and maybe talking about where magma is generated. We have been talking about a lot of near surface processes, too. I want to try to integrate some of this together to give a little bit of a perspective on maybe the overall systems in general.

This is actually a sill. People talk about sills a little bit in Antarctica, polar plateau and things like this. And we will be seeing more about these. This is a region in the world where you can see everything so abundantly clear, almost embarrassingly so, that you can actually look a lot at magmatic processes in great detail, a lot of detailed things. And we'll talk about this in a minute.

When people look at magmatic processes, they come at these from lots of different reasons. Some people want to understand the origin of the planet itself and how planets actually accrete and go into subdivisions. Other people want to talk about specific regions of arc and things. Other people want to know why we have ocean ridges of sea floor, some composition and continents and other composition. And some people want to know exactly the day-to-day long-term or short-term variations in volcano chemistry and look at processes that are happening.

You have very simple conceptual models,

quite probably untenable, where we have a magma coming from some depth, some source region coming up and held in a bubble we call a magma chamber. And then it erupts at will back and forth. This is nice conceptually but very unrealistic in terms of what we think for systems in general. They're much more messy, much more integrated than that.

If we look at a system like Hawaii in some detail a little bit, you see people like Mike Ryan, who puts together a model of what this may look like at depth in terms of the absence of epicenters or earthquakes that plexiglass models and other people have done some internal basically acoustic or harmonic termor tracing and things like this in these systems and other people looked at the surface in terms of inflation, elastic inflation, various models, and people put a sphere here or a lens or something. So these systems have many different looks from different ways you look at them.

CHAIRMAN RYAN: Bruce, just to clarify, that's not this Mike Ryan. That's another Mike Ryan.

DR. MARSH: That's right. This was very confusing for me when I came to this Committee, by the way.

(Laughter.)

DR. MARSH: I looked on the list of people. I told, "God, we've got a magma guy at the chair." Anyway, this is a magma guy. This is Mike Ryan. We'll call him Magma Ryan from the USGS.

(Laughter.)

DR. MARSH: But one of the things in these systems is that the magma comes up. And, actually, there's a lot of lateral transport in a system like Hawaii, Iceland, and many other areas, too.

There's lots of lateral transport and also at the ocean ridges, very similar. Things come up. Things go laterally back and forth. And we're tending to think of everything coming straight up in dikes and fissures coming up. And that actually happens, by and large, but near surface and things, there could be lots of lateral transport also.

For example, in Iceland, this is Gunnarson, *et al*., -- I am one of the *et al*. -- where we have in the Torfajokull, dikes form, fissures form coming down from center. We run down. And what doesn't read, this lateral transport reprocesses the crust, actually, remelts a lot of the crust.

As you recall, there is a lot with rhyolites coming out collections of debris in the crust and remelting, wholesale remelting, of the crust in some areas from vigorous transparent, areas like Haimaey that Steve Sparks talked about later. Since we had very little seismic episode before that, it's hard to tell exactly where the magma definitely came from, but it's on this trajectory also of this kind of transport.

So if we look at some of these systems put together, we can look at systems in many ways. Hawaiian people would make a lot of calcium versus magnesium, for example. And they get their very primitive stuff down here, high magnesium materials. They would look at the system like this chemically.

We actually look at integrated what the system may look like. It may be somewhat like this, not necessarily just a pod coming up with a chamber here and erupting. And we have material that comes out on the lateral flanks in the southeast rift zone, et cetera.

The system when it erupts, actually, and depending on what the longevity is, -- for example, Steve was mentioning Etna and things like this -- the age of the system, the size of the system has a lot to do with, really, what this will look like.

A monogenetic system is not going to have time to redevelop a long magmatic mush column like this. This may be 75 or 80 kilometers down here. In a very mature system, large volume system that's worked for a long period of time like they've got in Hawaii, a million years old, may have a very integrated system like this. And we know from looking in the Antarctic world, for example, and also looking at deeper seismic work, that this kind of structure really persists for a lot of ways in this world.

If you look at Hawaii, for example, -- and here's the Kilauea Iki Lake in the Hale Mau Mau area. And here's the 1959 Kilauea Iki eruption here. This eruption had big fire fountainings. And we talk about water contents, et cetera.

This was driven. These were 1,500 feet higher, more fire fountains driven, probably by about .3 weight percent water, for example. These are very dry materials. And they have fire fountaining.

And the lavas actually went in here and filled this lake up to over 100 meters deep. Actually, one of the hardest problems working at magma is that you don't get to actually play with it. You don't get to actually see.

We talk a lot about magma chambers and things, but we have really never found one that we actually can get into. And these are the closest representations we have, 100 to 125 meters. There's been all the way down to, Alai Lava Lake, 14 meters deep. And they get a crust on top.

And Tom Wright and Herb Shaw and other people had the wherewithal of actually drilling holes in this thing and doing experiments in them, doing the rheology. This is really some of the most unique experiments done. Here is Tom Wright in an earlier episode of his life and drilling down.

One of the interesting things about this drilling if any of you have ever been there is you drill down. The thing is drilling along. You hit, actually, the solidus. That means where the magma actually is molten. You keep drilling. It drills just like a solid, keeps drilling and drilling and drilling.

Finally, you can hear a sound change. You're bringing up core. And you're at about 50 percent crystals, 50 percent melt. It drills like a rock until it gets out to 50-55 percent crystals.

From that point, you can actually take the drill stem and push it in by hand in this. And you can push it. Really, it feels mushy. You can start pushing it down. And almost it's like, you know, putting a syringe through something. You can actually then feel. When it gets through this crystal region, you can push it into the deeper part of the system in there.

So this is rather firsthand. And, of course, the very important point is that, as Bill brought up, these are somewhat anecdotal in some ways, but we all come, as Kevin said yesterday, from our personal pet perspectives of what impresses us geologically, geophysically.

And this is one that does me because I also try to do things quantitatively on this. This is a hole, actually. It's annex holes, two inches across. And you can see the beginning down there of where that's only 600 degrees and the crust here was about 12 meters thick when this was taken.

So if you actually look at this, then, look at the crystal entity versus temperature in a system like this -- and this is Makaopuhi Lava Lake. We just looked at Kilauea Iki. But Makaopuhi is very similar, 85 meters, 83 meters deep.

We have a liquidus out here where there are no crystals. This is very important to keep in mind, no crystals. And we cool down here to the solidus, where it's 100 percent crystals.

So if the rock cools progressively at

equilibrium slowly and go down here, you form a gabbro essentially or a dolerite or a dye base down here, 100 percent crystals. And it crosses in the middle of this region of about 50 percent crystals, where it is basically a rock back here.

This is a solid material. Actually, it's a dilatent material. The crystals are so tightly packed that if you shear it, it actually expands because the packing of materials, especially of these kind, an ensemble of this kind of silicate materials, crystals, produces a dilatency.

And this dilatency, actually, when you shear it, it expands. It's like when you're walking along the beach, you step on the beach sand. And you'll see around your foot it's dry looking. It's because the sand is at maximum packing to begin with. You step on it. You shear it. There's not enough water to fill in around it. So it's dry around it. And this is what happens.

So this material is at that 55 percent crystals, but it's a dilatent material. When you shear it, it wants to expand. It wants to get larger. And it chokes up things.

Lot of volcanoes in the world, actually, the bad actors, are right at this point. Very many domes in the world, Merapi, et cetera, are right at this point of 50-55 percent crystals, a very sluggish material. They are very hard to deal with.

In this region, of course, things are more fluid out in here. And so we can start out here with a viscosity maybe of 100, maybe 75 to 100 poise. That would be ten pascale at the seconds. And we move down here. And the viscosity, of course, we'll see in a minute gets enormous going through here.

This is what it actually looks like if you actually take those samples out of the drill core. Now, you'll also see in here that we have some transported crystals. These are large olivines and things that are picked up in the flows.

Now, you're going to think about magma coming up through the Earth. Magma is coming up through the lithosphere. It's like coming through a gravel pile, especially alkaline basalts we look at. They're full of all kinds of junk.

If you look at these crystals, for example, these are way out of equilibrium. These are of 92. These are mantle wall rock crystals of olivine that have fallen into the system and a very common find.

After a couple of months, however, they

diffuse and exchange. And they're down to of 78, forced right 78 percent magnesium. So, in other words, they have come to equilibrium with this stuff.

This is a glass. This brown stuff is glass. These are small, little microlith patches, clusters of crystals growing together. And out of this comes bigger crystals. Actually, it's a very complicated business. We can get into it in detail if you'd like.

And the glass is building up of titanium. That's why I'd like that. It gets that reddish color. And then suddenly the titanium oxides appear and take all that out. And you end up with a thing like this, a rock in the end. These are also entrained, big crystals at the end.

So this is the sequence. And somewhere in here, of course, we're talking about this thing locking up and becoming a rock from a magma. There's part of it out here that's really a magmatic area. Part of it, this stuff back here, repartitions itself strongly.

If we look at just general magma rheology, it's no different than any kind of rheology of a suspension of solids and liquids. And it's a very, very difficult system to deal with for engineering, for example, because there are many, many -- pulp and paper industry, people who work with all kinds of emulsions and everything. They have to worry about materials of all kinds of sizes pumping in through pipes, et cetera. And there's actually probably 50 or 60 different rheological models for material for effective crystal into your particle content upon rheology.

This is relative viscosity over when you start with. For the magma, we put this as the liquidus. And there are various things. So this is what you determine for your maximum packing. I have it here at .6. It could be .5. It depends on your ensemble of solids, your combination of spheres, lathes, needles, nails, whatever you have in these kinds of things.

And there are all kinds of other things we could get into. The one that Meghan was talking about is the Roscoe, the one which I've talked about for years and the one which I kind of favor. It's very simple, straightforward, and actually seems to in many ways represent well what we see in many magmatic situations.

So, remember, we get the higher crystallinity here. The system actually goes into this other world of locking. So we call these things solidification fronts. And all around the body, everywhere around the body, where a cooling surface is occurring, we have solidification fronts. So it's fully solid on one side. And out in the middle here, it's very, very fluid material. And it's a major transition.

Now, the thickness of this zone, this package, of course, tells you something about the age of it. If a material is injected instantaneously at a constant temperature, we would have a very, very small liquidus/solidus separation, very tiny solidification front, maybe centimeters wide. And, of course, within minutes, hours, days, et cetera, this thing thickens. And so the whole thing propagates inward. And they thicken and propagate inward.

Now, deep in the Earth, these things could get enormously large, where we have long cooling times, lots of magma laying around time. When we're up near the Earth's surface, these things are very rapid. And you get very rapid chilling. And so you don't get big, thick solidification fronts.

And you can see in the lava lake in Hawaii, for example, that that took on the order of still probably not solidified in the middle. And that was 1959. So these fronts are meeting, coming down from the top and bottom. But this is what we have.

Part of the difficulty in dealing with the uncertainties, what we're all talking about here is this messy world, where we have not only these things. And we can't really treat them as inert materials, but they're growing materials.

In other words, they're sticky. They're sticky solids. They actually touch each other. And they kind of weld because they've got little chemical boundary layers growing on them. And it's a major challenge for us. We just can't go into the literature and grab things and put up.

So here is melt viscosity going up the side. You can see how large it gets up. This is the melt viscosity back interstitially in the crystals and things.

In terms of a deep magma transfer, you hear basic people talking, "Now, the magma coming up." What about the magma coming up at depth? One of the problems you have had is this kind of conundrum over the years. Delaney and Pollard and other people have talked about it, that magma moving, entering from a pipe, for example, entering a slot, whatever. We have something called a thermal entry effect.

In other words, if magma enters from some reservoir here, let's say Gene's reservoir in the mantle and coming up into the crust or into the upper lithosphere, if the region around it is cold, immediately the fluid starts cooling. And so you get these solidification fronts on the sides or you plate material out on the sides, you keep losing material.

One of the problem for a perfectly laminar flow is that this kind of term, V.gradT, in the energy equation, these are two vectors dot product. One of them is this way. One of them is this way. The dot product is zero.

You can flow this thing pretty much as fast as you want. It really doesn't do anything to the solidification front. It just marches in on here. So this has always been a problem.

Delaney and Pollard said, "This can't go really much more than two kilometers or so before they close in depending on how wide this is." Of course, you can widen it, make it a kilometer wide. And you can go anywhere with it. But if you have a dike-sized thing, you know, 10 meters, 50 meters, 100 meters, you've got to worry about this in terms of length.

I think there's a way around these kinds of things. And various people have tried to get around these things as saying, "Oh, there are real irregularities in here. And it might cause a little mixing and things like that."

Probably. I mean, these are messy systems. I want to influence people that way. But there is probably also something more general about this sort of thing. So if this were true, we would have magma quenching before we actually got much of the surface. And maybe it does happen that way in a lot of places.

Well, one of the interesting things about if we look at the pressure temperature diagram here, this pressure over here, typical for a dry system temperature pressure, solidus/liquidus. And I have on here also marked approximately where we would run into this 50 percent crystal entity region here. We start out with a magma up here somewhere at higher pressures. It could be even at 20 or 30 kilobars or more.

Here's the adiabatic ascent path. In other words, we took that liquid right there. And if we just think of the following situation, we took a model like Gene generated yesterday. He generated a liquid at high temperatures. And we just extracted that out. And that's by definition adis liquidus. We bring it up to the Earth's surface. We start moving it upward however we want to bring it, but let's think about a crack propagation problem.

And here is the adiabatic ascent path. In other words, the liquid wants to actually go out in this region. It wants to superheat like this.

Now, one of the things there is, we never ever observe a superheated magma on the Earth's surface from any endogenetic. What do we mean by "endogenetic"? We mean any magma that is deeply generated, generated deeply inside the Earth.

We do see superheated systems, like at Sudbury, for example, meteorite impact systems. That system was at about 1,700 degrees. So that was highly superheated at one time. But generally we never ever -- well, generally. Absolutely we never see any superheat.

This is always kind of a mystery. And I have thought about this for a long time. And one of the ways you get rid of superheat is through thermal convection. And one of the things that is very interesting about thermal convection, this is a paraffin system, experiments done over the years by me and other people in my lab, Genevieve Brandeis and Matthias Hort and lots of other people, is that this has a liquidus/solidus on it, too.

This is a superheated system. This is about ten minutes into the run. This is the rating number. And what runs it, this really vigorous convection, is it's superheated. As the thing cools down, it loses superheat. It loses its convection. In fact, as soon as it gets to the liquidus, the superheat is gone. Convection ceases in this thing.

In every system we looked at -- we've looked at now five or six different systems. They're all like this. We don't know. This is for magma. This has a little liquidus and solidus in here, too, on here, but this is the difficulty of working with magma, is that we can't just put it on the bench and do an experiment like this.

I've actually been toying with International Nickel to have them build me a slag pond so we can do some of these experiments because we can do it in Canada without a lot of OSHA problems. But we'll see.

So what happens? I think what happens is it is superheated. It convects back and forth every time it tries to superheat. And this thing oscillates down. In fact, now I can show analytical results for this. It oscillates back and forth on the liquidus.

It comes out right basically at the liquidus, which is very interesting in a dry system.

Then it ends up very hot, like Hawaiian systems. It may have some junk in it, picked up on the way, which is very interesting also. The junk is only olivine. It never has any CPX, OPX, or Spinel. They're gone. They're not only dissolved but probably fused from the high temperature coming up.

These are also very mobile. They're not explosive, of course, because Hawaiian stuff in general doesn't have a lot of volatiles in it, some but not a lot. And so it's very, very mobile. And they can go a long ways. Of course, these lava flows flow down.

Now, when we look at a wet system, a wet system is very interesting. We add some water to the system. What we do is we suppress. Where the system is saturated, we suppress the liquidus and solidus so it moves back like this, the geometry.

Once it gets under-saturated; for example, if we put in, let's say, two percent water, and up to this point it's saturated, at this point it's under-saturated, it could hold a lot more up here, it goes back to, really, the same initial liquidus and solidus system, but it's actually pushed down to much lower temperatures.

The other important thing is magmas when they come to the Earth's surface, of course, the solubility of water is zero. There is no -- it goes down to nothing.

There is no solubility at the Earth's surface. And so the liquidus-solidus interval that both of these have to occupy is the same. So, in other words, if we put the other one on here, it would be right here. So they would be the same. So this has this very important aspect of it.

Now, one of the problems, of course, of getting magmas up, you think, "Well, the same thing could happen for this alkali basalt." And alkali basalts are notorious for being full of lots of nodules and all kinds of things, inclusions, and just in general. In fact, many of us call them NABs, nodule-bearing alkali basalts, because they almost always have pieces of mantle material, wall rock, all kinds of junks in them.

And it may be the fact that these are dikes. These are monogenetic, small. They come up rather rapidly. But they also have this feature. And that naturally may cause a lot of havoc, even acoustic problems coming up and dumping a lot of stuff in.

And one of the problems is how do you entrain these things, these nodules? Well, this may be a way we partially can entrain them. And they also have sometimes large crystals, like hailstones, in them, anorthoclase crystals. And it may be due to these are produced in that kind of process, too.

And so what I'm talking about is integrate looking at all the back and forth and seeing if we have information for this. One of the things that is interesting now, when this thing gets near the surface here -- and this is one of the issues we have been talking about back and forth -- is what is going on here. And that is our whole issue.

We talk about something like Lathrop Wells, for example, or alkali basalts in general. They get here. And then they have to get to the Earth's surface. This is the viable window out here. This is the viable window back to 50 percent crystals. So how does it actually make that to the Earth's surface?

If we just brought it up isothermally, that's where it would be. And Steve was saying, well, he thinks it heats up a little bit. Other people think it goes down. We'll talk about this a bit. This is a very critical, critical thing.

So what we have been talking about in some ways is we have been talking about magma coming up at depth. It's wet magma. It saturates at some level with water. And it starts generating a bubble phase.

And there's a whole, of course, world, a very complex world, as has been mentioned back and forth, of when you go from basically a strombolian, pyroclastic eruption and from this thing totally fragmenting when it gets up.

It's kind of the reverse of the crystal process. When the bubbles get up to 50 percent and then 60 and 75 percent, it's a bubble world with little films of magma in there. It's not magma with a little tiny continuous phase. We call it a continuous and dispersed phase.

The continuous phase now is the air and gas phase. And so it goes from a region where we have the continuous phase down here as magma, the dispersed phase of little bubbles, and the process of actually how they ascend and get in and how it leaks out an whether we have chilled margins. I'll talk more about chilled margins and these things, how we get this out.

It's a very, very complicated problem. I don't think any of us really understand in detail, although in roads are being made all the time. But, nevertheless, we're faced with trying to get some sort of a problem on this.

So if we actually look at Lathrop Wells phased diagram, we hear a lot about Nichols' and Rutherford's point over here. And that is. And this is a dry magma calculated on the melts code. And we have even done a little bit of rock melting on it to make sure we are in the ballpark.

And so here is the liquidus-solidus. And so here is the Rutherford. If it is saturated Nichols and Rutherford, it has to actually -- and I have added a little bit of heat in it to come up here, but we'll look at this trajectory. In other words, it's below its solidus. It's below the point in the Earth's surface that would be 100 percent solid. This is very interesting.

Now, looking at how it ascends from about 200 megapascales, for example, this is a code done by Mastin and Ghiorso. And I've used it for various results here. But this also tells about a lot of other people who have done it, Papale and Sahagain, Proussevitch, and other people, Mark Ghiorso and Mastin, et cetera.

So this is an albite-rich. Here is the basalt system. It's not any different, really. But we'll just look at the system here with water in it. It starts up. And this is under isentropic system; in other words, no energy going in or out in terms of through the walls.

It's a constant entropy system. And you see as it comes up it's a huge amount. As it gets near the surface, we're talking here, up here at this region plus, you know, the upper 25 percent of the system. It is dramatic cooling. We're looking at 200 degrees of cooling in this region here.

Now, we can force it to stay isothermal by doing all kinds of things, and you would say this. So if we add in crystallization, for example, if we crystalize the entire system, we would add about 150 degrees. So we could caudally bring it up to kind of isothermal here. But if we had 50 percent crystals, we would only bring it up in part.

So this is a very critical thing. So Steve had been mentioning that it might heat up in this way but probably not. It probably will be at the best isothermal, all of these calculations we looked at in detail.

It's hard to really get around this and in this in general. But this is a point. You know, we don't know for sure. It's very hard to say exactly what it is. But, unfortunately, this is what we are faced with. This is in the upper level region where we deal with. But the best approximation I would say is that it probably comes up isothermally.

Now, if we actually look at more water contents deeper in the crust, a very interesting thing happens. You actually look at basalts coming up deeper. You add in other water. These things crystalize at depth.

They actually are starved out. They actually never make it up to the Earth's surface because of the fact you keep going higher and higher with water pressure. They just actually get so down so far they can't get to the Earth's surface at all. This is not only my work, but David Harris and a lot of other people have done this similar kind of thing.

So it's interesting that there's probably a filter, that the really wet stuff if there is anything down there can't get out of the Earth. And we're looking at stuff that's marginally wet. If we're forced, we get stuck at Yucca Mountain with four and five percent or three to five percent or two to five percent water. That's probably because we're down below this region, but that's the marginal stuff. But that's also the more dangerous stuff.

We actually look at the crystallinity at Lathrop Wells. These are samples that Dino collected and we have been looking at. We have been doing crystal side distributions and lots of things on these.

This is what people -- these are alkali basalts, but they're also trachy. In other words, they have aligned microlytes. These microlytes are very tiny, like Frank was saying. And you can see they have been growing, very, very rapidly because you can see the internal -- they've got these kind of sparrow tails.

You know, they're disequilibrium textures, they have been actually just quenched crystals, basically growing out very rapidly in these things. And these are the microlytes, which are this here. So you'll see here we're about 50 percent here approximately glass and 50 percent crystals more or less.

And in the spatter, Frank was saying in some of these areas -- he didn't say for this, but there might be 10 or 15 percent. So we're talking about, you know, maybe 20 percent crystallization or 30 percent crystallization. But even if we took 50 or 60 percent, I think we're still in isothermal.

But, actually, we can track this in the flows. We can track it three-dimensionally. If we have a whole sampling three-dimensionally and actually get down in the flows a bit, we can actually tell the crystal size distributions and with the thermal model, which we don't have time to go into.

We can actually tell a lot about what it was doing in the conduit, how it looked in the conduit coming out, but what the lateral temperature variations were. We hope to get on to that, actually look at some of that stuff.

So what are we looking at? Well, I've given a little bit here, but we're looking at something that's buoyant. It's going to be hitting right down here near the solidus. And what is it? Well, it's quenching to beginning with.

Let me mention one thing that is very important here. This is not a temperature quench. A temperature quench is a thermal migration from a proximal boundary. So it's like a surface traction, something that has to move in.

When you do a pressure quench, we're coming up here like this, the pressure is reducing. It's almost like we're changing. It's like a body force.

You take the pressure off this thing. It starts nucleating crystals inside because you see what it's doing here. It's going right across the crystallization region. It's going from the near liquidus right down to the solidus.

Depending on how fast it does that depends on how much glass you get over crystals. If it happens instantaneously, you get a glass. Although it's a high temperature glass and that glass will begin to crystalize as long as it's kept at high temperatures and it will start to get a decent texture, get the needles of crystals, all kinds of things growing in.

So how quickly it does that is very important in terms of how many crystals you actually can get because there is a finite rate of nucleation in these processes.

So this is a very important thing, this pressure quenching. And this is the world we're faced with in here. And that's why our problem is so complicated.

A little bit about the generalized rheology now. If we talk about it, if we're talking about a system, like I talked about at depth, where crystals are growing and marginal solidification fronts or systems that had picked up a lot of crystals in them, sludge, for example, all kinds of stuff that they have in, we have to take into account a basalt plus crystals out here.

We start at something at 1,200 degrees. We've got to worry about where it is in the liquidus/solidus region because you saw it goes up without limit, basically. In fact, this is probably the biggest physical property variation that we know on the Earth. It goes by a factor of 10^{14} and changing by about 100 degrees. It's terrific.

Now, if we go into the glass phase and we go through this thing and don't allow these crystals to build up, we're on this. This is what Lore, *et al.*, use as a curve. They went through. And they're basically assuming that this thing goes to some kind of a glass and goes through here. Here's the glass transition temperature. So this is a different world.

So, in other words, I'm not saying that, really, we have this in the eruptive column. We probably have a combination. But we may be on this. But the key issue here is that if we get a temperature like Nichols and Rutherford says of 1,100 or 1,050 or something like this, we just can't go down to this curve and take it. We've got to know where we are relative to the liquidus and solidus. You've got to actually know.

And so the better thing to do is normalize this thing, make this dimensionless. So we start out at one at the liquidus. And then you calculate your dimensionless number for where you are because that sample to Nichols and Rutherford, that was at the liquidus. So that had a very low viscosity. So you have to actually worry about the process before you can actually choose the viscosity in this thing. So that's very important.

So we just can't take their thing and go down to it and say, "Well, down here this is going to be it or it's going to be way up here." We have to actually know about the process going on, whether it's glass or whether it's rubbly or what it is.

If we worry about the effect of bubbles -- and it's interesting. Here's a capillary number. All you want to think about is these are little, small hard spheres. These are distributed small, little bubbles in the magma. They're hard. In other words, they're very, very tiny. They have a high surface tension on the surface. So they're strong.

So they really don't do much to the rheology in this. They increase it somewhat a little bit but not much. And then if you have drawn out bubbles that are large and all sheared and drawn out, they basically kind of lubricate things a bit.

But we're not talking about huge variations here. We're talking about variations, certainly not a factor of ten in this thing, so just in terms of having some bubbles stuck in the system, so when you go from one thing that's fragmenting and everything else back and forth.

So when we talk about degassing, we don't know how it degasses. But obviously it does, maybe not, though. I'll talk a little bit about that. It may be a little differ. But obviously we go into some sort of phase like this.

Now, we've got to worry about a few things. Now, this is Springerville volcanic field. This is some geothermometry from phenocrysts, clinopyroxene in there by Keith Putirka and others. And this is actually a field I think Chuck worked on at one point.

DR. CONNOR: Long time ago.

DR. MARSH: Yes. It's very interesting just to take their data and put them on a PT diagram like this, like a Lathrop Wells type thing, and look at them. These are alkali basalts also and not too different. They're actually in a, I think, -- what is it? -- eight million, up to about a half a million or 500,000 or one and a half million, something like that.

But let's look where they are out here. They're very interesting. So if we actually adjusted the phase diagram, brought it out to where it would be here, we're talking about this whole system then dominated by maybe things that only have a one percent water in them or something like this.

So when we're talking about back in here, we're talking about this point back here that puts us here. I want to add some caution to us all for basing a lot of what we're saying on that single piece.

Now, I have looked through a lot of the Lathrop Wells and other thin sections out there. And I have not seen any applicable phenocrysts at all in them.

Now, I'm not saying they don't exist. I've seen the photomicrographs that have been published. And they're certainly there. But we should be very careful making sure that we're relying on that.

Alkali basalts are full of junk. They're full of stuff. And most systems are full of all kinds of things. If you look around enough, you can find all kinds of things in these things.

So we've got to be careful about relying entirely on that rock. And there should be other ways we can get at it. I'm not saying disregard this. I want to say we want to back it up. We want to reinforce our conclusions of this. It might be good to look at some other geothermometry in some of these.

And so look at the rock. We haven't seen any pictures of rocks. The first one I've shown here, photomicrographs and stuff. We should be looking at.

One other thing we want to be thinking about is we're thinking about magma that's really homogeneous. In other words, if it has a certain amount of water, it all has that certain amount of water.

If we think about the eruptive column coming up, we say, "Oh, this stuff is at the top. It degasses. And the stuff below it has to get rid of a lot of water." Well, maybe the stuff below didn't have a lot of water in it.

We talk about stuff being generated in a parcel. One of our assumptions we always make, bald-face assumptions, is that things are homogeneous. By no way if you look at systems in general, they're not isotopically homogeneous. They're not homogeneous in trace elements. They're probably not homogeneous in water content because water, it's hard to diffuse it round. It has very low diffusivities.

And so what can happen is you can generate a parcel to magma. The leading part of it could be the slightly lower density stuff. That's the stuff that goes ahead.

This is another effect. This is the effect of the gravitational potential, gravitational chemical potential, gravitation effect on the chemical potential of water equilibrium in a column.

This is shown by Kennedy over the years and John Verhoogen and McBirney have talked about. Lots of people have done it. But I want to caution you. This is the saturation surface and pressure and water content down here.

And this is for if your saturation is at 50 megapascales, 100 megapascales. But this is what the gravitational contribution would say. It would say if you have a standing column that goes down this far and it was an equilibrium, chemical equilibrium -- some of us talk about equilibrium. Some people say we don't have equilibrium, but we probably have some combinations where we are.

This would be the distribution of water. Now, I actually don't adhere to this kind of thing because I think that magmatic systems are so tumultuous in many ways it's very hard for them to achieve this, but this is something they want to try to get towards. But we also might think about the buoyancy in this in terms of water, in terms of the magma being slightly different densities in the column basically being not homogeneous from bottom to top.

And so a conservative point of view would be to take and say, "It's all water," but a better point of view would be maybe to try to evaluate it, try to figure out during the eruptive sequence that things change.

Maybe it isn't just a gassing that we have a problem. Maybe it isn't that much of a problem for us. Maybe water content is actually changing. Okay?

So we look at these things out here near Yucca Mountain. And we wonder about, you know, what is really going on in these. And so we can look at some other things that are coming out.

One of the things is, as various people have mentioned, Meghan and other people, Bruce Crowe and other people, of the proximal -- and there's this nice paper now by Greg and others on Crater Flat flows and things like this.

If we look at these things and model these flows, there's about one kilometer, by and large, for a lot of these things and you look at these things in terms of the effect of viscosity of 10^{7} cgs. This would be about 10^{6} pascale seconds it would be. And in two days, that's how the flow would go if we took, actually, any kind of flow.

I used Herbert Hubbards' here, gravity currents, but you can take incline plane. I've done many of these. And this is treating it as a viscous material.

And I'll mention there are other caveats and problems with just modeling these things as a viscous current spreading on the table. You pour it on the table. It spreads out.

You can say, "Okay. How the volume, how the flux change over time." You can say, "Well, I think it's actually a granulated kind of a debris flow that's elastically coupled. And there are just big chunks."

You can do this all kinds of ways, but this is one approach. And you can look at these. And they are almost mutually exclusive, unfortunately, for us. But that's two days. Here are ten days. You can see where this thing would go, not to mention further.

Now, you could say, "Well, we don't have that much magma there." Well, it doesn't matter. The lava just gets thin, would travel out thin. If it was just water, it would go a huge distance in this. So this is one thing to keep in mind when you do this.

We put it up to 10^9 cgs units, 10^8 pascale seconds 10 days. This would be 20 days. These flows, these things we're looking at out there, they may have had a month long. In terms of just looking at them, they look like the type of flow at the end of a month or two months or something like this. And, of course, there are multiple lobes. And it has something to do with the topography, detailed topography, et cetera.

But this gives us an indication, at least, of some numbers that we can deal with in some ways. And we can work from there. I've kind of put this at the upper end because what is it? It's a dry debris kind of system. If it's viscously coupled, it also has these microlyte buildups in it, these crystals building up. So it's an extreme. We're looking at an extreme on this end of it.

If we took this and we look at this in terms of -- I wonder why we're missing out part of the slide. Anyway, the radial flow distance of these, this is for a total volume of .03 cubic kilometers, like at Lathrop Wells.

And this is the radial extent. And these are kinematic viscosities of 10^{10}, 10^{9}, 10^{8}, 10^{7} on this. And if you look at this around a kilometer, these are the kind of numbers you get, somewhere between 10^{8} and 10^{9} in these.

And here is for various volumes. You can start increasing the volumes. And if you get the volumes up and you want to keep it at the right distance, you've got to increase the viscosities. There are all these trade-offs, but it is interesting to see the trade-offs and where they are.

So if we say, "Well, we use the wrong volume," okay. Well, we can adjust these things. These are not that hard to do. But, in effect, it's this kind of number is what we're talking about here.

MEMBER HINZE: Few minutes?

DR. MARSH: No. It's going to be more than a few minutes.

MEMBER HINZE: We can come back.

DR. MARSH: Okay. One of the other hard points you can put your hand on now is the rheology of glasses. If you look at pure glasses, one of the interesting things about glass, high-temperature glass, is that if you look at basalts to andesites to rhyolites, we know in terms of the viscosity of these magmas they have a huge range.

But in terms of glasses, we're talking about $10^{12.6}$ to 10^{13}. In fact, if we bring them all at the same temperature, you'll find that they're actually quite similar. But, anyway, we're up in a range. This is the glasses now. We're just talking about pure glasses.

And so that is a very interesting region where we would be in these kinds of things. So we worry back to what we're erupting at, where we are, and how much glass. Crystals, of course, can stiffen it up, too. So we worry about these things.

We talk about scenarios in the -- I won't go over these. We all have these in the back of our mind, various scenarios, and stuff flowing along and maybe coming back out, et cetera. And so there are all kinds of different problems in these.

Now, I want to say in terms of penetration, if you take these numbers like we have been talking about, 10^8, 10^9, 10^{10}, you get these kinds of drift diameters with the packages in there 4 meters, et cetera. You don't get very, very deep penetration. So you want to bring the viscosity way down. You certainly get more. It's a perfectly good trade-off if we can do it.

The other big factor we'll see in a minute is what we're using to drive the flow. I think this is very important. If we look at the flux in general, we're driving it with a pressure gradient.

And there is a function out here of the radius. And the radius changes as you're cooling inward. The viscosity changes, of course, spatially, but this is a very interesting thing.

If you just have an inclined plane flow like on the surface -- and basically that's something I've used in the last calculation, you have this -- and perhaps something that's coming up from below. We have this flux equivalent to this flux.

And you say, "Okay. Well, what is it? What's this column, this density column?" Locally it may have a very low delta rho. Actually, the delta rho may be positive. It may want to go down. It's a problem we have in the upper crust. It's hard to know exactly what it is.

Now, the other thing you can say is "Okay. We're 300 meters down. We can take delta P equals delta rho/GH. Depending on what we use for delta rho, we can get up to ten megapascales" or something like this. But this is also specious in many ways because when this thing opens up, it actually has unrecoverable deformation.

Dikes go in. You can actually look at these in the wall rock. They deform the wall rock. And after especially in stuff that's partly granulated in things, after about five or six dike radii out, they're deformed. There's no evidence in the rock there is any deformation taking place.

So if we actually took the magma out of here, the hole would stay open probably. We have many mine shafts all through the West that are full down 300 meters. It's not a problem.

So in terms of squeezing the magma out all the time and having this, this is another upper bound probably. So we want to worry about that. In other words, whether it's being forced in here at this to zero pressure, that's a question.

The fact that we get high-level sills, very high-level that want to go laterally, rather than come out, is also an indication that it's a more complicated choice in here than we have, especially if it gets rubbly in here and things like this, of what it is. It moves down the drift. It's very important.

The other thing to think about is that we have a propagating fissure up here. And this thing may be one or two kilometers, for example, going across and may be venting at the surface when it's done venting down below, a little small area. And we have had precursors of power plastic debris and scoria buildup. We all have to worry about this.

In other words, the initial conditions we assume when we start our problem is enormously important when we start out. We've got to know where we start, why we start. It's very hard to start an Earth problem clean with the right initial conditions in these things. So, in other words, we're going down the stretch.

The other thing is the quenching in here. Magma quenches on everything. Why? This is the Hawaiian stuff. It's because it's in the crystallization range, everything it touches.

How do Hawaiian geologists get samples? They take a wire and a steel hammer. They throw it out there. They pull it in. It's got a big gob of stuff on it. Okay?

And this is spatter in trees. It goes along things in general. I've done a lot of calculations on this. Here's MacCulloch's tree in Scotland. And you can see the quench margins along that. And here's this Eocene conifer in here that had these quench margins. You can calculate these. And you can show this quenching. You can show how long it takes.

And alkali and basalt nodules have quench margins. Also, they have vesiculation margins around them. And these vesiculation margins may do the cooling, too. And this cooling may quench this material out. It may help boy these things up in time. And we can calculate the quenching of these things.

This is in terms of time over a minute. You get something like you can get ten centimeters or so in some of these things in terms of what it is.

The same kind of calculations. These are my calculations up for the -- here are the lava lakes. Here are lava flows in Hawaii. And you can see. You can calculate these. They're all basically the same kind of processes that go on in these.

Now, in terms of the detailed cooling process, I'm not going to lead you through all of this, but what I want to show people is that in general when we have something in the conduit or up against the canister, this is -- we have a sheet, and we have cooling. It doesn't matter how deep it's buried, really. And you can run through this thing. And you come out.

Is it the contact temperature? Really, that temperature right there is really the average of the two initial temperatures in this system. We add latent heat in. We get a little bit more out of it but not that much. It comes up a little bit more.

So what happens when we actually do the full problem and say, "Well, could it melt back?" We can do the whole problem, the whole Stefan problem of this and say we have a moving interface. Where it's going out here, it can go back and forth. The front is given by this. This is the parameter, this B parameter. It's very important to know. Unfortunately, you calculate it from this right here. These are all the different thermal properties. We can simplify it down.

But it comes out. It says that the magma temperature plus the wall rock temperature has to be equal or greater than twice the melting temperature of the canister of the material that it's in contact with twice before it will actually cause the melting to go backwards after it starts quenching. It will move backwards.

That's because these things are cooling

down. The whole process of magma is once it's up, it's cooling down. It's in the thermal crisis. It's starting to cool down all the time in this.

One area I want to talk about showing some examples, it's one thing to show --

MEMBER HINZE: We have to --

DR. MARSH: Yes.

MEMBER HINZE: One minute. Okay?

DR. MARSH: This is wrapping.

MEMBER HINZE: Okay.

DR. MARSH: In the Antarctic region, I want to show you both sides of this issue where we actually get melting. We're looking at these big sills, these big sheets that have come in all over the whole region.

We're talking about 150 kilometers in these sheets of magma coming in in detail. You can see all the way up through the whole system. And so we have a lot of exposure, as I showed you before.

And here is what you see at the margins of those big, long sills, those sills that have gone 100 kilometers. This is granite, and this is the dolerite. It has picked up and broken up the granite, but there is no melting at all in here. These are all coarse grain.

But look at this stuff. It's very fine grain. This is what we call a quench. These are 300-meter-thick sills that were being periodically put in. You can see it breaking out pieces and things, but there is really no melting in the granite.

This whole region is like this, this whole thing. You can follow these things for tens of thousands of square kilometers. And you see this quenching in this region, all through this area, these fine grain areas on the margins, coarse grain in the interior.

Now, we do have areas where there is melting. And these are in areas where you go from one sill to another where an enormous amount of magma has passed up through -- and I mean an enormous amount, hundreds of cubic kilometers -- from one body to the next. And what we find is it actually has melted the granite. It has formed a chilled margin, but it has actually melted the granite. It's very interesting. It's a fascinating area.

This is the chilled margin of the dolerite. And then what has happened, it has melted the granite, compacted the granite, got a melt out of it. And then the granite magma has ripped open the dolerite chilled margin.

Here it is here. You can actually see there is part of it. Here is the other part. Here it goes around here. There it is right there. There is the chilled margin. Here is the granite magma. It's actually going, ripped it open, injected back out in the dolerite, fascinating sort of thing, this kind of a process. And these pluses and minuses, the stress field and things due to cooling and overloading and things like this.

So the bottom line in these kinds of things, we have a tough problem ahead of us working on this. And there are lots of little areas in there. So the dynamics and the physics and chemistry we want to check, we want to be very careful.

Certain things we are not going to get to I don't think in terms of understanding the entire details, but we want to make sure we don't mix and match here the systems anecdotally that have up here or have longer histories, bigger volumes, different things, and just add them into the system in general. So these highly mobile, for example, Hawaiian things, we don't want to stick them in there.

So we have the hardest problem of all. And I think if we could do this, we have a major feather in our cap. It's a challenge. And we're all on the same side, whether we realize it or not. And I think we're converging, whether we realize it or not.

That's it.

MEMBER HINZE: Okay. Well, thank you very much.

DR. MARSH: Sorry.

MEMBER HINZE: That was a great movie.

(Laughter.)

MEMBER HINZE: That's said in jest. I suspect there's going to be a little discussion about some of the things you presented, but we'll wait for that until after the next presentation.

And, Neil, I'm going to ask you to help me through this. The next speaker theoretically, at least, is Dr. Art Montana, former head of the department at UCLA, who tried desperately to reach here yesterday and spent an hour and a half on the tarmac in Madison, Wisconsin in an airplane waiting, trying to get here. But he supposedly is on the telephone bridge.

And he is scheduled to discuss the thermal and mechanical magma/canister interactions associated with the intrusion at the proposed repository.

Neil, where do we go from here?

DR. COLEMAN: I will work the slides on this end.

MEMBER HINZE: Okay. Dr. Montana, are you with us?

DR. MONTANA: Yes, sir. Can you hear me?

MEMBER HINZE: No, I didn't hear you.

DR. MONTANA: Can you hear me now?

MEMBER HINZE: We'll turn it up from this side. If you'll say something, we'll judge to make certain that we can hear you.

DR. MONTANA: All right. Good morning.

MEMBER HINZE: Okay. That's good. And I think he can be heard. So let's move on.

DR. MONTANA: All right. Thank you very much.

THE THERMAL AND MECHANICAL MAGMA/CANISTER INTERACTIONS ASSOCIATED WITH THE INTRUSION SCENARIO AT THE PROPOSED YUCCA MOUNTAIN REPOSITORY

DR. MONTANA: Good morning, everyone. I appreciate this opportunity to present some of my opinions and interpretations regarding the design of the Yucca Mountain facility.

I regret that I was unable to attend. They canceled my flight just as we were pulling off from the gate yesterday. And then I was informed that

Washington, D.C. was canceled. Bill called me and told me that the meeting was to be canceled. So, anyway, I'm sorry about that.

Listening to me this way would be like listening to someone's telephone conversation in a restaurant, I'm afraid. The advantage is you can't throw anything at me.

(Laughter.)

DR. MONTANA: At the onset, I want to say that I am really impressed with the work that has produced the large number of DOE, NRC, and EPRI reports. And as I listened to Bruce, I realized just how far the field has advanced since I was plodding along. I've done my best to interpret the opinions of the authors of these reports, tainted, of course, by my own prejudices.

I have been asked to consider the interaction between the alloys and the containment vessels and the volcanic fluids, magmas and vapors. I will get there in a moment.

The uncertainties arise when we set off to calculate the likelihood that an igneous event will occur at the Yucca Mountain repository during the time period under consideration.

Again, I'm impressed very much with the

results of those who were charged with the geologic and volcanologic studies. No one could have done it better.

The problem that immediately arises is that we are dealing with the probability of a single event. And statistics cannot be rigorously invoked in such instances.

For example, if you toss a penny ten times, statistics will allow us to say with some confidence that the likelihood of yielding four to four, five, or six heads is pretty good. Toss that same coin once, and the statistics tells you nothing about whether it will come up heads or tails. It either comes up heads or it comes up tails.

The first slide, Neil?

DR. COLEMAN: We are on number one.

DR. MONTANA: Are we all right?

DR. COLEMAN: Okay. We're now on slide number two showing inside the tunnel.

DR. MONTANA: No. We should be on slide one.

DR. COLEMAN: Okay. We are.

DR. MONTANA: All right. For example, when the Weather Service proclaims that there will be a 70 percent chance of snow in Butte, Montana tomorrow, they're not talking about a single event. Rather, they mean that in the 100 years of keeping records, it snowed 70 percent of the time when conditions were similar to those that they predict for tomorrow. So *n* there equals thousands, not one.

Geologists are not afforded such luxury, even though we're geologic time. Certainly occasionally we're afforded an opportunity to make an informed guess, such as at Yellowstone, where the last three eruptions have had a repose time of about 600 to 700 thousand years.

But igneous activity in Nevada forms no such predictability. So we simply cannot afford to design a repository for high-level nuclear waste without assuming that an igneous event will occur and that it will impact the canisters.

Now, should the DOE and the NRC insist on continuing with Yucca Mountain as the repository, serious consideration must be given to designing the drifts so that they can be backfilled. And I realize that "backfilling" is a dirty word, but it is the solution that most minimizes the risk given the uncertainties that otherwise are involved.

My father was a hard rock miner. And we lived in mining camps throughout Montana, Idaho, and

Wyoming. Naturally I became a miner and then earned my undergraduate degree in mining engineering before switching to geochemistry. As a miner and a mining engineer, I have backfilled many, many drifts and stopes, put in the first stope below a 5,000-foot level in the Butte district.

This causes me to wonder given all of the dire consequences that might arise if and when such an igneous event does occur why not backfill the drifts containing the containers. Regardless of the cost, we must opt for safety and predictability, rather than adhering to preconceived concepts of accessibility.

The next slide, Neil?

DR. COLEMAN: Okay. I have it on the tunnel slide now.

DR. MONTANA: That's correct. You're all familiar with this picture, of course. I just want to say that in some respects, the process of designing the repository appears to me to be totally backwards.

You are presented with an arrangement that includes drifts of 5.5 meters in diameter, canisters at 1.6 meters in diameter, certain configurations for the drift shields, a 20-millimeter gap between the stainless steel alloy, stainless steel containers, and the alloy-22 surrounding shields, a 4-millimeter gap between these. And then and only then are we asked to assess the susceptibility of the canisters to intrusion of groundwater and the products of volcanic activity. Perhaps my perception of this process is incorrect.

Okay. Let me say something about the susceptibility of the containment vessels, the canisters to corrosion and failure resulting from magmatic activity.

Now, I work with steels and other alloys under what I would call extreme conditions. And I have firsthand experience with non-ferrous alloys similar to that being considered as a protective envelope around the stainless steel containers as many of the pressure vessels that I designed and used in my experiments at high pressures and high temperatures were cobalt-based alloy and nickel cobalt-based Rene 41, which is similar to your alloy-22. I also have considerable experience with molybdenum alloy pressure vessels, which I hope I can mention.

Considering the potential chemical interaction between magma and the canisters at elevated temperatures, the 2004 EPRI report used several sources of information to assess the extent of corrosion of the alloy-22 shelves surrounding the stainless steel waste canister when contacted by magma, supplementing the scarcity of available data with the information on corrosion in high-temperature glasses and molten salts.

Then, in addition, they used in their evaluation information on various nickel alloys as the data, as we will see for alloy-22 itself, were insufficient to determine the temperature dependence and the corrosion rates.

Next slide, if you will, Neil.

DR. COLEMAN: Okay. We're on slide three.

DR. MONTANA: Yes. It should be one with a table up at the top.

DR. COLEMAN: That's it.

DR. MONTANA: These are data on alloy-22, which is being proposed as the shield surrounding the stainless steel container. You'll see that alloy-22 is largely a nickel alloy with lesser amounts of chromium and molybdenum and even lesser yet amounts of iron and cobalt

Then because data for alloy-22 are limited, they used, "they" being EPRI used, alloy-X and Incon el 625, for which more data were available. Alloy-X is similar to alloy-22 except that the iron to nickel ratio is higher, as you can see.

Incon el is again quite similar. The point I want to make here is that the chromium content of these three alloys is quite similar. And that currently is an important feature.

If you look at that graph down at the bottom, don't look at the details. I just simply want to point out here this is for alloy-22. You'll see that the data on the tensile strength and the O strength offset strain, if you will, terminated about 760 degrees Celsius, nothing available that I was able to find above it.

The next slide -- Neil, please.

DR. COLEMAN: Okay. We're on slide 4.

DR. MONTANA: -- shows figure 5-27 from the EPRI report of 2004, which plots corrosion rate on the *y*-axis, the vertical axis, in microns or micrometers, if you prefer, versus per day, micrometers per day on the vertical axis, versus reciprocal temperature in one over Kelvins on the *x*-axis.

Let's see. If you look at about, oh, let's say, .14 on there, just to put you in terms of temperature you might appreciate more, .0014 on the right would correspond to about 450 degrees. .0009 in the center would correspond to about 850 Celsius. And

1430 K would be approximately 1,150 Celsius.

And, as the legend says, this shows a temperature dependence of the corrosion rate of nickel chrome alloys in molten electrolytes. And, again, you can see far better than I can with my yellow-red color blindness that the data for alloy Z-22, cast alloy-22, only go up to about 760 degrees Celsius.

Anyway, they took the data from those three alloys at the top primarily and drew a best fit curve and concluded that the corrosion rates at magmatic temperatures range up to about 30 microns per day.

Now, this corrosion is similar for all of the chromium-containing alloys, suggesting to the investigators that it's primarily the oxidation of chromium itself to chromium oxide, Cr-203, providing a protective coating. Other mechanisms are possible, of course, like sulfidization.

Now, Lai, -- I'm not certain I'm pronouncing his name correctly. It's L-a-i --*et al*., in the number of reports in the 1980s and 1990s published by Haynes, the makers of the steels, -- I sent away. I received all of Lai's papers and talked to the folks.

Lai tested the corrosion of various

stainless steels and high-temperature alloys in the presence of oxygen, sulfur dioxide, carbon, monoxide, methane, chlorine gas, hydrochloric acid, and others.

Now, their results also revealed that the formation of a coating of Cr-203, chromium oxide, in chromium-bearing alloys provided protection from attack by other components.

But an important point that they note that I didn't notice in any other reports was that at temperatures above 1,000 Celsius, the chromium oxide became volatile. Unfortunately, I was unable to find more information about that process. It might be worth looking into it if you haven't.

Interestingly, the same group discovered that Cabot, C-a-b-o-t, alloy number 214, performed better than alloy X under oxidizing conditions at 1,100 degrees for 1,000 hours because of the formation of a refractory coating of aluminum oxide, Al-203, that forms from the 4 and a half percent aluminum content of the alloy. You know, the nominal composition of alloy-214 is not greatly different from alloy-22 except for the aluminum.

I found no evidence anywhere that similar aluminum-bearing alloys were considered for Yucca Mountain. Maybe they were. Maybe they weren't.

Perhaps one of the studies most relevant to Yucca Mountain is that of a Douglas and Healey in 1981, who investigated the oxidation sulfidization of unalloyed chromium and unalloyed nickel in basaltic liquid at 1,150 Celsius for as long as 96 hours.

The combined effects of oxidation and sulfidization reached about 20 microns per day, with chromium again performing better than nickel, apparently because of the formation of Cr-203.

And then more recently Findlan and Peterson in 2004 conducted experiments for EPRI using alloy-22 immersed in molten Hawaiian basalts at 1,200 degrees Celsius for periods from one hour to two weeks.

Maximum penetration of a corrosion front in the longest experiments was about 300 microns, which would average about 20 to 30 microns per day, which is consistent with the previous data.

The next slide shows that, the results of that. It shows the crucible removed. It shows the quenched basaltic liquid and either the chromium or the nickel ring inside. It looks pretty good after being at 1,200 degrees for 1 to 2 weeks, but closer examination showed that it was, in fact, corroded and pitted.

Now, Westridge in 1990 investigated the corrosion of various alloys in rhyolitic liquids at 850 Celsius, particularly using Incon el 625, which was on that previous slide.

Now, rhyolite is more oxidized than basalt, but it's also less sulfidized. The corrosion rate there averaged about 25 microns per day. So these data seemed to home in on a figure of 25-30 microns per day.

Then EPRI in 2004 presented the results of modeling, concluded that the most important parameters, the temperature difference between solidus and liquidus; that is, the temperature interval over which the canisters would be at contact with molten material, as you would expect, of course, because of a greater diffusivity of catines and anines in the liquid state.

With this in mind, they concluded that most of the corrosion would occur in the temperature range of 1,150 Celsius to 800 Celsius. EPRI's conclusion from these studies was that no waste package would fail during an igneous event at Yucca Mountain.

Now, assuming that basaltic magma penetrates the drifts at a liquidus temperature of

1,100 Celsius, -- it could be less, it could be more -- the DOE report of November 2004 concludes -- and here I'm going to read this and quote -- "Even if magma were to penetrate a waste package, the magma outside of the waste package is expected to stagnate once the drift is filled on the order of 1,000 seconds, approximately 17 minutes so that there are not likely to be driving forces that would flow in through a waste package. Magma is likely to fill the drifts before the waste packages heat up to a point of failure."

Then they conclude that "In view of these results, it is safe to conclude that in the absence of major cracking of waste packages, a significant amount of magma will not flow into or through waste packages and that the waste forms will remain in place."

While that may be so, I certainly have no expertise here. But if a dike propagated to the drift might not continue up through the drift, possibly to the surface, resulting in a more prolonged flow of magma through at least one or more of the canisters.

And the December 2006 NRC report poses a similar scenario, stating that DOE and NRC now agree that it is likely that a dike intersecting a drift might -- or they actually say "will" -- continue to insert.

I might say at this stage that I remain unconvinced that there are adequate experimental studies to support the claims that alloy-22 shells will be inert to failure when exposed to magma and attendant vapors. Possibly there is no alloy that would provide the desired assurances. However, there is another alloy that I would like to bring to your attention later on, time permitting.

I want now to speak of the effects of corrosion on the tangential tensile strength in the containment vessels and the surrounding shield. For the moment, let's assume that magma contacts a canister or canisters and destroys the outer 25 percent of the alloy-22 shield, a value that's consistent with the maximum value in the EPRI model. That is to say that the outer 5 millimeters of the 20-millimeter-thick outer shell of alloy-22 is destroyed by prolonged contact with magma.

Now, for those of you who have experienced the wonder of an overcooked hot dog, you will notice whether you put it over a fire or boiling hot water that it ruptures because of internal pressure. And it ruptures in a direction along the hot dog. It ruptures because of internal tensile strength in a tangential direction. And it doesn't tend to pull the two ends apart. So you get fractures that run tangentially. That's the weakest part of a hot dog or of a waste canister.

If I may have the next slide, Neil?

DR. COLEMAN: You're on slide six.

DR. MONTANA: Pardon me?

DR. COLEMAN: We're on slide six.

DR. MONTANA: Okay. I can't view mine and come out with what number you have there. This should be a circle with a P in the middle for internal pressure, showing the alloy-22 shell. And up at the top, we show the internal tensile tangential stress, the sigma for stress, the $_t$ for tangential.

That is the weakest part in any cylinder. The inside surface being pulled apart by tension is where things always fail. And it's an easy matter to calculate that tensile strength knowing that the pressure is in a thin-walled pressure vessel, which all of these are. For a thick-walled pressure vessel, it's a different story.

Let's go to the next slide, Neil, if you will.

DR. COLEMAN: Okay. That's slide seven here, showing three circles.

DR. MONTANA: Three circles. You're right. Okay. So let's take a look at this. All three of these show the alloy-22 shell. The one on the left shows the uncorroded alloy shell, 20-millimeter wall thickness and its tangential stress at the top. Some people refer to that as hoop stress. That's okay, hoop stress or tangential stress.

And if we calculate that tangential stress, it turns out that it will amount to 40 times the internal pressure, whatever the internal pressure is. If we corrode the outer 5 millimeters, the outer 25 percent of that alloy shell, as shown in the middle circle, then the tangential stress is 53 times the internal pressure, an increase of about 30 percent.

If we happen to be in the situation where we lose 75 percent of that 20-millimeter-thick shell with leaving a thickness of 5 millimeters, then the tangential stress becomes 160 times the internal pressure.

For the 316 stainless steel vessel with a wall thickness of 50.8 millimeters, the tangential stress is only 15 times the pressure, the internal pressure.

Now, if a magma contacts a waste canister, the internal temperature and pressure will, of course, rise. And a simple calculation shows that that would be initial pressure of one atmosphere. Now, if you heated it up to 1460 Kelvin, you would have to multiply that internal pressure by 5. And that would go from .1 mpa up to .5 mpa.

Oh, by the way, let's see. One of the figures -- and I'll have to go back. Figure 5-23 in 2004 EPRI report shows the effect of a 12-meter column of magma applying compressive stress to the shells, which would tend to offset some of the tangential tensile strengths. They calculate that a 12-meter column would provide about .5 mpa of compression. And my figure comes out to be .3 mpa. So you might want to check that if you ever publish that anywhere.

In other words, they show initially when there's a column of magma, a 12-meter-high column of magma, overriding the canisters, that it would initially be under compression as long as you had that magma in contact with the alloy. But the way I look at it, it wouldn't be. It would still be under tension. It's a small matter, but, nevertheless, check it out. Check my math, too.

By my calculation, thermal expansion of the alloy-22 shell, at 1,000 degrees, it might be much higher but at 1,000 degrees will tend to widen the initial 4-millimeter gap between the canister and the alloy shell by about 7 millimeters.

In other words, originally there's a 4-millimeter gap between the stainless steel canister and the alloy-22 shell. And if that shell was heated up to 1,000 Celsius, that would expand by 7 millimeters, bringing that gap up 11 millimeters.

But at the same time, then, if the stainless steel heats up to 1,000 degrees, that will expand twice as much, to 14 millimeters. And that would eliminate the gap. And it would also tend to compress the stainless steel canister, which is good.

But also, Neil, at the same time, would you change to the next slide?

DR. COLEMAN: Okay. Slide eight, one large circle.

DR. MONTANA: Right. At the same time, it would add to the tangential tensile strength in the alloy-22 shell. So looking at this slide, that outer, lighter-colored shell is the alloy-22, which, in effect, would become shrunk-fit onto the stainless steel or press-fit.

So the weakest part of that configuration is still the inner surface of the stainless steel, but shrink fitting that outer alloy on would decrease the tensile stress and the steel. But it would increase that $sigma_t$ in the outer alloy shell. So that adds to the problem.

MEMBER HINZE: Art, this is Bill Hinze.

DR. MONTANA: Yes?

MEMBER HINZE: We have about five minutes left for you.

DR. MONTANA: Okay.

MEMBER HINZE: And so if you can hit the real essentials of your talk, we would appreciate it. Thanks so much.

DR. MONTANA: So the scenario here would depend on the temperature and duration of the heating of the alkaline shell, parameters that seem to me not to be well understood.

All right. So let's go to the next slide, if you will.

DR. COLEMAN: Okay. Slide showing temperature versus strength.

DR. MONTANA: That's correct, yes. This also is from EPRI. And it shows the strength of the various alloys, temperature in Celsius versus strength in FTA. And you will notice that the UTS is the ultimate tensile strength.

You will notice that the values for

alloy-22 again only go up to 760 Celsius so that they use alloy-X, the ultimate tensile strength, UTS. For values above that, you can see the tensile strength drops off markedly at above about 800.

It's the same for the welded variety, which is the GTAW, the gas tungsten, our welded variety. And then the creep are the .2 of a percent offset values down below.

So if we take our previous value of, let's say, .5 pascales in the interior pressure after it's heated up to 1,000 degrees, the alloy shell, and multiply that by 40 in the uncorroded shell, then you would end up with a tangential stress of about 20, of course. And you can see at magmatic temperatures that the strength is higher than 20.

If we corrode the outer 5 millimeters, then we have to multiply that .5 by the factor of 53 that we calculated. And that's about 27, of course. It's a little dodgier. If we lose 75 percent of that outer shell, we have to multiply that .5 by 160. Then we're up to 80. And it's getting, as far as I'm concerned, dangerously close.

And those data, again, remember, are for alloy-X. They're not for alloy-22. It's some disquieting to realize that we're working with data from an alloy that doesn't exist. Okay?

I have to repeat here, too, that EPRI assumes lower contact temperatures from the magma than do DOE or NRC. And I also want to point out here that we haven't considered the added tangential stress from thermal expansion that I went over before. So, anyway, it's a complex scenario, not without some uncertainties.

And we must also be aware here that the world is not perfect. Imagine a package weakened, say, when a burly, 300-pound worker hits it with their wheelbarrow. And the ASME always recommends a safety factor of at least four when designing pressure vessels.

Okay. I'm just about done here. I'll skip a few things. Let's see.

Okay. Let's assume just for the moment that canisters survive immersion in magma. Another situation that concerns me is the following. In the event of a volcanic event, let us assume that the canisters retain their dimensional and chemical integrity, although intersected by the magma, and that the drip shields are partially or completely compromised.

If the surviving canisters are partially entrained by the magma, what will be the next step? Will they be exhumed or will they be left unattended without drip shields and with weakened alloy sheets? These are, as far as I'm concerned, serious considerations. To my way of thinking, they beg for initial backfilling.

Well, my last statement is that my conclusions are either backfill the drifts or give serious consideration to abandoning the Yucca Mountain site.

I realize there are good reasons to have it in Nevada. We have already destroyed the Carlin Trend with the open pit mining. And the nuclear test site has done its job. And also it has a nice low water table. Those are good advantages.

But I see nothing to be gained by speculating about the probability of an igneous event at Yucca Mountain. We should accept that it will happen and enter the repository accordingly assuming the worst case scenario for temperature, corrosiveness, duration, and momentum of the magma.

MEMBER HINZE: Thank you very much, Art.

DR. MONTANA: One last slide.

MEMBER HINZE: Oh, we've got the last slide. Okay. Are we at that? Is that the last one?

DR. COLEMAN: Lithostatic pressure.

DR. MONTANA: It should be a cartoon.

MEMBER HINZE: No, I don't think that's a cartoon.

MEMBER HINZE: No

DR. MONTANA: Did Neil throw out my Dilbert cartoon?

MEMBER HINZE: I think your Dilbert cartoon has been deep sixed, Art.

(Laughter.)

MEMBER HINZE: I think we can all use our imagination and --

DR. MONTANA: Well, let me read it to you.

MEMBER HINZE: Well, all right. One minute.

DR. MONTANA: Well, all right. The two guys are sitting there, the pointy haired boss and Dilbert himself. And the pointy haired boss says, "We ship our new MP3 player in two days. How is the Elbonian factory coming along?"

Dilbert says, "The prototype is the size of a small tractor. And it will only play Elbonian folk music."

The pointy haired boss says, "I'll budget a little extra for marketing."

Dilbert says, "It's made of nuclear waste."

MEMBER HINZE: Okay.

DR. MONTANA: Actually, when Scott Adams wrote that, it didn't say that at all.

MEMBER HINZE: Okay. Thank you. Thank you very much, Art.

With that, I'm going to open up these last two talks, Bruce's and Art's, for discussion. Are there any questions from the Committee. Ruth?

QUESTIONS AND ROUND TABLE DISCUSSION

MEMBER WEINER: I have a question for Dr. Montana. And it's something that I wondered about throughout your talk. These waste containers are not empty. They're basically full of spent fuel arrays.

Does your analysis take that into account or would there be a difference in the tensile strength of the tangential tensile strength between an empty container or did you already consider that they're not empty?

DR. MONTANA: No, it doesn't matter. When you calculate the stress, it doesn't matter what's on the inside. When you calculate the internal pressure, you're just talking about an expansion of a certain volume of gas. And the expansion goes up by a factor of five. And an ideal calculation in the pressure goes up by a factor of five.

So no, it doesn't make any difference. I did use the total volume on the inside, calculating or at least trying to make guesses as to what the volume would be after it was filled; that is, the unfilled part of it, when I calculated the effect of adding water to the inside. But I calculated that even if there were two liters of water inside each of those canisters, it wouldn't affect the pressure that much.

So no, it doesn't make any difference whether it's empty or full as to the calculations that I discussed with you.

MEMBER WEINER: Thank you.

DR. MONTANA: It's a good question, but I think --

MEMBER HINZE: Thank you.

Other questions?

CHAIRMAN RYAN: No.

MEMBER HINZE: Okay. Eric Smistad?

DR. SMISTAD: Just I guess can we open it up to Bruce?

MEMBER HINZE: Yes, please. We need that.

DR. SMISTAD: This is probably a quick question. I was wondering, Bruce, if you have or are planning, you know, a paper or a report or something where we could get a view on some of the quantitative work you have done on the application side, not necessarily the buildup side but on the application side?

DR. MARSH: Yes, yes. I am working on a paper on this. It's nearly finished on some of these aspects, some of the critical aspects, really, of the flow in the modeling viscosities and the near surface features, those things. Yes.

DR. SMISTAD: Okay.

DR. MARSH: So that will be ready within weeks, I mean, several weeks probably.

DR. SMISTAD: Okay. Something we could get a hold of then in a couple of weeks?

DR. MARSH: Oh, yes, sure.

DR. SMISTAD: Okay.

DR. MARSH: Yes.

DR. SMISTAD: Thanks.

DR. MARSH: Neil is a co-author.

DR. SMISTAD: Okay. Just if I might, Bill?

MEMBER HINZE: Please, please.

DR. SMISTAD: Just maybe a couple of questions for Art. Art, this is Eric Smistad with the

Department of Energy.

DR. MONTANA: Yes?

DR. SMISTAD: I might have missed sort of the conclusion on the waste package. And maybe you didn't make one, but I was trying to determine what you felt the final fate of the package was in the look you did.

DR. MONTANA: What the final what was? I'm sorry.

DR. SMISTAD: The fate of the package. What did you determine?

DR. MONTANA: Go ahead.

DR. SMISTAD: Did you determine that the package was going to withstand the environment or did you --

DR. MONTANA: I don't think I'm in a position to be able to say. I don't think anyone is in a position to say given the information I had to work with. That's my point.

I don't find myself in a position to say whether the canisters will fail or whether they will survive. And that is the unsettling point.

I don't think other people have more information than I do on this. And so I find it curious that we're willing to go ahead and use this design that's presented to us with the uncertainties that I pointed out.

One of the last slides that I showed was the strength of the steel versus temperature of the alloys versus temperature. You can see that the internal pressures in that alloy, protective alloy shell, can build up to the point where it's very close to the ultimate tensile strength and certainly the strain and strength of the alloy itself.

So I don't know. Maybe someone else knows. But I don't know that someone else knows.

CHAIRMAN RYAN: Bill?

MEMBER HINZE: Mike?

DR. SMISTAD: Thank you, Art.

CHAIRMAN RYAN: Just a comment as long as you've opened it up to maybe ask a comment on Dr. Marsh's presentation. I found, Dr. Marsh, your presentation really compelling for a couple of reasons.

As basically somebody who is physics-based myself, I appreciate the fact you're taking analytical models and trying to explain the body of evidence with the analytical models. That's a pretty compelling case, as opposed to phenomenologic or observational, but it seems to me that you're really working hard to integrate all of those aspects into kind of a unified view.

Is that a fair summary on my part?

DR. MARSH: Yes, it is, Mike. And the other thing it does when you start doing this kind of work, you tend to realize what everybody has been saying. In other words, you have to get the geology right.

You have to recognize the problem correctly. It's hard to just isolate any aspect of the problem into a simple, simple exercise. And you have to realize what came before, et cetera, et cetera.

So it's not only setting up the physical problem and actually solving the equation, but it's actually honing them, melding them to the correct problem in hand.

I mean, often what we do is we set up a system for ourselves. And we believe it. And we go with it. And we say, "Well, this is what came out of it." However, the initial problem isn't actually well-conceived to begin with. So it's a give and take.

And so none of these are perfect fits. However, we would hope that with time, we would box these in more. And this is the issue, I think, that's at hand. One of the things that I find sometimes embarrassing in the Earth sciences is that if you actually show a standard problem set up to a physicist or a chemist, ten of them anonymously, they go through the same process and they solve and get more or less the same answer. You show ten geochemists or ten volcanologists. It isn't the same situation.

And that's primarily because our tools are different. And our anecdotal presence and our experiences are different. It doesn't mean they're wrong, but it means that once we start looking at these things and actually putting them together and boxing it in, the focus becomes clearer all the time. And then we understand each other, why we're coming.

So this is the approach, the approach I have. And I often feel frustrated just in saying, "Well, you know, I think this is this because it's this way somewhere else," et cetera. That may be true, and that may be helpful, but it's hard to make a final answer.

So, for example, at one point Nichols and Rutherford, I mean, it reminds me of the story of the Confederate army, Robert E. Lee saying up at Gettysburg when he finds the Union army is just five miles away in Longstreet. And he says, "Where did you get this information?"

He said, "I got it from one of my scouts."

He said, "Well, what is he?"

He said, "He's a Shakespearean actor."

And he said to him, "We move on the word of an actor?"

Well, we move on one data point. I mean, we really want to back this stuff up. It's very important. These are pivotal points. And we want to find out where the pivotal points are, where we're actually putting our cards. And I think that this is kind of the bottom line in these kinds of analyses.

Some of these things, you can move them around a lot. And you really don't get much different. But there are certain areas that are very critical.

CHAIRMAN RYAN: So maybe the other Mike Ryan and I aren't that far apart if we both started with physics. So that's good to know. But thanks for that clarification.

Thank you, Bill.

MEMBER HINZE: Thank you.

Further questions or comments from the Committee?

(No response.)

MEMBER HINZE: How about NRC? Are there comments, questions? I'll assume that --

MR. HILL: No, we don't have any questions.

MEMBER HINZE: Okay. Very good.

MR. HILL: We understood what was being discussed.

MEMBER HINZE: Okay. Chuck or Bill?

MR. MELSON: I just have a quick question, Bruce. You mentioned high-temperature glass. You know, we see, you know, all kinds of high-temperature glass, presumably in quenched pumices. You know, they should be high-temperature, cooled almost just by the expansion of the gas.

What is the property of these versus one which is a contact thermal quench? Can you actually after the fact do an X-ray analysis or other ways to spot the particular conditions that created a given glass?

DR. MARSH: You mean these glasses I showed here?

MR. MELSON: Any volcanic glass.

DR. MARSH: Okay. Well, these glasses here, the Webb and Dingwell, those are experimental laboratory, made, prepared glasses, held at high temperatures. And the rheology is done at high temperature. So they're a very controlled region.

Now, I don't know anybody who has done any glass except for the original stuff Herb Shaw did originally, you know, in his sphere experiments, really, high temperature in rhyolite glass solicit glasses. That's when his original viscosities were done. He did some experiments on those, actually. But I don't know any.

These quenched margins that I show in Antarctica, for example, as hard as those are quenched, those are all microcrystalline. You know, the nucleation rate in basalts is high enough to keep up. And there's actually no glass in that.

Now, lava flows, of course, they do quench to crystals and glass and stuff. So you can get all kinds of things back and forth. But in terms of natural rheology and high-temperature glasses and as a function of bubble contents and things; in other words, under real controlled conditions, we have a lot of inferences on how things flow, pyroclastic things and stuff, but these are really controlled experimental glasses, high-temperature glasses.

MEMBER HINZE: Chuck?

DR. CONNOR: Yes. I have a fairly general comment on both talks, actually. When Bruce presented his talk, I was struck by an observation people have made when they go to the San Rafael field and review the work that I and my students are doing. And that is, well, we have a snapshot of the eruptive process.

And when we're faced with the snapshot of the eruptive process, something which is incredibly complex and time-variant, we can't expect to see the full range of what has happened in these conduits as they have evolved and so on.

And so I appreciate the concern and the processed-based sorts of models that both the NRC and Bruce and others are pursuing, but, in fact, they're incredibly complicated.

For example, do we deal with homogenous nucleation? Do we deal with super saturations of 100 megapascales, which the scientific literature say may commonly occur in these systems and I believe would completely change some of the results that are presented, at least in the transient case?

In that context, I think that the work that Art Montana presented about the indeterminacy of whether these packages would fail under some circumstances is quite important.

So with these really complex problems in mind, I just want to make a comment about the draft white paper that we have been presented. And that is that if we go back to the probability, I do differ from Art. I do think the probability can be assessed. But, you know, nowhere in the white paper does it say that if we went 20 kilometers east of Yucca Mountain, the probability of volcanic activity by anyone's model, as far as I can tell, State of Nevada, NRC, independent researchers, that probability drops by about two orders of magnitude. In other words, we are out of the range of concern.

So, you know, a panel like the ACNW is a high-level panel. I wish you would think in your white paper of just bringing up basic points that, for example, it is a unique geologic situation. And if the site were only 20 kilometers away, the issue might actually vanish off the radar screen.

I don't see how you can put a paper like this forward without making that kind of comment, especially in light of the excellent presentations we have seen about the complexity of this issue.

MEMBER HINZE: Thanks very much, Chuck.

I believe Dr. Sparks, Steve Sparks, has a comment.

DR. SPARKS: Yes, just one or two points, Bruce. I was a little concerned about the cooling effect. I will just make a point that it rather looked to me like you were showing calculations of adiabatic expansion of the gas without heat transfer to the solid components.

And if you just do a pure adiabatic expansion of water at 1,000 degrees Centigrade to surface pressure conditions, you do indeed get cooling of two or three hundred degrees Centigrade, as you showed, but if you take account of the heat transfer between the gas and the ash -- and, of course, the gas is a small component in the magma -- you actually get much smaller figures. So I would suggest that that was looked at a little before coming to the conclusion that gas cooling was an important factor.

I would sort of also draw attention to some nice work that Kathy Cashman has just published in *Nature* for Mount St. Helens, which shows that the latent need of crystallization is really the dominant effect and that in the case of Mount St. Helens, the heating looks like it was sort of not far off 100 degrees Centigrade as the Mount St. Helens magma came up, notwithstanding having about 4 percent water.

The other point I would just make is that just going, pursuing the glass issue is that I think it's true that a melt at the temperatures of eruption, 1,000 degrees Centigrade or so, one would probably describe these as super-cooled liquidus, rather than glass. Dingwell's work shows the glass transition occurs at much lower temperature. And you showed some of his data.

So that unless the shear rates were huge, which they aren't, in at least the sort of lava component, you would expect that essentially if you would like these to be sort of behaving like melts, rather than sort of glasses, they would never get across the glass transition temperature. So those are sort of two sort of technical points about the --

DR. MARSH: I'll just say a couple of things. One, Steve, is that when you're bringing it up and you say the material will heat up, well, if we don't have latent heat and we bring it up isentropically, regardless of the gas, and you say the melt should heat up a little bit, where does the heat come from? Are you talking about crystallization effects of --

DR. SPARKS: Yes, crystallization specifically. You are, of course, quite right that if it didn't crystallize, you would get some modest amount of cooling.

DR. MARSH: Right. And then about the glass, once you get to the solidus, you get a glass. The formal glass transition temperature, of course, is much lower. But there still is a glass produced. And you can see this many times.

If you get to the solidus and it's not crystallized into crystals, it will form a glass. It will not be the formal representation. In fact, if you look at the derivative of the heat capacity, for example, that's how the glass transition temperature is determined. There's a bump in it.

But, actually, it's all a glass, all the way down to that point. That just happens to be the formal point, where you get to the atomistic definition of a glass. However, it is still, by all intents and purposes, a vitreous material before that point.

But in terms of the shearing and stuff, you saw there if we're talking about things coming up in these tight dikes and things like this, the strain rates could be very high. And you saw that curve. I didn't go into the right-hand side of it, of Dingwell's, but it becomes non-linear.

And so I think Steve's point is well-taken in terms of it's Newtonian, the lowest strain rates, but at the high strain rates, it becomes non-Newtonian, which means that the flow, instead of being a nice parabolic flow, it actually has a tight margin on the sides. And it has a plug flow almost to it. And it may actually get to the point where it starts brecciating on the sides and pushing itself out.

So it's a good point that the rheology is here, is strain-rate dependent, but that can be modeled. We can evaluate that. I think some folks recently in EOS or something like that had an article in that the brecciation in the pipes may actually -- because there's this whole problem of how you get this stuff out and with the degassing and things like this.

DR. SPARKS: I agree with everything you say there, Bruce. The issue is whether the strain rates are high enough.

DR. MARSH: Right, going into how wide the dike is.

DR. SPARKS: Golomon and Manger have published a paper in which they show that in rhyolites, which have viscosities, glass, if you like, the melts do become glasses because the shear rates are high and the viscosities are very high.

I am not aware of any sort of evidence of comparable processes occurring in basalt volcanoes or the ones they describe in rhyolite volcanoes. So I guess it's a question of whether you ever get shear rates sufficiently high, in 1,000-1,100 degrees, Centigrade in basaltic melts which would get you to the point where it would start behaving like a glass. I guess that's the point.

DR. MARSH: These things can be evaluated, really. When you look at these flows coming out, I mean, the microlyte distribution, the crystallization history, the CSD, crystal size distribution, for example, looking at them spatially could tell us about conduit process. It could tell us, really, about looking down the flow, across the flow. And it could tell us what that stuff was doing in the flows and seeing whether or not there were jumps in it and things like this.

We can get at these things. I mean, these are not things we just, you know, can assert. We actually can get some information on it.

MEMBER HINZE: Okay. John Kessler I believe had the next question.

DR. KESSLER: Thank you. John Kessler, EPRI.

I just wanted to make a point of clarification for Dr. Montana. The comments that he was making actually were all addressed in the EPRI 2005 report, which has been cited in the ACNW draft white paper.

Dr. Montana, we did assume under some conditions that indeed some of the waste packages would rupture under the assumptions that we made, making arguments and extending them beyond the 2004 work, pretty much as you said. We did take that into account.

Now, to a broader comment that gets to both something that Dr. Montana said and Dr. Connor just said, which is, you know, if only we did one particular thing, we could just take this problem right off the table. There are lots of if only things that could be done, not just one of them.

Certainly theoretically one could move the site somewhere else. What the purpose of the EPRI studies was was to say, "Maybe there are other ways we can address this problem."

We looked at it from a consequence standpoint. We did take into account quite a few different mechanisms of failure given a partially filled drift, where we had waste packages sitting in either the filled part, adjacent to the filled part, or farther down the drift, and then -- what is the word we are supposed to be using? -- use a shorthand version, which we call model abstraction to come up with a "So what?" in terms of doses for people downstream.

I think that before we say, "Oh, well, we could make life easy by just doing something," let's look and see what it is that we're trying to avoid and whether it's worth avoiding that by making some very large changes in programmatic direction, rather than maybe simply working a little hard to sharpen our pencils and seeing if there are other ways we can put this risk in perspective.

Thanks.

MEMBER HINZE: Thanks very much, John.

Dr. Andrew Woods?

DR. WOODS: Thank you. Andrew Woods from Cambridge University.

I would like to turn to Bruce Marsh's talk and just raise another technical point concerning the use of modeling. Ron mentioned the use of simplified modeling. You develop models or apply models of lava flows spreading over the ground to infer values on the viscosity.

And I think I would just like to make the comment that as these flows evolve from deep in the subsurface up to the surface and then flow on the surface there, the flows are evolving. There's transient chemical changes in the flows, which lead to changes in the radiology.

One of the I guess challenges about the inferring viscosities of the subsurface from the surface can perhaps be put into focus by -- you showed some pictures of very narrow dikes in Iceland, you know, working in the optics area. You showed some pictures of dikes, which are very narrow. And the magma that is feeding these lava flows on the surface is flowing through these very narrow dikes.

The viscous pressure losses associated with moving magma up very narrow dikes over several hundred meters will be substantial. And are they consistent with the buoyancy of different forces that you proposed will actually drive the magma up through those dikes?

DR. MARSH: Which dikes are you talking about, Andrew?

DR. WOODS: Sorry.

DR. MARSH: The Antarctica stuff or --

DR. WOODS: We just think of magma rising up through the crust up to the surface. It's rising through some flow path through some opening in the ground. There will be viscous pressure losses associated with that flow.

Have you considered how magmas with the sort of viscosities you are proposing would actually rise through that? What aperture size would you require in order to get the flow rates consistent with these lava flows spreading in 20 days with these volumes?

DR. MARSH: It's actually quite interesting. I mean, you could do the calculations I think a little bit better than when I showed. For example, if we actually took that flux of material on the surface and said, "Let's just ignore the rheology on the surface" and said, "I need to have a dike or a conduit or something to actually deliver that material," you can have a very generalized model and you need some sort of driving pressure and we have a viscosity.

Well, the flux would be some sort of delta P/DPDL. It's a very important factor, of course. We have an aperture to the fourth power that's a function of time, but we'll just take it to some characteristic length. And we have some viscosity in the denominator.

So you can trade off back and forth these things. And it turns out if you want to get like .03 cubic kilometers and things, it's pretty easy to supply that almost with relatively modest pressure rates.

Now, viscosities that are actually not this high, I mean, Steve's number of 10^5 pascale seconds, that's 10^6 cgs I'm talking about. So we're not that, actually, too far apart. One of the numbers I used was 10^7 in some of these things.

The problem with actually going to these dikes and things afterwards is that you don't know if that was actually the active thing or not. Sills are one thing, but, as you know yourself, you guys use over-pressuring. And when the system is done, it goes back down. So when we look around the Earth and see little, tiny dikes, you wonder what was supplying them.

So we go to Antarctica, for example. We look in these dikes that supplied flood basalts on the surface. We actually can walk up through some of these things. The damn things are, you know, as wide as your desk there now. So we really can't use that now as information. It might have been, you know, meters wide.

We also see this in the sills. The sills themselves are full of entrained crystals. Some of them have 50 percent entrained slurries. They could move like that.

What happened is they were inflated. And as the repose time started in, the system then went down. It pushed out the end of it, some liquid out the end of it. And it ends up the sills are actually deflating down.

So it's kind of like Chuck says. We're looking at the aftermath of these things. And our insight into through the geology. We have to read the dynamics. So that's the hard problem.

DR. WOODS: Thank you for that reply. I think it would be useful to actually see some of the calculations of what the inferred dike widths would need to be in the subsurface.

You presented a calculation in your talk showing magma moving down a repository drift with viscosities of 10^8, 10^9 pascale seconds. There were no calculations for lower viscosities.

You just mentioned there that perhaps you need viscosities more like Steve was saying to have the magma descend in the dikes. And it would just be interesting to see in the spirit of having these simplified models of the process models, see some of the processes you are envisioning in that shallow subsurface as well as on the surface so there's a sort of coherent, integrated framework.

DR. MARSH: Yes. I mean, those plots I gave I just showed you. We can expand those plots, everything. You can have anything you want on there. In fact, there are a lot of other curves on there that I didn't talk about.

So they're not unique. I mean, once you see those and you say, "Well, I'd like to have my own values," fine. You can put your own values in. It's really --

MEMBER HINZE: Bruce, I am going to --

DR. MARSH: We're cutting into the lunch period?

MEMBER HINZE: We're cutting into lunch. I would like to give the DOE representative, Greg Valentine, a chance to ask a brief question and a brief answer. And we will have time at the end of the day here to come back to this most interesting subject. Greg, please?

DR. VALENTINE: Okay. Maybe this is more of a comment than a question, but I do want to point out that we have published a couple of papers in the open literature recently that go into some detail describing the features of the lava flows in the Quaternary Volcanoes.

And these flow fields are compound flow fields that are a combination of stacked flow units, components of channelized flow. There are components of breakout along the margins from internal flow. And they're not radially spreading viscous fluids under gravity, as you have modeled.

So I completely agree that we need to be using physics-based models and that we need to craft the problem well, but we also need to be doing things that are consistent with the fundamental observations in the field.

CHAIRMAN RYAN: It would be real helpful if you could maybe give us the detailed citations, particularly for those that --

MEMBER HINZE: We have them.

CHAIRMAN RYAN: We have them all?

MEMBER HINZE: We have them. We have them.

CHAIRMAN RYAN: Okay. Good. Thank you.

MEMBER HINZE: No problem there. With that, we will try to have time for further discussion later this afternoon at the end of the presentations. We'll adjourn now. Mike, I'll pass it back to you, but let's start again at 1:00 o'clock.

CHAIRMAN RYAN: All right. And with that, we'll conclude the morning session. We will start promptly at 1:00 o'clock. Thank you.

(Whereupon, a luncheon recess was taken at 12:09 p.m.)

A-F-T-E-R-N-O-O-N S-E-S-S-I-O-N

(1:04 p.m.)

MEMBER HINZE: Before we get started this afternoon, I want to ask everyone to try to stick to the schedule as much as possible or maybe even give us a few extra moments if at all possible because we do have a tight time schedule.

Also, if any of you are concerned about flights, whether they are flying and whether they are available, if you would give that information on your flights to Michelle, who is sitting at the desk here to my right, I understand that she will be happy to look into it for you and get back to you. I think that is a splendid service and we do appreciate it.

CHAIRMAN RYAN: Thank you, Michelle.

MEMBER HINZE: And when we -- and now that it is 11:30 -- or, pardon me, 1:00, we will have Tim McCartin to give us kind of a view on the whole problem of alternative views in performance assessment because we are hearing alternate views.

MR. McCARTIN: Yes, thank you. I can talk fast but I can't fast enough to get us to 11:30.

In providing some perspectives on the use of alternative models, I will say I always look at an ACNW presentation as a way to look at ourselves and are there areas where we can improve either in our communication or in the analyses we are doing.

And in thinking through some of that in preparing the talk, I'm comforted by, I think, the discussions, over the last two days, point to some areas where I think we can improve and you will hear that in the talk today.

In terms of the regulatory requirements, there are really two pertinent aspects of the regulations. One, the consideration of alternative models in accounting for uncertainties. And the regulation has these as two separate items. And there is a reason for that.

And I think if I go to the lower one, the accounting for uncertainties, we are really looking for items in the performance assessment where there is a variability that we are accounting for in parameters.

And there's things in the analysis of the igneous event where the number of waste packages is included in an extrusive event depending on a particular scenario, but that is something that there is some variability there. And that's really significantly different from how we would treat alternative models.

And some of that discussion we already had with respect what do you do when you have alternative models that are really separate assumptions? And the regulation specifically says you need to consider alternative models. There is flexibility provided to the Department on how they want to consider it.

We heard talk of Shlomo Newman's methodology in the hydrology area for combining alternative models with weighting parameters. Clearly that is something that can be done. But I think we, at least at the NRC and here's where possibly in discussions with the Committee in previous talks, we haven't been as clear as we could have been.

And with respect to the alternative models, we believe in terms of understanding and presenting our understanding of the problem that we would prefer at the onset of the analysis to not try to lump alternative models together in a performance assessment calculation and do some grand sampling where you are mingling all these alternative models.

We would prefer to keep the alternative models separate and do a performance assessment where we certainly would analyze the uncertainty in the parameters associated with a particular conceptual model but we would not mix multiple models together and keep them separate.

At some point, one might look at these different models and if one wanted to assign weights to them and come up with a single number, that is appropriate. And that part -- I guess that's part of the problem. We have not really been presenting that information to the Committee.

And I think there's -- and I'll maintain in the white paper there was discussion of our point value is -- we're not being risk informed. And in that sense, I will partially agree with the Committee in that if all we were doing is looking at one point, absolutely correct. We are not being risk informed.

But these other alternative models that might lead you to other probability estimates or other parts of the problem, I think we need to present that information to the Committee so they can see well what do the different alternative models lead you to in terms of dose calculations?

And I think that is a valuable piece of this analysis that we should continue. If someone want to equally weight all the models and come up with a single one, you can.

But we prefer to keep them separate and that's why we have sort of focused on a single number primarily because the use of a single number, if it is a mean of a distribution -- and given I have, as Dr. Connors pointed out, I have uncertainty in my analysis that I may get to.

I have a range for one conceptual model and that range of probabilities supported by the conceptual model will have a mean. We've pointed to the mean primarily because because it is so linear that yes, we can sample it. But the net effect on dose will be the same as if we used the mean.

And in terms of looking at different conceptual models for the event probability, clearly there has to be, you know there can be a range of uncertainty for a particular conceptual model. And that range needs to be defended.

But if I have the mean value for each of those different models, I have a very good sense of its effect on the dose. And that's why we've focused, I think, primarily in talking about the mean.

But I think unfairly we haven't really been communicating. And I think we can provide a calculation where we have the different probabilities and one can see the different conceptual models what the effect on the final dose is.

Likewise, there's other things -- magma-repository interactions, damage to the waste packages, there are things there that are different conceptual models. Once again, there I think we have not presented to the Committee the range of these conceptual models.

And I think our code has the ability to do this. We should be presenting that information. For example, I'll give you, you know, is the waste package damaged? There may be some views that the waste package is not damaged versus another view that the waste package, all the contents are available. It is completely damaged. Those are different conceptual models.

I really would not want to sample those in my performance assessment and basically get half the waste package contents are available. I don't think that's useful. There may be other ideas in terms of well, it's not completely damaged. Maybe it's partially. Whatever. Whatever the different conceptual models are, I think we have the ability with our performance assessment to present those results and you can have a sense of well how does this effect the overall dose component.

And I think that information of risk information with respect to dealing with alternative conceptual models would be useful for us to do those analyses, talk through them, and see that -- in a way to get a better sense of how risk significant some of these are.

And I think that, in a nutshell, is -- I will say the intent of the regulation was to keep alternative models and the uncertainties you might have with that separate from uncertainties in sort of parameters that are looking at variability of a particular -- be it a retardation factor or some other aspects of variability in nature.

And I think that is the part that I was thinking we should be able to do this. And we haven't been. And I think that could be part of the reason for some of the views in the white paper with respect to not being risk informed. And clearly we, at the staff level, oh, yes we are. But we need to present some of this information. And I think it will be helpful.

CHAIRMAN RYAN: Tim, I think that's got to be a great step forward. And I think the second part, as we touched on it a little bit earlier with Britt and John Trapp, if that then gets woven together with well, why, you know, what is your framework for decision-making as a regulatory decision-making process? If you weave all that together, I think you would go a long way to being very transparent.

MR. McCARTIN: Exactly.

CHAIRMAN RYAN: You know, which is a great goal for all of us, you know. So three cheers. I mean I think if we can move forward, we'll get a better understanding as will everybody else in the process. And your views would be a lot clearer.

So, yahoo.

MR. McCARTIN: Yes. Exactly. I mean in the discussions today, if there is one thing I take away from it is resolving with certitude the differing opinions is maybe an impossible job.

But in terms of presenting the different views and their bases and what the impact is, is a very achievable one. And from that point, we can review a DOE license application.

We can present our understanding clearly. And other people can come in and decide whether gee, I still, at the end of the day, I'm in full support of the John Garrick vision. Take your best shot.

CHAIRMAN RYAN: Yes.

MR. McCARTIN: This is my view. And I don't think there is any problem with that as long as one displays here are other views. And what it means.

And everyone will have a different view. But when you look at it, you'll have a clear understanding of why someone arrived at a particular decision.

CHAIRMAN RYAN: That has been the push of our letters.

MR. McCARTIN: Yes. It took us a while to figure that out.

With respect to our TPA approach, I feel that the way we've developed our TPA model was to have an ability to accommodate a lot of different views. And so with respect to alternative models, in terms of damage to waste packages, can we represent gee, packages are damaged but not completely?

We have a way to deal with that with a parameter to get a sense of an alternative model that would have partial damage of the waste package. Likewise, secondary break-out, what if there is -- that's really a different view in my opinion of the way the igneous event, it doesn't go straight up, it breaks. We have a way to look at that particular alternative model.

Variation in probability, we do do this as a post processor. But it is, once again, a parameter. We can display, even though it is linear, it still, I think, would be useful to see well, the different views of probability, where do they sit with respect to each other?

And I think, like I said, I mean we haven't dealt with alternative models as directly as we can in presentations. And I think that is one that I think, for this particular problem where there are strongly-held different views, everyone will benefit by seeing a clear depiction of where this sits with respect to the overall estimates.

Certainly there is uncertainties that we do with any particular alternative model. As I said, there is variation in the conduit diameter, you know. You have a model that says a conduit comes up through the repository. Well, there is variation in the conduit. We vary that.

That is parameter mass loading, a parameter of what the dust levels are, there are things that we will always be sampling for a given conceptual model. It may vary from conceptual model to conceptual model.

But I think it would be -- I believe it would be very useful for us, the staff, to go through the calculation of looking at these different models and different ways of displaying it. And provide some risk perspective for them.

In terms of the considerations, when we do the analysis, I will say -- and here's where I think we've been talking past ourselves in some respect -- I know with respect to the event probability, when we use the mean, clearly the mean has to be supported by some range of uncertainty for a particular model.

We do use the mean just because it is a linear effect. That if we sampled it, we'd end up with the same average dose curve as if we just used the mean probability for that conceptual model. And so we haven't really sampled it.

But that's not because we aren't interested in the range of uncertainty that is supporting a particular conceptual model, which does effect what the mean value will be.

The number of packages, the same way. It is a very linear effect. In some of our analyses we may assume five waste packages rather than sampling one to ten. We know we are getting a -- the results are the same.

So we do -- because of computational demands in running the computer, we do take advantage of things. But they are in areas where it is very specific that we realize that sampling that parameter would not provide anything different with respect to that overall dose curve.

There are other things that we do know effect the doses. And I will say I had something in one of my slides and I must have printed out the wrong version. First and foremost is the timing of the igneous event. That is a very important aspect.

And we do a lot of analyses early on in the first few thousand years because that really is when the short-lived nuclides are present that have an ability to get out through an extrusive event. That's the only way they are leaving the repository horizon.

The number of waste packages, damage to the waste package, entrainment of fuel, mass loading are all things that have large effects that we think -- we certainly sample some of those. Some of those can be different alternative models, et cetera.

But I think we'd like to go back, think about the problem a little bit more. And think of how best to display different conceptual models. And I think that is a piece of information that I think the -- I will say that is the common theme of the ACNW letters over the last two years.

Well, you hit us on the head long enough, we eventually pick up on it that in terms this is kind of the risk information that we haven't been presenting. We felt we were, I'll say. But we really weren't.

And I think we can present that kind of information and I think it will be useful for everyone in the very difficult area of alternative conceptual models, which it isn't just in igneous. It probably is -- it may be more present in igneous than other areas but there is a potential for alternative models in other areas.

And with that, just closing with I think quantitative analysis of the significance of the alternative models I think would be very helpful. It would assist dialogue among the different groups.

Clearly I think it would help our discussions with the ACNW. It certainly supports the review of a potential license application. And once again, understanding we aren't trying to.

And I take Dr. Melson's comment earlier today that we look like a proponent -- I know Britt also talked very seriously. We are not a proponent. We should not be. And it is just what are the range of views, look at the information supporting it, and make a decision. But everyone should understand the range of things being considered. And I think in terms of our license application, we want to be looking at all the range of views.

It helps both two areas of our review: requests for additional information that we might ask of the Department and also there is the performance confirmation program. Are there things that we believe are uncertain enough that we think this is a good avenue for the performance confirmation program?

So there is a lot of benefit for this. And with that, I'll stop. And be happy to answer any questions.

MEMBER HINZE: Thank you very much, Tim. Very heartening.

Mike?

CHAIRMAN RYAN: Again, Tim, I think that is terrific. You know you've kind of outlined a program that really couples to me with what we talked about earlier. That there is a regulator kind of aspect to all of that.

And I think documenting those kinds of things carefully and thoroughly now has an added benefit that if there is any challenge to a regulatory decision or decisions, you know you're not scrambling to say well, you know, what were we thinking three or four years ago? Or last week? Or the month before?

It's kind of all laid out there as you are going along. And it really, I think, gets you to the point where you are very transparent all the way along which would be great.

So to me the kind of two ideas tie together pretty well, which I appreciate.

MEMBER HINZE: I was just asking Ruth to move her microphone down.

CHAIRMAN RYAN: Oh, okay. Well, I thought you were waving at me. I didn't know what was going on.

(Laughter.)

MEMBER HINZE: That will never, never happen.

CHAIRMAN RYAN: But boy, that sounds terrific. And it really will help us understand the details of your thinking as we are going along. That should be great. Thank you very much.

MEMBER HINZE: Ruth, you had a comment?

MEMBER WEINER: I had several. And, again, I want to commend you on this presentation, Tim, because if you do include alternative views, alternative models, it gets you out of this argument that this one is right and that one is right. And no, I don't think you are right. I think X is right.

And it also would give everyone, including the stakeholders a comparison, and, I think, a better feeling for performance assessment. Performance assessment is this black box that no one quite understands. And I believe that this would help a great deal with the public understanding of performance assessment.

You mentioned a number of areas on your slide four where you would include alternatives. And I wonder if those were just examples and those were not intended to be totally inclusive. Because I was going to say particle size is a question.

MR. McCARTIN: Sure.

MEMBER WEINER: Weather is a question. Water flow is a -- there are a lot of areas there that aren't included in this list. And I'm sure you didn't mean it to be.

MR. McCARTIN: Yes. I should have put for example on there. But yes, it wasn't meant to be a comprehensive list.

MEMBER HINZE: Dr. Clarke?

MEMBER CLARKE: Just a quick one if I could, Bill.

You mentioned the TPA, Tim. Where are you with the TPA?

MR. McCARTIN: Well, the TPA is always a

developing process. And in that we continue to improve it as necessary.

We are in the middle of making some revisions to the code to accommodate the remobilization of ash. There are other changes with respect to colloids, et cetera.

That work is ongoing. We hope to be done in the near future, certainly some time this year. However there are -- I mean we have our previous version of the code, the TPA-41J code, that is running and available. And we use that to the best of our -- you know, to solve the problems.

MEMBER CLARKE: I just asked. We haven't heard about it for a while. And I just wondered.

PARTICIPANT: We'll be here when you are ready to talk about the new one.

MR. McCARTIN: Right, okay. We'll take that as a to-do, yes.

MEMBER CLARKE: That's fine, Tim, thank you.

MEMBER HINZE: These are all good comments. And certainly your presentation, as I say, very heartening. One of my tentative conclusions regarding the white paper was that we didn't really know what was important because we really don't know whether some of these differences we seem to be discussing are really significant to the risk.

And I think you are interested in capturing that. And that is heartening

And it is also very important for us, and I think for everyone, to know whether you are coming in with bounding conditions or whether you are coming in with mean conditions. And that sometimes has -- maybe it has been clear to you but it certainly hasn't been clear to the listeners or at least to me.

With that, are there any other questions? Who has a question? Who is this? Leon?

MR. McCARTIN: Now, Bill, I will say I mean we do have our Risk Insights Baseline Report which does capture some risk. But I think the harder problem of quantifying some of the alternative models, et cetera, we have not.

And we all have our opinions in terms of where is the 800-pound gorilla. And I think that is a way to try to ferret this out.

MEMBER HINZE: And also, that's a couple of years old. And one of the things that I wanted to ask in the conversation yesterday morning was in any way has your risk insights changed as the result of that. And I don't want you to get into that now. But that is something that we are not privy to. And it is very hard for us to really prepare this report in what I consider to be a very definitive way or very helpful way to the Commission without having some of those insights.

And we can't do that. We don't have the TPA. We don't have -- we haven't learned about the TPA as Dr. Clarke has just alluded to.

Well, with that, Leon, is this directed at this topic?

DR. REITER: Yes.

MEMBER HINZE: Okay.

DR. REITER: This is for Tim. But I can hold it back.

MEMBER HINZE: No, please, please.

DR. REITER: Leon Reiter, Consultant, Technical Review Board, Tim, and I'll stand corrected by somebody from DOE, the TSPAs that have been looked at by DOE, there are generally two approaches to dealing with model conceptual uncertainty.

For most of it, the people will look at -- do analysis, look at the various models, pick the model that fits the data best. And I guess if all the models don't fit the data equally well, they might pick the most consequential to them.

However this is not the way it is done in PSHA and the PVHA, Probabilistic Seismic, Probabilistic Volcanic Hazard. There the experts weight the models. I'm not quite sure -- is that -- are you saying that is an inappropriate way to go? Or it has to be accompanied by something? I'm not quite sure what --

MR. McCARTIN: No, I didn't say it was inappropriate. The key is making it transparent. And I think as long as the process shows the different conceptual models and the weight and the basis for assigning of weight of X to these different models, it is easily reviewable. And that is the part.

And my only point with if someone gave me the grandiose average dose curve where all the conceptual models were folded in with weighted values and I had one curve, it is very difficult for me to review that. I need to go back and understand what were the different alternative models considered? What was their impact? What were their weights?

And that is where I think we are with using these, you know, different values. We want to keep them separate. And then you can understand how they are all folded together.

But it certainly is acceptable if one

believes they want to weight all the different conceptual models and come up with a single number, that's fine.

DR. REITER: That's okay?

MR. McCARTIN: But the key is doing it transparently so that you understand how you got there. And one can question possibly a basis for either a weighting of that conceptual model or whatever.

DR. REITER: In the context of PVHA, you would go into the rationale as presented by the individual experts and if you thought the rationale was inappropriate, then you might comment on that? Is that correct?

MR. McCARTIN: Yes, yes.

MEMBER HINZE: Well, thank you very much. Unless there are pressing questions, we'll move on. And thanks for helping us stick on schedule, Tim. Always very helpful.

With that we move back to the consequence issue. And more particularly the remobilization issue of the volcanic ash. Neil Coleman from the ACNW staff will make a brief -- did I emphasize that -- a brief flooding topic -- well, it must be another movie -- a brief discussion of flooding history and the geomorphology and its importance on risk.

DR. COLEMAN: Okay, am I coming through on the mike? If you hear me wincing it is because I walked a half a mile in this slush and one of my feet took a wrong turn.

We will move from extremely hot fluids to ambient temperature fluids. And I've been asked to go a warp speed.

Next slide please. I'll just say the purpose of this is to talk about how Fortymile Wash is important to the extrusion scenario.

Next slide. I just wanted to add the caveat that we are evaluating a purely hypothetical scenario. Key observation, the footprint, as previously defined by DOE, has never been penetrated by basalts so far as we know despite extensive site characterization. One came very close. But it is a key observation in 13 million years, we have no evidence that it happened.

Next slide. Why is it important? If a volcanic vent were to intersect a waste tunnel, expelled materials could contain high-level waste contamination.

These would be deposited on the surrounding hills and plains, in the adjacent drainage basins, and there are several, subsequent erosion and fluvial transport would carry some of the contaminated ash towards the RMEI, the reasonably, maximally exposed individual.

And this is a change from the previous NRC performance assessment code which was strictly an atmospheric plume deposition. Here is a more sophisticated and a more realistic treatment.

Next slide. You can read this. It is talking about the characteristics of the Wash.

Next slide. I just wanted to point out the location of the Wash for those that aren't that familiar with it. This is a very nice satellite view of it. Yucca Mountain at top center. Lathrop Wells cone is here.

The approximate location of the RMEI in the vicinity of perhaps somewhere between Amargosa Valley and this fan of material. Fortymile Wash has three basic segments. A northern very extended area that is eroded into bedrock. The section that you see here which is eroded in an alluvial plain. And then it breaks out -- it is no longer incised down here and forms a distributary fan.

One other thing. Lathrop Wells cone, just to put it in perspective, this is the only volcano that has erupted nearby since modern humans have walked the Earth.

Next slide. Just one thing I wanted to note, oh, I'm sorry, you can't read all of that. What it says at the top -- the far western channel, this one, appears to be the most active. One reason for this may be that there appears to be a gentle westward tilt of the whole fan which suggests an interesting possibility that not all of the fan is necessarily available for deposition in the spread out of water.

And, in fact, even the road plays a role. In the 1995 flood that went across the road, water actually ponded behind the road berm and then created a new channel heading south. Man-made structures play a role.

There is more information available on this fan. A scientific notebook exists at the Center for Nuclear Waste Regulatory Analyses. We were hoping to include a summary of some of the information in that but if it is made publicly available in time, we will do that.

Next slide. Here is the whole drainage basin. One of the things about this is you can have a storm taking place up here and no evidence of precipitation down here. And get into fairly hazardous conditions if someone is trying to cross the wash as actually did happen to someone. And I've been there when water was flowing in the wash with no precipitation in the area.

This figure I have borrowed from Don Hooper, who I believe is here today, but this report by Don, 2005 report, a very good, well-written report that describes most of the things you would want to know about the Fortymile Wash system.

Next slide. I want to throw this slide in. This is an inferred fall out of Lathrop Wells volcanic ash reconstructed by USGS and other DOE contractors from very incomplete preservation of ash giving some idea of what an ash distribution might be like if you were to say superimpose this on Yucca Mountain.

Next slide. Am I going fast enough?

MEMBER HINZE: Yes.

DR. REITER: Additional considerations. The volcanic ejecta consists of tephra of varying sizes and also lava flows. The largest tephra will accumulate near the vent to form a scoria cone.

Any high-level waste debris that becomes entrained in scoria cones or lava flows would not contribute to dose because these are permanent features of the landscape. They erode over millions of years.

The NRC TPA approach, in a way, does consider deposition in a scoria cone because they allow for very large fragments up to ten centimeters to incorporate waste. But it does not, so far as I know, and Tim is here and he can correct me if I'm wrong -- it does not consider incorporation in the lava.

And as Professor Sparks pointed out a very interesting point about the fact that you can have this combination of fairly quiescent lava flows along with the pyroclastic or Strombolian phase, it makes sense to consider a fraction of any extruded waste becoming entrained in lava flows.

It is something to consider. It would be a significant amount.

Ash deposited outside the Fortymile Wash drainage would not contribute much dose, if any, to the RMEI.

Next slide. And here you can see that picturing the -- here we are looking right down the drainage divide, the crust of Yucca Mountain Mountain. If an eruption were to occur, ash that would be tephra over here would become part of the Solitario Canyon drainage. On this side, part of Fortymile Wash.

Next slide. Another slide I have borrowed from Don Hooper. It is not from his report. It is from a presentation he gave in Buffalo, New York, showing hypothetical plumes, depending on, I guess, wind directions, the average wind directions at the time of the eruptions.

And I'm sorry, I don't see colors very well but the drainage basin of Fortymile Wash is outlined here. And this shows in this scenario almost no ash would be deposited in the drainage basin. Here, on the other hand, much of the drainage basin would receive a veneer of tephra.

Next slide. Tim, in his talk, mentioned this slide. And I really just wanted to throw it in. This shows what he was talking about, that the first thousand hears or the first fifteen hundred years, you would see -- if the eruption through the repository occurred then, you would have a peak dose occurring at that point because your shorter-lived radionuclides -- and americium 241 is a particular example. You would have two half lives decayed in that time.

Next slide. Just a couple of views from down in the wash. You can see this high wall created by erosion of large floods in the past.

One thing, just a little side note, packrat middens have been proposed for use as paleo stage recorders because flood waters completely destroy them, tear them apart. They are highly hydroscopic and there are many of them in the upper parts of the wash.

Here is a close-up view. This is one of the areas of deepest incision at the site. It is about a 20-meter high wall. There is a fair bit of integrity in it. It's not a loose material that is exposed along here.

Next slide. I've assessed the hydraulic gradient along this system. The blue -- that is blue, right? And yellow? Okay. I should have checked on that. The blue is the topography along the eastern bank of the wash. Yellow is the thalwag, which is the line of lowest elevation along the channel.

And you can actually see here how it is more deeply incised to the north until it grades into the fan to the south.

Hydraulic gradient, .011. This is significant. This is not a lazy eastern stream. This is capable of producing quite powerful floods.

Next slide. The only thing I'll say about this. This is tabulated by Don Hooper from several other reports. I've added indications of -- these were floods that traveled all the way across the Amargosa Desert. These were floods in Fortymile Wash, traveled all the way to the Amargosa River, and contributed to the flows in it.

No one observed that happen in 1969. Or no one documented it. It almost certainly happened in that year though. I would also say the 1995 March flood, a near tragedy was averted because a worker at the site attempted to cross the wash and was swept away. Did survive, was treated for hypothermia.

He described the roar as the water came down. An almost two meter high wall of water came down the wash. He was a very fortunate person to survive that.

Next slide. You can read this. This is the tabulations of peak flood discharge in cubic meters per second for the 100-year flood, 500-year flood, regional maximum flood by Squires and Young.

If Don is still here, there is something he should be aware of. I attempted to reconstruct the 1969 event using everything I could find. And have come to the conclusion that that flood, big as it is, in excess of the 100-year flood, was probably underestimated.

The reason being, these kinds of floods excavate transient channels. That is the base of the water, the base of the channel at the peak of the flood is actually deeper than what you would have measured had you surveyed it before because it has scoured out greater depth.

That means the flow depth is underestimated meaning that the total discharge is underestimated. Also the power of the flood is underestimated that way. Maybe by as much as 50 percent.

Oh, there is Don back there. You are sitting behind such a tall guy I almost didn't see you.

But this may be the case as well for these floods of large magnitude that have been documented. I find no evidence that they considered the scour depth. And the 1995 flood that swept away the worker removed one of the scour chains from -- these are devices used to look at how deep a flood rips out material.

One was removed or torn out at the Narrows, which is one of the monitoring stations. Meaning -- and it was anchored at a depth of about four feet -- a possible indication that the depth of scour at the peak of the flood.

Next slide. I've already talked about -- let's go ahead. There is a quote from Tanko and Glancy. They had actually thought at one time that in the present climate, you would not have floods reach the Amargosa River from Fortymile Wash. And here they saw it twice in the 90s. And they also speculated -- and it is a very good speculation -- that it happened in `69 as well.

Next slide. Sediment transport. The largest floods in this kind of system will completely dominate sediment transport. You have a stepwise transport with these where the small floods, like the one I witnessed out there, transport sediments but due to transmission losses, infiltration losses, the flood stops. Sediments are deposited right there.

These are then available to be flushed out by the really big events that come through like 1969. The smallest sediments, less than 62 microns, that's the clay and silt-sized materials, these have the potential to remain suspended and travel the greatest distance.

These are also the ones of greatest concern in health physics from -- you know, the dust that is carried in the wind and inhaled. In fact 62 microns is at the upper range of any concern for that. But this is the stuff that will go the farthest.

There are also substantial reservoirs of available sediments. One I'll mention here -- this is Busted Butte. This is Busted Butte Wash. This apron of material, these are sand ramps. And I've walked on these over in this area. Extremely loose material, lightly vegetated, collection of aeolian material.

And this is one example of the sources of sediments that are even available right here and not referring to the upper parts of the drainage system.

But I found a great source of these aerial photographs for the site. This allows you to go down and actually look at the rocks in the channel. Fantastic resource.

Next slide. The dose significance of large floods, smallest sediments have the greatest potential to be remobilized by wind, contaminated sediments less than ten microns are the bigger concern for inhalation doses.

And as Keith Eckerman has pointed out, particles at one micron are deposited in the alveoli, that is the deepest penetration of these particles, twice as effectively as five-micron particles. So this is not a linear, you don't project that linearly.

I mean you reach a point at 62 microns, at that level you are inhaling sand.

Next slide. A slide from Don Hooper's 2005 report. And this one shows the watershed and a fairly realistic depiction of the depositional basis. And here is the fan where it is indicated that deposition begins. Here you see the Amargosa River coming down from Beatty. And it continues on south, does a left hook, and goes into Death Valley.

Next slide. Here is the representation that was presented to the Committee. And we had not asked for the staff to give a presentation at this meeting because they gave two very good presentations last year on different aspects of this.

But one concern that arose, the simplification that is used in this redistribution model is this is considered the area of the active fan. And no material -- just as a simplification, no material ever leaves it to the south despite the evidence that we have of these extensive floods that do occasionally happen.

Next slide. So some actual pictures of what these really large floods look like.

Go on to the next slide. Oh, actually go back one second. I did want to mention that 1969 regional flood, the one that I'm proposing may have been underestimated produced a lake of 50,000 acres in Death Valley. The 1998 flood also generated a small lake there.

I'm not saying that this is all flows from Fortymile Wash. This is primarily from the Amargosa System. There are also contributions from the Mojave River to the south that also feed in there. But these were two of the years when there was long distance transport of water and sediment.

Next slide. Here is a typical view at Badwater. For those of you that have been at the lowest point, the lowest point in the U.S., Salt Flats, this was in the spring of 2006. Abe van Luik provided -- well, I saw a presentation that he gave with these. And I asked for them. He said they are on his website. A very interesting website and one worth visiting.

Next slide. Here was a photo that Abe took during the winter of 2005, in February showing the Amargosa River in flood. This is approaching Death Valley. You can see the steep sides of this rift basin and the water flowing in. And it was one of the most incredible years for flowers in anyone's recollection out there.

Next slide. And here was the lake that was created at that time, March 6th, 2005. Large lake formed at Badwater. It sort of makes -- the first time I saw this, it made me want to take up windsurfing.

Next slide. Now here is less than ten days later, already beginning to recede, exposing the salt flats. But it is the same view as in the last one.

Next slide. Just wanted to add a few notes here because while I was looking at this whole business of thinking about flow dynamics and sediment transport in the system, I thought well what do we have -- what is there available on fuel, uranium dioxide fuel, spent fuel from reactors, which is the principle fuel that would be in a high-level waste repository.

Next slide.

MEMBER HINZE: Well, need to cut this out.

DR. COLEMAN: We're nearly done.

MEMBER HINZE: Okay, great.

DR. COLEMAN: TPA-3.0, the staff used this triangular distribution ranging from 100 to 10,000 microns, which is almost all sand-sized material and some gravel. But for TPA-4.0, this was changed to one to 100 microns, which is reducing the fuel to an extraordinarily fine powder, all of the fuel.

In TPA-3.0 -- okay -- Dick Codell, I wanted to mention he has a paper that we cited but we didn't say a whole lot about it in the white paper. He modeled an intermediate size range from .001 to .1 centimeters. The staff, when they made this change from 3.0 to TPA-4.0, citing NUREG, which was actually very difficult to get a hold of, 1320, however the crushing experiments that are described on irradiated U02 fuel produced only a small fraction of fine grain material. The impact energy density was up to 77 joules per cubic centimeter.

Next slide. And here is one of the figures from there. Less than two percent of the material was reduced to below 1,000 microns. Now there is also data in there on higher burn up, peak burn up at 30,000 megawatt-days per metric ton uranium, which showed that you could reduce ten percent, the impact -- the impact crushing test could reduce ten percent to under 100 microns.

Next slide -- I'm sorry, under 1,000 microns. Available information about the fuel suggests ceramic pellets just might retain a lot of their integrity in a volcanic conduit. Travel time and distance would be very short because they are already near the surface. Pellets have high yield strength at elevated temperatures.

Magma quenching on pellets could provide a protective layer. Entrainment in a frothy magmatic fluid over a short distance should not create high impact stresses. The melting point, of course, is much higher than the magma, 2,800 C.

And there is a natural analog, xenoliths, which can be of considerable size, travel much greater distances, kilometers, ten kilometers through the crust, and survive quick adequately. That is an excellent natural analog. Why are xenoliths not all reduced to ten microns?

Next slide. Implications for performance. Well, let's go ahead. You folks can read that.

Conclusions. Actually I sort of gave conclusions as we went along.

Next slide please. A significant fraction of extruded high-level waste would be entrained in stable features, scoria cones, lava flows. Consideration of entrainment in lava flows has really not be considered. I don't know if that is included in the DOE models. Are yours all tephra as well?

DR. SMISTAD: I'll talk to that in my

presentation.

DR. COLEMAN: Because of its large drainage area, it is possible to have flow in Fortymile Wash from distant storms where little ash would exist which would erode and transport sediments in the channel without adding additional contaminants.

Next slide. Roll on. Roll on. A more realistic size distribution for spent fuel would probably reduce calculated doses. This can easily be tested in the TPA code or the next iteration of it.

And I just wanted to point out some of the folks who had documented these floods in the region. Now retired, Pat Glancy, David Beck, USGS.

MEMBER HINZE: Thank you very much, Neil.

We'll go immediately into Dr. Sara Rathburn, from Colorado State University, who will be briefing the Committee on the important processes for fluvial and eolian transport of sediment and, therefore, potentially of volcanic ash that is contaminated with radioactive waste.

Sara, it is a pleasure to have you here.

DR. RATHBURN: Thank you.

MEMBER HINZE: And if you can keep us on time, we'd appreciate it.

DR. RATHBURN: I can do that.

Okay, all right. Thank you. I think I can keep everyone on time. That's because I feel like I get the easy job. I sort of get the travel tour. I'm going to back up a little bit and talk a little bit more in general than Neil giving more specifics on fluvial and eolian processes at the site. And I'll just talk about dryland rivers in general.

So my main points are to look at the processes that transfer water and sediment down the hillside into the channel and down gradient.

So I'm going to define drylands to include all of the above, hyper-arid, arid, semi-arid, and dry, sub-humid regions. So what these characteristics, channel areas have in common is that the potential evapotranspiration exceeds the rainfall. So there is a net moisture deficit annually.

You can envision that these span very diverse climactic regions. And we can have cold, high latitude, high altitude regions. We can have warm, low latitude, low altitude dryland regions. So I'm going to focus on just the warm drylands in this talk because this has more application to Yucca Mountain area.

And these are characterized by high variable degrees or aridity. They have this low rainfall to potential evapotranspiration ratio. And we know that there is sparse, unevenly distributed vegetation.

But the fluvial processes are significant agents of erosion. And they are important landscape-forming features.

Next slide. The fluvial systems, in general we can look at them using different models. I'm going to go through this quickly. Just for the sake of brevity, we can use a mechanistic model. And that would be maybe looking at this balance between driving and resisting forces. So the flow hydraulics acting on a bed of alluvium or bedrock, for example.

Process form interactions at various spacial and temporal scales, that is underlined because I'm going to get to that in a minute. Many of these terms arose from early research on ephemeral channels in the southwest where they looked at equilibrium, and thresholds, lag times, complex response, I'll also get to.

Persistence versus transient looks at how long these land forms exist. And a land form would be persistent if it endures until the next comparable magnitude event.

We can look at a basin model, upstream

versus downstream fluxes of material and transfers say from tributaries to trunk streams. Stan Schumm's three zones have some real pertinence for Yucca Mountain area. So I'll get back to that. Role of disturbance would be another way, biotic geomorphic interactions, and looking at a long-term history.

But I do want to emphasize the uniqueness of these arid region systems. We know that they are ephemeral. We know that they are flood dominated. A flood in these regions occurs any time there is flow that is delivered to just a normally dry channel irrespective of the amount of water.

They are discontinuous in time and space both in the channel features and the events that change them. Riparian vegetation plays a very important role in stabilizing the banks and bed. It creates a lot of roughness. It dissipates flow energy within the channel and on the overbank areas.

There is an important role in subsurface and upstream hydrology. So transfers of water, say from the headwaters down further into the basin, many times those transmission losses don't allow water to flow the full length of the channel like Neil discussed.

And complex response describes how a

channel can actually undergo two different states, say erosion and deposition as a result of the same triggering event.

Next slide. There has always been kind of this question of a balance or an equilibrium between the process or the physical forms that we see in a channel or within a drainage basin and the -- or sorry, between the processes acting on the physical form. So many times in perennial rivers, we can actually make measurements of the channel form and the processes. And there are some linkages between those.

We can't necessarily say that. And we know that the forms of dryland rivers may not result from a response to the dominant process.

So this is getting back to that process form interaction at different time scales. This is a really nice way of looking at an increasing length scale on the Y axis, increasing time scale with these boxes representing changes to the channel.

On the time scale, it is very short, say a flood event. We can rapidly modify transient bed forms, transient features such as maybe ripples on a sand bed stream. But it takes much longer time scales and a whole sequence of flood events to actually change the profile gradient. So that trace of the deepest part of the channel downstream.

At these middle regions we could actually change the plan form or how the channel looks from up above, whether it is a single thread or a multi-thread. And we could extend this out even. Something that maybe we could change on very large time scales may be stream piracy or complete drainage reversals.

Okay. This is the second model that I thought was particularly applicable to the Nevada test site. This was developed by Schumm. And he has developed sort of an idealized way of talking about drainage basins where we have a production zone up at the headwaters, a zone of transfer within the middle portion of the basin, and a depositional zone.

And we know this fits very well when Neil talked about Fortymile Wash where we end up having inputs and outputs all along but in general the dominant processes are expressed here. This could be this depositional fan that he showed near where the highway crosses Fortymile Wash.

Okay. Next slide. So let's go into the drainage basin and I will point out some of the key processes. And kind of sort of the take home message from this.

While we know that there is this

combination of low rainfall and sparse vegetation so we get locally very high rates of Hortonian overland flow. The hill slopes erode by surface wash processes or the runoff could actually infiltrate before it reaches the channel.

This generates very high drainage density, sometimes on the order or 100 kilometers of channel length per square kilometer of basin area. So you can see they are highly dissected, sparse vegetation, flow that gets branched out, and finally sort of coalesces downstream.

We also know that there are very high sediment yields. And this is a curve developed by Langbein and Schumm. And I'm not going to go into much detail. But they determined that the highest sediment yields are produced at a combination of effective precipitation of around 300 millimeters per year. That is about 12 inches, okay.

More precip, there's greater vegetation to stabilize the slopes. Less, there's less vegetation but there is not the flow that actually drive sediment into the channel.

Okay. What else we know about the drainage basins is that because of the low amounts of precipitation, there is little subsurface flow available for solute removal. So soils develop very slowly. They tend to be very coarse grained, particularly in younger dryland areas. And there is low production of clay minerals.

We get thin, shallow soils, calcretes because the products of weathering tend to remain in situ. And so we develop this gradual accumulation of layers, in this case calcium carbonate, that are cemented in and can then further influence infiltration and runoff.

This is just a view of Death Valley. Here is a debris fan, a debris alluvial fan. It is probably dominated by debris flow activity bringing coarse material from the highlands down into the valley.

Okay. What else we know about the drainage basin is that rills and gullies are important sediment delivery agents to the channels. So overland flow takes this water, it actually concentrates in rills and then gullies. And it expands by headward migration, so increasing the drainage network, delivering lots of material to low areas.

The channels themselves are wide and shallow. And they are usually of low sinuosity. They have low bank stability. And they are frequently braided. And they may terminated in a fan.

You can see that they are not armored so there is not an accumulation of sediment on the bottom that is a coarse lag that is left behind. This is very typical of dryland regions. And that armor means that there is really high availability of sediment to be transported.

Next slide. So the processes of stream flow, we know the floods need to move the sediment. They are transient in nature. The stream flow, it is non-uniform and unsteady. There are flashy hydrographs, steep-rising limbs, steep-falling limbs, short time to base. So this high intensity, short duration.

And the transmission losses are important. This can shift channel geometry every time that there is a flood so it makes it very hard to gauge these flows, very hard to get real time data.

Next slide. But the role of floods is supreme. The process studies that are available in the literature are dominated by analyses of floods.

We know that they have the potential to move very large quantities of sediment. They drastically alter the channel morphology. The channel may evulse completely cut off, shift to an entirely new location, and it disrupts the in-channel vegetation.

So Costa did some work in 1987 and he found that of the 12 largest floods ever measured in the U.S., all occurred in semi-arid to arid regions. And ten of those occurred in regions with less than 400 millimeters of rainfall. So this is exactly the conditions for Fortymile Wash and to appreciate the high spacial and temporal variability of these flows.

Next slide. Moving on to the fluvial sediment transport. These are transport-limited systems so there is more sediment than actually can be moved by the water that is available. It moves a stepwise episodic events, often in waves. Neil mentioned this.

I'll start with a bedload, first of all. Bedload is the coarser fraction. It can actually saltate, roll along the bottom, or get entrained up in the flow for little bits of time. There is very high bedload transport efficiency because of equal mobility. So that means that clasts of all sizes can move at one time during one flow event.

This is some work that I took from Knighton and it is after Reid and Laroone. And I wanted to circle this area. Much of the very good sediment transport data comes out of Israel, out of the Negev Desert.

And they found that they actually have collected about 30 years of high-quality data on bedload and suspended load transport. And you can see several orders of magnitude higher bedload transports in the Nahal Yatir than in a near perennial oak creek.

High suspended sediment concentrations are also very common. They have been documented at 30 to 50 grams per liter. So 3,000 to 50,000 ppm and upwards of 230 grams per liter. So this can actually account for about 68 percent solids. So it starts bordering on a hyperconcentrated flow.

And I've shown the Colorado River. Although it is perennial river, this is the Pria River in flood that has joined the Colorado River. The Pria mouth is just upstream a little bit. And you can see the very highly turbid flow that is distinctly from the Pria River. Okay.

CHAIRMAN RYAN: I'm sorry to interrupt, but I just got handed a note. We're finding out many flights have been cancelled in the local airports today, and if you need help with accommodations, the staff is more than willing to help you get set up for accommodations. Jenny Gallow or Michelle Kelton will be -- Michelle, are you over there? She's not, okay. Well, we'll help you get accommodation. I'm sorry to interrupt, but I just want to tell folks we'll be happy to help you if you need it, so let us know.

MEMBER HINZE: And help with accommodations if they can't get a flight. Right?

CHAIRMAN RYAN: Yes, we're going to help get them setup, so they don't have to sleep in the hallways.

MS. RATHBURN: Okay. So what else we see is that there's discontinuous erosion and deposition, and this makes it very highly complex, where we may have scouring and generating these microterraces in the bottom of the channel because flow may be due to a sort of low magnitude flood, and it's just carving out deposited sediments that are in storage waiting to be entrained.

The vegetation provides an important roughness component, as shown here in the Negev, there's actually vegetation in the channel. It decreases flow velocity. It actually dissipates flow energy, and it can generate deposition on the downstream side. So there can be unsynchronized scour and fill in a complex response-type manner, and this complex response is shown here, where we could have sort of one trigger. It may be a downstream drop in base level, or that level to which rivers erode, or some internal threshold that actually triggers some channel incision. These are cross-sections, so we're looking into the channel, and flow would be coming out at you, so we've got channel incision.

As this incision propagates upstream, we're going to get increased sediment supply that's now going to cause aggradation. As soon as that incision stops, we're going to reduce that sediment supply and start another round of incision, and eventually the channel will stabilize, but we get these two filled terrace as a result of one single lowering event. So when interpreting flow events based on alluvial deposits, it's important to remember that we can have this complex - two things happening at the same time.

Okay. I want to briefly mention piping as another mechanism that brings sediment into the channel. These can occur, typically occur in areas that have higher silt clay content, and some discontinuities in the bank. Here's a Hatfer scale and we're looking at the bank of Cienega Creek, and there's a pipe that's developed here. This is within the unsaturated zone, and it bridges, and it can break delivering a lot of sediment awaiting transport into the channel bottom. Next slide.

This is a summary, and I wanted to put this up because it allows me then to talk about some of the processes important to 40 Mile Wash. And this is just this climate we know that has potential evapotranspiration that exceeds rainfall. We have low soil moisture, sparse vegetation, high erosion rates that feed into high sediment concentrations for the channels. Also, low net soil removal, so we end up getting coarse material that's going to feed into some gravel bed, probably braided streams. Duricrust and evaporite formation that will influence then subsequent runoff.

Generating this high width to depth ratio channel, high drainage densities within the system, and the hill slopes and channels are going to operate the same, whether it's introduced pyroclastic material, or it's ambient native alluvium that's there. Okay? It kind of depends on the distribution of that pyroclastic material, and the size range, whether it's from sort of bomb size to ash. It's going to all be moving downhill, down gradient delivered to the channel, and eventually waiting to be flushed out. Next slide.

Aeolian transport may also be a really important source of sediment transport. It may be the dominant, the Sahara Desert obviously has a lot of Aeolian transport. It's usually sand-size particles and smaller. Many times a desert pavement develops, or the coarse class are just left as a lay on the surface. These can be stabilized, these sort of fine grain deposits can be stabilized by vegetation, or a biogenic crust can develop that's sort of algae, and cyanobacteria, and microphytes can actually stabilize it lightly, but if it gets broken, it certainly then exposes these fine sediments to potential alien transport, will move as saltating grains across the surface or entrained in the wind column. It sort of depends on the dominant wind direction. I guess at the site, it's actually from the southwest to the northeast, but it's highly complex, and there's a pretty involved flow field that needs to be considered when looking at entrainment of pyroclastic material, so the ash. Next slide.

We do know that there's an important component to soils that is derived from Aeolian deposits. This is some work by Chadwick and Davis in Lahontan Basin, and they saw that there were important soil-forming intervals caused by pulses of windblown sediment. Here's a loess cap here, and here's one on top. So at times when the winds were very strong, entrained lots of sediment, it actually deposited, and it infiltrated the coarse alluvium forming loess caps here separated by clay ridge argillic horizons within this soil. This is a compound soil that's about 65 to 70,000 years old. Next slide.

Steven Tooth has an excellent article that summarizes the processes of form and change in dry land rivers. You can read that. I'm not going to go over that. Next slide.

I do want to mention some of the challenges, and I know all of you have worked out here on the alluvial and aeolian processes are aware of these. It's really important to get good quality precipitation and flow data, and it's also very difficult. It would be important to get direct realtime measurements, or even historical and systematic records. It's important to understand the connectivity between these systems from the hill slopes to the channels, the tributaries down to the trunk streams. And one more final challenge is understanding dry land river behavior over long time scales, even extending well into the Cenozoic beyond the quaternary, because many of these sites have withstood quaternary glaciation, so they sit as relic landscapes, and you can actually interpret the successional sedimentation right there because they're so well preserved. And that's it.

MEMBER HINZE: Thank you very much, Sara. Both of these papers are now open for questions or comments. Ruth.

MEMBER HINZE: Sara, how likely is it - just as a guess - that you get any of this deposited material lifted up, and what would be the time scale, because when you deposit at first, don't you get a sort of crust, and then it dries out. Then material is available to be transported by the wind. What's the sort of fraction of stuff that gets transported, and how fast does this happen?

MS. RATHBURN: So that sort of depends on the grain size, and whether or not you actually have some protection of the surface by maybe coarser class. But if it's fine grain sediment, I'm talking sand and smaller that just makes it to a depositional fan, and I'm going to have Neil speak to this, too. Yes, indeed, it will infiltrate. Depending on the soluble minerals in there, maybe you will get some kind of a crust, but the wind is going to entrain anything that's kind of unprotected, so there's the potential always, I think, for Aeolian transport. And I have not actually walked out on the fan. I haven't been to Yucca Mountain, except vicariously through all of the papers that Neil sent me, so I'm going to defer to you to talk a little bit more about it.

MR. COLEMAN: The only thing I would add is, talking with Pat Glancy in those times after floods had happened, that on windy days they did notice - it's anecdotal information - did notice more dust in the air coming up off the fan, but it didn't, necessarily, last a long time. They noticed it maybe for a week or two afterward - anecdotal information.

MEMBER HINZE: Questions? Chuck.

MR. CONNOR: Neil, I have some questions or comments about the tephra dispersion results that you showed in your presentation. First, on the White Paper, you know, tephra dispersion modeling is probably one of the most active areas in volcanological research.

MR. COLEMAN: You're referring to the slide showing the ash from --

MR. CONNOR: Or, for example, this one.

CHAIRMAN RYAN: Which number is it?

MR. CONNOR: Slide 12.

MR. COLEMAN: Yes. That's from Don Hooper's presentation, yes.

MR. CONNOR: Right. Or anything related to the tephra dispersion, because that's part of this process of getting material into the wash to move downstream. And I just want to point out that something like 10 papers a year are being written specifically on tephra dispersion modeling, and none of that literature was cited in the White Paper. I think you really need to be careful with this tephra dispersion modeling, because, for example, on these plots, your waste in -- oh, I see, you're forecasting the waste dispersal. Okay. Is that right, the waste accumulation, or the --

MR. COLEMAN: Yes, that was a model result that Don presented. I wasn't there when he gave the presentation, but I have the talk, a very interesting one.

MR. CONNOR: Okay. All right. I think --

MR. COLEMAN: Don is here if you want to --

MR. CONNOR: Yes. Is that waste accumulation, or tephra accumulation?

CHAIRMAN RYAN: You'll have to come to the microphone, and tell us who you are, and answer the question, if you don't mind, please. Thanks.

MR. HOOPER: This is Don Hooper from the Center. Yes, the figures are marked as being waste. Some similar figures show just tephra, but those --

MR. CONNOR: Okay. That clarifies it for me, thanks.

MEMBER HINZE: Okay. Further questions? Please, John.

MR. STAMATIKOS: This is John Stamatikos from the Center. Neil, a question I have for you concerns the statement you make on slide 39, where you say, "Assessments that neglect long-term distance transports of silts and clays by large floods will over-predict the mass small diameter contaminated ash deposited near the RMEI." And I want to go back then to your chart on page 17, and ask you how significant do you think that over-prediction will be, given that I count 21 flood events, and only three of them would have discharges that would reach the Amargosa, and thus, have the potential for what I think you're getting at, if I understand what you're saying on page 39, that the amount of material that would get carried passed the RMEI and out into the Amargosa.

MR. COLEMAN: To have three events in the very short period of record is a lot, and to have one event that is beyond what had been calculated or estimated as the 100 year flood in such a short period of record, I intuitively start to wonder if the 100 year flood magnitude is correct when you have one that happens so quickly. The vast bulk of the sediment transport is going to happen in these very big floods. Now what was the first part of your question?

MR. STAMATIKOS: Well, I was just trying to understand how you got to that. I mean, you may be right. I don't know the details of the 100 year flood question. I just would point out, or ask you about the fact that you have 21 events in that same short period of time that move water, measurable water, at least, in the 40 Mile Wash, but only three of those are events in your table here that carry that material much further beyond the RMEI and into the Amargosa.

MR. COLEMAN: But many of these would not even have left the vicinity of the site. They're just noticed flows that happened. These were the large flows that went all the way across the Amargosa Desert. And one reason I also suspect why these floods have been under-estimated in terms of magnitude, is it was thought flood waters were just infiltrated over such a long distance, it simply infiltrate and disappear. The fact that they didn't indicates that, first of all, the permeabilities may not be that large. But, also, if you underestimate the flood magnitude by a lot, that can explain it. There's a lot more water to go, and didn't have enough time to infiltrate.

Now as to the amount, one of the things, the Committee letter had pointed out the concern about the simplification used in the redistribution model about -- almost like a bucket approach where sediment would come down to the active fan, but it was never allowed to leave, despite - other than by Aeolian processes to blow say toward the RMEI; when, in fact, there's this record of these very large floods that have transported it so much farther. And as Sara has pointed out, the shear volume of sediment that can be carried in the water, it's enormous.

MR. HILL: Britt Hill, NRC --

MR. COLEMAN: Oh, let me just add one other thing. It seems that one way to deal with it would be to at least address it by including a factor in the remobilization equation that allows for long distance transport. That would do it. How that is determined, a professional judgment.

MEMBER HINZE: Britt, do you have a

comment?

MR. HILL: Well, I had a question. Britt Hill, NRC staff. How much of the mass would be suspended load sediment, versus bed load sediment? My understanding is almost all the material we're talking about is bed load transport. When we looked at this simplified approach for looking at mass balance, the consideration was the bulk of the sediment is deposited in the active unvarnished parts of the fan. And while certainly there can be channel flow or some focused flow that continues all the way down into Death Valley, the amount of sediment that's carried in that flow is really just fine suspended sediment.

MR. COLEMAN: The stuff that's of greatest concern in health physics.

MR. HILL: What's the density, though, of the stuff that's of greatest concern of health physics? And are we sure that waste particles that have that high density would be hydraulically equivalent to clays?

MR. COLEMAN: They wouldn't be exactly hydraulically equivalent, but there is an incorporation factor that's used to make sure that ash particles are larger than the waste particles that are incorporated.

MR. HILL: Right. But during the transport process, we use the actual particle density of the included particles. For example, if you look at crystal fragments, you don't see crystal fragments as suspended load, even though you see a lot of clay particles, even small crystal fragments when you're dealing with densities of about 3 grams per cubic centimeter, they are transported along as bed load, not suspended load, unless you --

CHAIRMAN RYAN: Could I jump in? I want to ask a question, because this is exactly the kind of back and forth and discussion that I think could be well-served by the process that Tim talked about, and you talked about earlier this morning, Britt. Let me expand a bit.

Recurrence interval is an easy thing to calculate for recurrence of floods. Now you may not be happy with the statistics based on the number in your sample, but you can sure have a central value that you calculate, so let's take that one off the table. That's easy to address. Are three floods important in the sample in the time period? I don't know. Calculate it. That's straightforward.

All the issues of what's important to health physics, and I'll tell you my version of it, is anything that's under a micron, or thereabouts of a micron down. I don't care anything about 10 micron particles. They're not really restorable, 20 up to 100, forget it. It's not going to happen. Might get in your beard, you and me, but that would be about it. So if that's the endpoint we're looking for, and we're making assumptions that impact that up or down, I don't care whether it's up or down for the moment, but documenting how we made decisions that can impact that endpoint, and if we agree that is the endpoint, that would be a real good discussion.

Now, again, I offer that without trying to figure out anybody being right, or anybody being wrong, because there are a range of models, and we've talked about why that's important. That would be an interesting exploration, so I think, to me, anyway, from the conversation I'm hearing, this might be an example where some additional explanation along the lines that we've talked about with Tim, and you all this morning, that would be helpful. Am I making sense, or am I out of whack?

MR. HILL: You're making sense.

MEMBER HINZE: Well, I think you're making sense if you really focus in on the uncertainties. And I think that's one of the things we're not hearing.

CHAIRMAN RYAN: Well, the alternate conceptual models are step one, and talking what's certain or uncertain about them is step two, and I appreciate all those points. But this might be one of the things where Tim was saying if we do a little bit more thorough and rigorous, I guess, job of laying it out so if we both go in different rooms and read the summary, we'll both come back to the room with the same thoughts. That would be good, so I just offer that observation for you to think about.

MEMBER WEINER: Could I make a --

MR. COLEMAN: Also, the question that Britt asked is one reason I requested the scientific notebook, which actually he wrote.

MEMBER HINZE: Let's move on. I think it's obvious that we have a number of interesting topics related to not only alluvial but Aeolian transport, and we haven't heard all of them yet. And with that, Ruth, do you - are you in charge of Anspaugh's five minutes here?

MEMBER WEINER: And I in charge of events?

MEMBER HINZE: Right.

MEMBER WEINER: I wanted to ask Neil, have you talked to Dr. Anspaugh?

MR. COLEMAN: I have not reached him by phone, but I sent email.

MEMBER WEINER: Oh, okay. Then we can just wait. Let me just ask if he's on the bridge.

MR. COLEMAN: Is Dr. Anspaugh on the bridge line?

MEMBER HINZE: No, then we should move.

MEMBER WEINER: Yes, let's just move ahead. Let me just say what we had planned to do, if we can get Dr. Anspaugh. At the 2004 meeting that we had, where Don Hooper presented, Dr. Lynne Anspaugh made an excellent presentation on resuspension of fine particles. And we wanted to get Dr. Anspaugh here for this meeting. He's, unfortunately, not able to come, and if we can get him on the phone bridge, we have his slides from that presentation, and thought we would present them to the assembled company. We do have the slides, if anyone wants them.

CHAIRMAN RYAN: We don't really want them. I mean, we can have them as part of the record.

MEMBER WEINER: Yes.

CHAIRMAN RYAN: We don't want to show them unless he's here.

MEMBER WEINER: No, we're not going to present for him. We only wanted him to present. That was my point.

MEMBER HINZE: Thanks very much, Ruth. And I believe those slides can be made available at the back of the room.

With this, we are the point of just finishing lunch, according to our original plans, and Dr. Britt Hill from the NRC will give some perspectives on the NRC's position regarding consequence as it relates to the igneous activity White Paper.

MR. HILL: Can you hear me well enough? Never know with these microphones.

(Off the record comments.)

MR. HILL: Since we all need to get to lunch pretty soon here, I'd --

MEMBER HINZE: No, no, we've had lunch. You're keeping us awake.

MR. HILL: Great. I'm the first talk after lunch, so let's move forward. I'd like to present a brief perspective on some of the NRC information that we're using to gain an understanding of potential risk from igneous events focusing on the consequence area, and not really talk about probability, at all.

In the short amount of time that we're

going to spend here, I'd like to just present a little bit of background information, but focus on the risk-significant features, events, and processes that we see for igneous activity, and take this rather from a process level, from a more risk-informed perspective of what's really driving our understanding of risk, versus things that may have a secondary effect on risk from potential igneous events.

Each one of the areas I'm going to be going over, I'll be talking about some of the review information that we have for areas that we think are significant to performance. And I'll be focusing on the information that the NRC staff has developed. It doesn't represent the full range of information that we'll be considering, but we really wanted to confine our discussion and comments today on the presentation of NRC-generated material in the ACNW's draft White Paper. In each one of these sections, I'll also be giving some very top-level concerns about how that material is being presented in the ACNW report, if, in fact, we have any concerns with that material.

I do need to give just a little bit of background information about why we conducted some of these independent investigations. We are faced with an unprecedented challenge here in trying to figure out for the next 10,000 to potentially a million years, what might happen if a basaltic volcano intersected a potential repository at Yucca Mountain. Needless to say, you can't really go on to a reference system and find a lot of reports and literature outside of the stuff that's been generated around this project. There are large information gaps, there were large information gaps in the existing literature when we began many of these studies. I'm pleased to say that the science has moved forward, and we've been able to close many of those gaps in very significant ways.

I want to correct a potential misunderstanding. We have not developed a position on igneous activity. We have not set out to say the work that we've done represents the truth or the most scientifically correct information. It represents a perspective, and in some case, the initial technical basis that we've used to develop an understanding of these processes, to develop risk insights, and also, to question the information that becomes available from the Department, and other interested parties.

We're open to new information. We're going to be evaluating that information as it comes up. Certainly, we can show from our many interchanges over the years, we've questioned the Department in important areas. Sometimes the Department has responded, and modified their models and technical bases, and other times they've said nah, we got what we need. So we've helped to have a more transparent understanding of these issues, but even though we achieve that level of understanding, we're going to be considering the full range of information that's available at the time of licensing, so we have not concluded anything about igneous activity.

We do have insights. We have been doing a lot of work. We have a good process level understanding about what's really driving our risk for potential igneous events, versus what are some of the things that are not that significant. Just from a very quick overview, the most important part is the airborne release pathway. This is the only pathway that gets direct deposition of material in some realizations out from the repository to the accessible environment in say the first thousand years of post-closure performance. That's the reason it is risk-significant, and is dominating the significance scenario. So an understanding of the volcanic disruption processes has been paramount in our program, with a secondary understanding of what's going on in the subsurface for potentially intersected drifts, and the response of waste packages that would remain in the drifts following a possible igneous event.

Obviously, the risk is going to be directly proportional to the amount of waste that's entrained in the volcanic eruption. That's high-significance to performance. Also, of course, the event probability is high-significance, but we'll compartmentalize that for another talk.

Some of the other processes that would potentially affect our understanding in a high way about volcanic disruption are the formation of secondary conduits, bocas or breakouts, and also the inhalation pathway. Once the material is on the ground or redistributed, how much mass could be inhaled per time by the receptor? Secondary processes, things that have lesser significance to performance, would include processes of surface water and wind reworking, and also, some of the things that go into eruption transport modeling, variations in eruption volume, uncertainties in wind, how that may be represented in models'; and, also, some of the ground water release pathway. So I'd like to keep our focus on the more significant aspects, but not ignore the lesser significant aspects from igneous activity.

We'll start off with kind of a process level, the starting point. What happens if magma that's rising up potentially intersects a drift? Our risk question is how far might that magma flow into drifts? Because most of this relates to waste packages that remain in a drift, this area is one that has medium significance to performance based on our understanding, so we've developed a range of information, primarily from numerical and analog experimental models to try to take a look at the risk-significance of this process. Obviously, there aren't too many analogs that we can go out to, where rising basaltic magma has intersected large voids hundreds of meters below the subsurface. In fact, we've been looking for one of those for years, and if anybody has a good example, we'd love to hear about it.

Some of that information, which, in itself, is probably a half day's worth of talks, some of that information shows that if magma intersects a drift, it will depressurize, flow rapidly, and fill these intersected drifts with the molten magma at approximately one to five minutes after intersection. Now we have examined a range of potential conditions, maybe not as complete a range as some people would like, but certainly, a large range of potential conditions that go from highly over-pressured gas-rich magmas, all the way down to, essentially, gas-absent magma flow driven by pressure, or over-pressures, in the drift. Anything from the order of about 700 pounds per square inch, to about 1,500 pounds per square inch gives you a range of flow velocities on the order of tens of miles per hour down an intersected drift for a gas-absent magma flow.

The end result of all that understanding is a high likelihood that intersected drifts would be filled with magma, but we also have the ability to understand the potential effects of alternative models that would look at limited amounts of interaction.

The ACNW White Paper does not cite or discuss some information that we feel is important towards an understanding of the models that we've developed for this process. There's process-level reports that haven't been addressed that talk about degassed magma flow, for example, two-phrase flow in dykes, and also, maybe ascent and flow processes that should be brought into the relevant discussions in the draft White Paper.

Another top-level area of concern that we have with the draft White Paper is that we really don't have a framework, and I think Dr. Hinze had talked about this earlier, we don't have a good framework to understand the significance of potential disagreements between the information that's presented as the ACNW's perspective on an issue, versus the apparent disagreements with some of the information that we've developed. This report could be enhanced by a common understanding of risk-significance, or by an understanding of conclusions that would explicitly talk to how significant are these differences, even from a technical perspective, or as we prefer, a risk perspective.

Finally, there are some limitations in alternative models that just really aren't addressed in the ACNW's draft White Paper. For example, observations that depressurized magmas do flow for more than a drift length out in just under simple gravitational load at active basaltic volcanos.

We had a lot of discussion throughout the years on waste package response to magma. We have a very simple risk question; will packages fail if they're directly exposed to magma? I think here it's important to recognize that the engineered system in this case represents a system that's important to safety, or important to waste isolation. There needs to be a good technical basis developed if credit is going to be taken for a safety system during a potential disruptive event. The burden isn't on the applicant to prove that a system will fail, or demonstrate that a system will fail; it's that the credit needs to be explicitly documented, if you're going to say the likelihood of failure is somewhat less than one. So we have no problem in probabilistic risk assessments, assuming that a component will fail as part of an event sequence during a disruptive event. If there needs to be credit taken, there needs to be a technical basis for it.

We have examined a range of information about Alloy C-22 response, stainless steel 316 response, during the physical conditions that we believe are representative of basaltic igneous events. The temperature response from the different melds, we heard Dr. Montana talking about differential expansion between the Intera 316 stainless steel and the outer C-22, but one of the more important aspects that we have to consider is that when C-22 alloy is exposed to temperatures above 600 degrees C, or so, there's a formation of secondary phases along grain boundaries. Those formation of secondary phases greatly alter the material properties after some amount of time exposure to these temperatures. The amount of time can be hundreds of hours at 700 degrees C, but these secondary phases can be forming quite substantially on the order of 10 hours at about 850-900 degrees C. And we know that these secondary phases will greatly increase the ductility of the metal, and weaken the ultimate tensile strength, resulting in, essentially, non-linear effects between temperature and ultimate tensile strength.

These are the kind of effects that would need to be accounted for if a full mechanical analysis was going to be done about waste package response to potential igneous conditions. There are physical forces, and it's important to remember that magma is a dense fluid. It has a density that's about equivalent to two Volkswagen New Beetles crushed together into a cubic meter. That could be sitting there under just the weight of gravity, or having some additional over-pressure being put on the system. So we have not done a full mechanical analysis, nor has the Department done a full mechanical analysis to demonstrate beyond any doubt a waste package would fail, but we view that the preponderance of information would clearly support the conclusion that waste packages will not remain in tact when they're directly contacted by magma. We also have no alternative information that would cause us to think worse things could happen than just waste package failure.

The draft White Paper really doesn't address some of the important information that I showed on the preceding slides about materials response, and also, coupled igneous processes in trying to evaluate waste package resiliency. It's not a simple assumption that we've made, nor has the Department made a simple assumption. We've looked at a lot of compelling information that seriously questions whether a waste package would remain in tact during an igneous event. Again, the report could be enhanced by some common understanding of risk or explicit conclusions to gain a risk perspective on the disagreements with the ACNW's positions. And, also, the limitations in alternative models should be more completely discussed. For example, there needs to be some coupling between mechanical analyses and thermal analyses for waste package response during potential igneous events.

Conduit formation is really one of the drivers for how much waste could be entrained and erupted during a potential volcanic event. It's controlling how many waste packages actually get up and are released through the airborne transport process. We've taken a strongly analog approach, because the numerical models, while they may be interesting, really make some fundamental assumptions that result in assumption-driven results on whether you want to have certain pressures, or certain conduit diameters, if you want to go to a numerical approach for conduit formation.

And here's an example of an analog volcano, something that Chuck is now using for his probability model. These are the outline of actual conduits in the San Rafael volcanic field of Utah, superimposed to scale on the proposed repository outline. And you can see that real volcanic conduits have multiple pathways to the surface. Each one of these larger stippled areas would represent some pathway that was likely active for some part of an eruption, but we have to integrate that all into a simple representation. So what we do is we look at the effective area of all of these conduits and say we're not going to try to model this realistically. We're going to abstract a simple geometry, simple cylinder, and just look from the range of effective areas, how much waste would be entrained during a potential volcanic event. And we're coming up to a size range that's anywhere from one to ten waste packages, with an average of five waste packages being erupted during one of these potential volcanic events.

By taking this approach, it also makes it very straightforward to us to evaluate the risk-significance of alternative information, reducing this down to a very simple geometric argument, rather than trying to argue that this field is the, or the inappropriate, or - I'm sorry - it's the appropriate or inappropriate analog for Yucca Mountain since, by the way, we don't have any of the conduits exposed 300 meters depth for most of the Yucca Mountain systems that we're worrying about.

Once again, we believe the draft White Paper doesn't discuss important NRC information that's relevant to conduit development, such as the magma ascent and flow processes, the modeling that's gone on to support the general observations we made; and, also, the range of field observations that we've drawn upon to develop the conduit distributions that we're using for our risk insights.

The risk-significance isn't provided for the apparent disagreements. I'm not always clear that we're in large disagreement, but certainly reading what's being written, there are apparent disagreements with the information that we've developed for this process, and there could be some alternative information presented in the report in this area.

Secondary breakouts, we know from active volcanos, basaltic scoria cone volcanos, like the 1975 Tobachick eruption, that from time to time in some of these eruptions, you get a breakout away from the central conduit, sometimes over a kilometer away from the main axis of the eruption. While our primary model, our base model, would be Pathway A, that's just a simple cylindrical conduit coming up and potentially intersecting the drift, we have to acknowledge that Pathway C may occur sometimes, leading to the formation of what's commonly called a dog-leg of flow connecting the main conduit with a secondary conduit. And, also, there's concerns that you may have sufficient pressure within a potentially intersected drift to drive magma up from that some point away from the point of initial intersection, and create Pathway B without some breakout from the main conduit.

We have information from analog volcanos, and also some numerical and experimental models that examine some of the different flow processes that may occur during these different conduit pathways. The effects of the alternative information can be easily evaluated, because we've reduced the abstraction of this process down to a simple, how many waste packages could potentially be disrupted? So you can look at the likelihood of this breakout occurring, you can look at the locations of the breakout occurring, and reduce it down to a fairly straightforward risk analysis to understand whether these alternative models are significant, or not significant to an understanding of risk.

The draft White Paper doesn't discuss or cite some of the important information that we believe is relevant to understanding secondary breakouts from the numerical and analog models; and, also, the information that we've developed or shown from field observations and active volcanos. We're not sure about the actual significance of some of these disagreements, again, trying to take a risk perspective. From our risk perspective, we believe these processes are highly significant to understanding total system risk, but we're not sure from the White Paper exactly what the magnitude of those disagreements are.

And, finally, the alternative models that are presented should have a discussion of some of the limitation, such as a lack of coupling between conduits and breakout, the limitations in those models just are not really well-developed.

Touch briefly on airborne transport of tephra. The total airborne transport process, while it's important, is not one of the drivers for risk in the sense that it seems fairly well understood. There is uncertainty and variability in the parameters, but the overall model seems relatively mature, especially when you compare that to a number of other models that are used in the performance assessment.

Down below, the lower figure, is a comparison that was done between the ash plume tephra model, and actual measurements that were conducted in the 1995 eruption of Serra Negra. I think it's important to point out that we went out into the field, developed the model parameters from field measurements, plugged those field measurements into the code, and got the resulting pattern. Now it's not a perfect mismatch, but you've got to agree, I think, that these are pretty good matches for the mass distribution between modeled and observed tephra patterns. So when we talk about model support, this is the sort of support that's a pretty clear example, that shows that there's a reasonable representation of the natural process for the numerical model that's being used in the performance assessment. This provides us with a pretty good tool for looking at parameter uncertainty, things like alternative views on what the eruption volume is, should we be modeling the entire volume of the eruption, or just the tephra volume of the eruption, waste partitioning effects, et cetera.

It's not to say that this is the only model that could ever model tephra, but it's one that we're using, because we see that it works, and the parameters are pretty straightforward. We would expect, though, that other models that would be used in licensing would have some level of model support to show that they're reasonable representations of tephra dispersion.

The draft White Paper could be improved by discussing some more of the important information that NRC has developed on airborne transport, such as model support, model sensitivity. I personally believe that a better understanding of model sensitivity could help focus a lot of the areas of apparent disagreement, of whether these areas matter or not. For example, I know Neil was talking about alternative waste size distributions. One of the ways that we've resolved an issue with the Department of Energy, this was igneous activity agreement 2.03, where the Department had developed an independent basis for waste particle sizes. We had our independent basis. They were somewhat different. We asked the Department to do a sensitivity analysis to see whether or not those differences were significant, and the end result that we documented in this KTI agreement letter was that these differences had almost negligible effect, and that the models weren't sensitive to these kind of variations in waste particle size. So this is one of the examples that we would like to see acknowledged in the ACNW report, bringing in that kind of relevant information to help frame whether or not the differences in technical basis are truly significant to performance, or are just different technical perspectives.

Also, we're aware that there is a lot of alternative models out there, but there is a lack of model support for many of the models that we are aware of in the literature. And, certainly, for a Gaussian plume, there's some pretty significant limitations in using a Gaussian plume-type approach in modeling airborne transport from real volcanos. Those are the kind of limitations that we believe could help improve the ACNW report by giving a more full explanation of those limitations.

Now we talked earlier about the inhalation of contaminated tephra being important to risk, the concentration of re-suspended particles gives the inhalation dose for the RMEI. This is one of the key parameters that we need for the performance assessment. We have developed some information from analog deposits, actual basaltic scoria deposits, and used measured airborne particle concentrations. There's a range of information that's now available from the literature, which was very sparsely populated about five years ago when this issue really emerged. One of the key aspects, though, from that literature is that airborne particle concentrations above these volcanic deposits seem to be fairly independent of the particle size of the bulk deposit, itself. In other words, there's a lot more re-suspendable particles there than the air can suspend at any given moment in time. So, certainly, when you look at the long-term evolution of a stable tephra deposit, you could be depleting surface layers and re-suspendable material, but for short-term effects, the things that are driving a lot of our understanding, the grain size of the deposit doesn't have a heck of a lot to do with the overall airborne particle concentrations in the orders of magnitude that we're worrying about.

There's some information that we developed on the analog data; and, also, how you take that analog data into the Yucca Mountain region. That sort of information could be more fully addressed in the ACNW's draft White Paper. Again, the risk-significance, we need some basis for understanding risk for the apparent disagreements. And the sensitivities or insensitivities of alternative models or alternative size ranges needs to be more completely addressed.

We've had a number of interesting talks on long-term redistribution; again, a subject that we could probably spend a good half a day on, but in the one minute I've got, we'll look at how much tephra could be moving down 40 Mile Wash through time. We're using site-specific information in taking a sediment balance approach to focus on long-term behavior, rather than event-by-event-type behavior, just because we looked at this problem and said there is so much uncertainty in how this system is going to evolve with time, or respond to a deposit that doesn't exist out here, that taking a more process-level approach seemed very difficult for us to gain any risk insights from.

It would be model-driven results, rather than parameter-driven uncertainties.

This sediment balance approach captures the long-term redistribution processes, again looking at overall behavior, considering deposition in the unvarnished main part of the fan, compared to the more stable unvarnished areas around it. This approach, while it may not be the most realistic approach, is straightforward, and gives us a pretty good tool for evaluating parameter uncertainty, such as how much tephra may be deposited in different scenarios in the catchment area from 40 Mile Wash, what might be effects of Aeolian redistribution be through time, what kind of partitioning might occur between the redistributed deposits, initial deposits, and airborne deposits for the RMEI location. We have to consider all these sorts of problems, and not just focus on a single event, or a single process that doesn't really capture the long-term evolution in the system.

The ACNW's report could really discuss more of the information that we've developed in the area on the analog information, some of the more recent modeling reports from last year that were done; and, also, why we developed this approach, and how it's capturing the characteristics of the Yucca

Mountain region, as opposed to an analog site.

The risk-significance, there's a common theme here, obviously. We need a common understanding of risk to gauge the magnitude or complexity of the disagreements. And, also, a more complete understanding of the limitations in alternative models, such as focusing on single events, versus time averaged approaches.

I know we've had to give a very cursory summary on consequences, and I'm hoping this helps frame a dialogue, and provide some feedback to ACNW to consider in the draft White Report. But I want to leave you with the following conclusions, most of which should be pretty obvious by now. The first, that we believe that we have sufficient information currently to support staff review of the potential DOE's license application regarding igneous activity consequences. We do not see any glaring gaps in the information that we will need to do our job if the DOE submits a license application in June of 2008, as has been announced.

In each of the areas that I've discussed, we're concerned that the ACNW White Paper does not address relevant information that's been developed by NRC for each one of these areas, and we believe a more thorough and complete representation of our perspective should be included in the White Paper.

As Dr. Hinze had briefly discussed earlier, the White Paper really could benefit by a consideration of risk insights to frame the technical disagreements, or some conclusions that would talk about the magnitude or judgment of the severity of those disagreements between the ACNW's analysis that occurs in the initial stages of the report, versus the comparison of that analysis to the perspectives that occurs at the final stages of the report.

And, finally, although we're aware that there are alternative conceptual models, or alternative information to many of the investigations that we've conducted, there could be a more complete understanding, and more complete documentation of the limitations of that alternative information when it's being used for comparison against some of the NRC's staff information. So, of course, this re-emphasizes the fact that we have not developed a position. These are not final judgments. We're listening to all of the information that's being presented today. And with that, I'd like to open it up.

MEMBER HINZE: Thank you very much, Britt. You're amazing, you're 30 seconds off from 30 minutes, so maybe you want to make certain next time that you're on time.

(Laughter.)

MR. HILL: Okay. Well, there's always room for improvement.

MEMBER HINZE: With that, let's open it up to the committee. Dr. Ryan.

CHAIRMAN RYAN: Thank you. I would say, though, that that 30 seconds is probably not risk-significant.

MEMBER HINZE: Well, it was actually 32, if we want to get into uncertainties.

CHAIRMAN RYAN: No, that's all right. Well, that makes -- the last two bullets in your conclusion slide I think are a good message for us, that we need to shape our questions and issues a little bit more fully than we have, perhaps, to this point. And I like the idea that you mentioned, that this is a good framework for a dialogue for us to understand what you've done in a more complete way, and for us to communicate our questions in a more complete way. I think that would be helpful.

I think, in general, the point that we may be not so far apart once we dissect all of that, is a good observation, and there may be new things we think about that we want to probe a little bit further. I think about dose conversion factors, for example. I'm always beating that drum, and sometimes they may be off by two orders of magnitude, too.

MEMBER HINZE: Let's not go down the ICRP pathway right now.

CHAIRMAN RYAN: We'll save that for another day, but it, nonetheless, is one of those things that could very well be a significant item from a risk perspective, because it could shift the dose, which is the risk, by a lot, or particularly for materials that have been boiled up in magma, and perhaps less soluble than they might otherwise be. So interesting to think about all those things, and I guess, to me, the path forward is to follow-up on what we've talked about today in several different pieces, which is kind of an agreement that you'll help us understand the range of things you've evaluated, as Tim McCartin laid out a bit, and you talked about earlier with John Trapp, and we'll do the same. I really appreciate kind of this top-level view, and the follow-up that will happen after it. Thanks.

MR. HILL: We're here every day.

CHAIRMAN RYAN: Fortunately, we're not, but we're only a phone call away.

MEMBER HINZE: Dr. Weiner.

MEMBER WEINER: One of the things that would be helpful to our development of this White Paper would be if the NRC staff would identify where they have simply taken - I don't want to use the word "simply" - where staff has taken a very conservative view without further analysis of - where you just cut off your analysis and said okay, we're going to do this because it's very conservative, and if we can show that they meet the standard, or whatever, with a conservative point of view, we'll stop there. It would just be helpful to identify those things. We try to identify them in our own way, but if the authors identify them, that's even better.

I have a question, too, and maybe you can answer them both. My question is, there are more things that need to be disrupted than just the waste package if you're going to get particles that are small enough to be inhaled out into the air. I mean, you also have the fuel rods, the fuel, itself. Do you have -- have you developed a mechanism, are you developing a mechanism to look at the disruption of the fuel rods?

MR. HILL: Well, for example, the information that we developed back in 1998 when we first had to consider the waste particle sizes, while it was guided by the limited information from crush impact studies, there were no crush impact studies that were done at the kind of temperatures that were relevant, nor involving full assemblies that were being broken apart like wall rock is being broken apart following prolonged exposure to magma, so it's not that crush impact is a good analog, but there needs to be some consideration of what would be happening at magmatic conditions. And that's where, at that time, we developed some information with people that know a lot more about this than I do, about would waste behave more - would it become more fragmented, do we worry about grain boundary effects, what would be happening under these conditions? And that's where the size distribution did change, because we had a number of waste people come in and say yes, the crush impact studies are not what's driving it. You need to give additional consideration to, essentially, grain boundary weaknesses.

So, I guess, to get back to the first point, that I'm not aware of any place where we have been very conservative, or wow, that's enough, we don't need to do any more. It's facing, though, realistically that given the limitations in available information, what would we have to do to develop a much more enhanced technical basis to look at the full integration of, say, waste form response throughout, not just the disruptive part, and the transport part, but once it sits on the ground for X number of years, the oxidation, aging, and chemical effects that may be occurring. I agree, there are an awful lot of complexities in how this potential event would be treating high-level radioactive waste.

We took a look at it, we gave it the best shot we could, to use the quote that we have, and we've also been using that as a perspective to evaluate what the Department of Energy is proposing as its technical basis. And as I said during the talk, in this specific instance, we're comfortable with that range being supported by the available information considering all the uncertainties in the conceptual model. But, certainly, if there are more mechanical analyses, or other information that we haven't considered, we're open to taking a look at that.

CHAIRMAN RYAN: Britt, just on this kind of update view of the world, never mind the mechanical properties of the fuel, I think Ruth got her answer to that. You, or somebody, mentioned that you're updating the wind rows, and you're looking at different updates to the model. When could we expect to hear about sort of the ensemble of things you are updating? The reason I'm asking is it would be probably not productive for us to start diving in on things from a model that's very quickly going to be updated.

MR. HILL: Our plans are to have a updated version and documented version of the TPA code by the end of this fiscal year. We're hoping to get that done earlier, but I don't want to make promises we can't keep.

CHAIRMAN RYAN: Down the line a bit.

MR. HILL: There's a fair number of module changes, like Tim has said. We are looking at a wholly new redistribution model. The previous model didn't have anything for redistribution in it.

CHAIRMAN RYAN: Well, let me suggest a path forward, and see if I'm on the right track. You had high and medium significance kind of parameters in sub-models, and so forth, identified earlier in your presentation. Would it be effective for us to try and focus on the highs, and then the mediums, or in some order? Maybe we could work on the idea, work on these first, because we're not doing much there, or leave these alone a bit, and we'll work on those later, once we get our own work down the line?

MR. HILL: Well, I think the supporting technical basis for all of this is pretty well documented, including the redistribution.

CHAIRMAN RYAN: Where you are now.

MR. HILL: Just taking it up to the final risk equation, that's the one where we're going to have to wait for the updated version of the code.

CHAIRMAN RYAN: Okay. Fair enough. I just want to make sure that we're not jumping ahead of you, and creating extra work, or creating extra work for ourselves, when something we're looking at is going to change. I just want to avoid that, if we can.

MR. HILL: Right.

CHAIRMAN RYAN: Thanks.

MEMBER CLARKE: Bill, can I follow up on that?

MEMBER HINZE: Please.

MEMBER CLARKE: Slide 4 I think speaks to, if you can get that out, what Mike was just talking about. I guess I would ask the question another way. Some of the things that you have designated as moderate or medium significance to risk are things that you're looking at now.

MR. HILL: Yes.

MEMBER CLARKE: You're looking at the total wind field, rather than a uni-directional approach. You're looking at alluvial transport, Aeolian transport, and re-suspension. And if I understood what you said as you went through the different pieces, it sounded like you're far enough that you still think those are moderate? In other words, when you finish your work, and you've run the TPA, do you think any of this will change?

MR. HILL: I want to make sure we have a common understanding of what risk-significance means. This is a relative risk ranking that compared to the model sensitivities, uncertainties, and alternative conceptual models that are available, considering that full range of information, what seems to be more significant in the igneous activity risk assessment, versus lesser significance, or negligible significance. There's other things in here that would have very little significance, such as variation in tephra sizes, given that we have a pretty good range to work from. So when we say something is a medium significance, it's relative to something that's very high significance, that may have the potential, for example, to give a factor of 10 variation in the risk assessment by the consideration of alternative models, versus something that may be oh, a factor of 5, factor of 3, sort of variation. So that's really what the mediums and highs are, is a prioritization, but it's not to say that we only look at the high stuff, or that during review, we'll only really look at the high stuff. We're going to look at the low stuff, too. But in a risk-informed regulatory framework, our most attention is driven on things that are most significant to performance. And it's the same thing here.

MEMBER CLARKE: I had that understanding of what you mean by risk insights, and I guess what I was asking is, that the work that you're doing now, it sounds like you don't anticipate that that would change that ranking at all. You might have a better handle on it, but Mike asked if we should focus on the highs, the mediums, and I'm wondering is there any chance that that might change when you finish what you're doing? It sounds like you said no.

MR. HILL: I don't think it's going to change in a large way. And, again, the purpose of risk insights isn't to really split this with a very fine boundary, it's to look at overall understandings.

CHAIRMAN RYAN: Mine was simply a workflow question for us of you, to decide what do we attack first, rather than do things -- the things actually change numerically when it's all said and done, so two very different questions.

MR. HILL: I understand.

MEMBER CLARKE: Okay. Thanks.

MEMBER HINZE: Allen.

VICE CHAIRMAN CROFF: No, thanks.

MEMBER HINZE: Let me take just a quick shot at a comment or question. First of all, I think your presentation of things that you think should be improved, will be very helpful to us. I must say that I'm disappointed that the comments are at as high a level as they are. As such, there's a lot of room for interpretation on our part, and also, misinterpretation, and that's the worst thing.

We would be better served if we could receive any lower level comments from you that would be more specific. And let me give you a couple of examples that I just looked at, as I - or interpreted as I heard you talk. For example, it isn't quite clear to me whether the Woods, et al paper with the shockwave going down the drift is still in vogue, from what you said.

Another example is that this past summer, Dr. Woods made an excellent presentation on breakouts, and those are high there. But we were warned before that presentation that that was strictly an interim presentation, and the - my interpretation of interim was that we weren't supposed to place that much credence on the results, because more results were coming down the pike.

Now these deal with both your high-risk items, and we're supposed to deal with those in the White Paper, and we want to deal with them, but we don't know how to deal with them, because we don't have the information. And the only way that we can do this is assume that you still - that the Woods, et al 2002 GRL paper is still in effect. I don't know what to do with the breakouts from Dr. Woods' work. That was strictly an analog basis, and there are a lot of assumptions that he made very clear to the committee. Kind of help me with this.

MR. HILL: Okay. As terms of Woods 2002, I guess I'd just like to refine the question about what is being invoked?

MEMBER HINZE: Well, you talked about the magma moving down and breaking out.

MR. HILL: Right.

MEMBER HINZE: That sounds a lot like

2002, to me.

MR. HILL: Well, the magma - I just want to make sure we're not focusing on shocks, because the whole purpose in the 2002 report to talk about shocks was that nobody had talked about it, when you're dealing with the compressible effects. But the --

MEMBER HINZE: And you served us well.

MR. HILL: -- important conclusion was that the shocks appeared low enough that they're not immediately disrupting the waste package. And it was the --

MEMBER HINZE: Britt, I don't want --

MR. HILL: Okay.

MEMBER HINZE: Excuse me, let me interrupt. These are examples, and I don't think that we need to take the time here to discuss those in depth, because we really don't have time. What I'm trying to illustrate is how a lower level of reporting from you would make us a lot more comfortable about stating your positions. And I've given those as examples of just - from your presentation and where we're having some problems, where we had some problems in producing the draft. So if you can, I plead with you, if you can, spend a half a day and try to put some page numbers, some section numbers, et cetera, and this can be a marked up copy or whatever, whatever makes it easier for you, would be very helpful to us.

With that, I'm not going to ask for a response, but that's where I'm coming from. I'm going to ask, are there any other questions?

CHAIRMAN RYAN: Just a comment, if I may, Bill. I think - and I'm taking the spirit of what we talked about earlier today, and what Britt talked about earlier in his presentation - that we're hopefully going to get there in terms of trying to identify - and I take away an important message from Britt. We could talk about, I don't know, a hundred things that may or may not have any influence on risk, or dose, or whatever endpoint you want, and that's the nature of science. You can talk about lots of stuff that may or may not be something that will change the orbit of the earth. But what would be helpful, I think, in the context of your question, and what we talked about earlier in terms of what Tim McCartin said would be their approach to better document, is to quickly focus us on the things that are significant to risk, and then probe those. And have the discipline not to get too far off of those center lines, as best we can. That'll be efficient for us, as well, I think. Am I on the right track?

MEMBER HINZE: Well, let me - I look at it a little bit differently, because I'm responsible for producing a document here in the next couple of months.

CHAIRMAN RYAN: So are we all.

MEMBER HINZE: And my head is on that, and your's, as well. And I hear Tim, and what Tim said today was one of the best things I've heard in this room for a long time, and it's great. But I don't really see it having a direct impact, except on our White Paper, because there isn't time.

CHAIRMAN RYAN: Well, let me say this - and maybe there will be some placeholders where we say we understand the staff is producing work in the next six months that will impact this question, and we'll come back on that when the information is ready. If we get that kind of feedback, that's -- it's not an --

MEMBER HINZE: Yes, we just don't want to --

CHAIRMAN RYAN: -- an absolute answer --

MEMBER HINZE: -- at that point.

CHAIRMAN RYAN: Well, it's not an absolute answer on a particular question, perhaps, but it sure is the state of where things are. And I think if there are issues that are in that state of flux, then we have to recognize it. It would surprise me if there aren't a few like that.

MEMBER HINZE: Well, we --

CHAIRMAN RYAN: That means they haven't done a lot in the last little while. I know that's not true.

MEMBER HINZE: And that's really in spades from our friends from the DOE side.

CHAIRMAN RYAN: Absolutely.

MEMBER HINZE: Because they're moving fast and well beyond us.

CHAIRMAN RYAN: We haven't had any briefings on many of these topics from DOE in months, and months, and months, so I think we need to recognize all that. So the White Paper may not be perfect, but it'll be close.

MEMBER HINZE: We're trying to produce a base point. With that, let me make a suggestion. Let's take a quick break.

CHAIRMAN RYAN: Okay.

MEMBER HINZE: A ten-minute break. We have three very interesting presentations coming up, and we want to give them their due cause.

CHAIRMAN RYAN: Fair enough.

(Whereupon, the proceedings went off the record at 3:17 p.m., and went back on the record at 3:28 p.m.)

MEMBER CLARKE:: If we could please come back to order, take your seats. We'll get convened again.

Is that enough?

MEMBER HINZE: That's enough. If it isn't, it's too late. With that, we move to the next presentation and DOE is going to give us their view, the white paper in terms of consequences of igneous activity.

Eric Smistad, we're looking forward to it.

Neil, do we have copies of this?

DR. COLEMAN: I believe you do.

MEMBER HINZE: I was asking in general, not for myself.

CHAIRMAN RYAN: Are there copies in the back, Neil?

DR. COLEMAN: I'll run some off.

CHAIRMAN RYAN: They are coming along, Mick.

DR. SMISTAD: Good afternoon, I'm back again. Just a reminder that like the last talk I gave, this presentation and the subsequent comments that we will send you are just evaluating the DOE portions of this report. We read the entire report, but we're just commenting on our portions of the report.

MEMBER HINZE: Excellent.

DR. SMISTAD: Again, the same instruction I had earlier today, we felt the report did capture, again, at the level it was written captured the work we'd done through the years. It's a snapshot. We are continuing work and we do realize that the report, you had to make a decision on what level of detail to include, so we had a cutoff point and we recognized that as well.

These are high level observations that I'm giving today that I plucked out myself out of the eight pages of comments we had and we will get you those detailed comments within your window.

We felt that there were some suggestions that dike drift work that we had done was perhaps conservative and it was in reference to, I believe, some work that maybe the staff or others had done. We were interested in particularly the comment on conservatism because we felt we wanted to see that work done in a quantitative fashion to help support the conclusion that perhaps our work was conservative.

So just a desire on our part to see the detailed work is really what this bullet amounts to.

We felt there was some information available in this reference here, dike drift interactions 2004, involving the analysis of the topography and thermal stress and the effect of that on dike propagation. Just a suggestion, we felt the report could have perhaps fleshed that out a bit.

We are, as I said, doing work as we speak and documenting it as well. This will come out in a series of AMRs, I mentioned towards the ends of this fiscal year. We've done -- the guys at LANL have done quite a bit of work out in the field and it's helped us refine some of these parameters, the parameters you see here, fissure length, I've got dike length here. Fissure length moving towards dike length, dike width, number of dikes in an event, the potential for eruptive -- a number of eruptive conduits in an event, conduit diameter is another parameter that will change per LA. And then I think this is what Neil was alluding to earlier, we've taken a look and are including our work partitioning of the waste in different components of an eruption here.

MEMBER HINZE: Excuse me, but this is over and above the work that Greg Valentine has published on?

DR. SMISTAD: I don't remember how -- no, it is part and parcel to some of the stuff that he's published, right, right. I don't know if all of this in the published papers or not. I don't think it all is, but I don't know about the partitioning piece. Anyway -- is the partitioning piece in any of the papers that you've recently put out?

MR. VALENTINE: Greg Valentine from Los Alamos National Lab.

The scientific work that forms the underpinning for these parameters is all provided in the papers that are being published in the open literature.

The condensation of that information down in the specific parameter distributions, for example, dike lengths or conduit sizes or the partitioning, is really going to be articulated in one of the AMRs that is going to be completed by the end of this month.

MEMBER HINZE: Thank you.

DR. SMISTAD: Thanks, Greg. We do have a new model. We realize that the Committee did not have access to this, a new model for redistribution. This had previously been communicated as an alternative model in our work and you guys had noted that in the report.

That alternative model is now our basis for LA. It's a process more process driven, mechanistic model that includes some of these elements, among others. But hill slope erosion, a more detailed look at transport and mixing, and then the fusion of radionuclides in the soil at the site of deposition of the fan.

We've also taken a more detailed look at magma repository interactions. There's a suite of -- I won't run through all of these, but a suite of more detailed analysis that we've done with that particular model. And the last one is -- I'll just note this one, the last dash here is looking at the aspect of freezing of the magma in the drifts itself. This analysis is underway right now. We have no results at this point.

Conclusions. Same -- I believe this is almost the same conclusions I had earlier this morning. I don't think there's anything I need to go through here in terms of conclusions.

That's all I have.

MEMBER HINZE: Thank you very much. Questions, comments?

Anyone?

MEMBER WEINER: I think this is very helpful. thank you.

MEMBER HINZE: Thanks very much, Eric. We'll certainly look forward to your written comments in the next couple of weeks or so. Appreciate it.

With that we're ready to move to the next speaker and EPRI's representative, Mick Apted, will be briefing us on their, EPRI's views on the white papers dealing with consequence issues.

Mick, it's a pleasure to have you in front of us again.

MR. APTED: Again, I'm Mick Apted.

CHAIRMAN RYAN: While it's getting wired up, if anybody does have travel issues or needs help, the staff is ready to help anybody that needs to get organized on a plane or an overnight hotel or whatever.

MR. APTED: I needed an overnight hotel.

CHAIRMAN RYAN: Okay. You have nothing right now.

MR. APTED: That's right, other than doubling up with somebody, so -- well, great. It's a pleasure to speak in front of this collective group, the ACNW white papers have sort of become the focus for. And the it's also an advantage speaking last.

I get to go back to other people's talks and pat them on the back and agree with them and maybe draw some points of departure with them and so on. So it's an advantageous position to be speaking towards the end of this meeting and the last two hours of time I have allotted to myself.

(Laughter.)

No, I have a rather short talk. As Meghan showed in her earlier presentation, the EPRI program back in 2004 when we first started getting into a strong interest in looking at this, what I call the igneous event analysis because we're concerned both with extrusive aspects of that that might occur or intrusive effect is where the impact is to bury waste packages that get to the surface. We assembled and began looking at a team to build around, not just with igneous and volcanological experience, but also people with materials background, people with risk analysis background, people with biosphere backgrounds and so on, trying to look at overall what could be done in terms of the analysis on this scenario.

Now generally, we've heard a lot and we're certainly endorsing a risk-informed approach. That's always been our approach to this. This is obviously in the regulations and a hallmark of the NRC. We also believe in a systems analysis where we're trying to organize the various different disciplines and aspects of this repository system and events that can happen to it over time.

I want to take a top down structure. Often in a lot of programs that I work on outside of the U.S., they focus on sort of very narrow scientific issues that become very important to people, but need to be placed into a larger performance assessment structure to get back to this idea of what is their risk importance. And we've heard that mantra repeated by I think everybody presenting here.

Our view is that we also wanted to include all the relevant disciplines, not just volcanology. Again, when we start in 2004, the issue of the igneous event at Yucca Mountain had been studied by the Department of Energy, the Nuclear Regulatory Commission for 25 years or more in that area and one of the things we wanted to particularly embrace is all their fine work on volcanology, but try to take it a little further in terms of including the other parts of the repository system.

We wanted to set up and evaluate all of the risk-important processes and characteristics that we thought were inherent in the system. We focused very much on considering what were appropriate analogs for the fitness of the particular process that was trying to be simulated.

I want to mention that analogs, we've heard a lot about the geoscience people using analogs and often saying we want to look at a live snapshot today or this snapshot, this part of the process in time. It's the same with materials people. We don't always have full information and in the same way that geology people rely on analogs, the material people rely on analogs.

I think that gets back to a bit of Art Montana's talk today on materials that the full information isn't available on certain alloys, let's say Alloy 22 doesn't go up to high temperature. One of the things we've tried to do is build in use of analogs, fully referenced, proposed. Of course, these kind of things can then begin the discussion about is that an appropriate analog, but there's a great parallelism between those types of use of analogs and materials sciences as it is in terms of the way the geological people are trying to use analogs to inform their side of this scenario.

Finally, the whole process is leading down to some, supporting some sort of decision making and for the different groups, they have different decision endpoints that they're responsible for as determined by the legislation.

A terminology we haven't heard much in the last two days that I've heard, but certainly is in the ACNW report is this idea of reasonable expectation, reasonable assurance. As far as I know, those are every bit as important in terms of the regulations, the EPA regulations, the NRC regulations in terms of using mean values and so on in some sort of risk-informed approach. And it's certainly a hallmark of what EPRI does, saying look, these regulations are written with this specific approach in mind. The numerical compliance criteria are written with this type of philosophy in mind in terms of approaching compliance. It's very important to us.

It will be more legible on your handouts than here, but in 2004, when we first sat down and convened this group of diverse experts and we said what can happen? Let's go through this system of an igneous event, sequentially, and let's ask ourselves important questions, things that are going to affect what could happen and over evolution of time in this particular event.

Well, of course, at the top is some sort of series of questions, can the igneous activity be eliminated on a probability basis? A yes or no. Obviously, if the answer were yes, we could all go home and we've heard Chuck say if we moved it somewhere, the answer would be yes, and we could move away.

What was interesting to me in the last two days of both Chuck's and Bruce Crowe's presentations on the probability, compared to the 1996 PBHA, it looks to me like they're going to be separating out this E1 and E2, the sort of event probability and then the probability of does that event actually intersect the repository? I think that's a real step forward and that's exactly -- these set of questions are exactly the questions we set up back in 2004 when we began our work. So we're very pleased to see that sort of additional enhancement, if you will, of the perspectives on the probability side.

The second question here is will the dike intrude preferentially outside the Yucca Mountain block? Yes or no. If you're familiar with the way EPRI has looked at a number of scenarios, not just the igneous scenario, but a number of the scenarios that have been suggested for Yucca Mountain, we find this sort of decision flow tree analysis is a way to sort of sharpen our pencils, focus in terms of what could be factors that affect the overall safety and performance of this repository for a given speculative scenario.

So moving past these questions of elimination on a probability basis, the other thing is we looked at that time in 2004 and said what other questions could arise in terms of the magma behavior, the interactions with drifts, the interactions with packages, the interactions with waste, the possible waste removal by some sort of either tephra, distributed down to the RMEI and then some sort of later remobilization.

When we looked in 2006, excuse me, 2004, in our review, there was a whole series of either, I can't even say there were alternative models being considered. At this time, some of these were null spaces. It's not a question of alternative models, a question of -- the whole issue wasn't even being considered at that time.

So certainly if we skip past each of these or have a model that looks beyond some of these possible, what I'll call mitigating factors that might mitigate the consequences, one can drive yourself to a very initial conservative igneous scenario. Now maybe that makes sense. That's where we start often with analysis is in the absence of information. We're pushed to conservatism and we have to look at, all right, maybe we're already in compliance there. So that's not a problem to start with the conservatism, but if we're eventually coming back to this perspective of reasonable expectation, reasonable assurance, we can't let the process stop there.

So since 2004, what we've done is asked ourselves a number of these questions here and I'm going to use these questions in looking now at our perspective of what is in the ACNW report in terms of where we find that the views being expressed or at least the text is in agreement with ourselves on certain issues. Also, we're going to try to show where we think maybe there's not additional factors that the ACNW ought to consider.

The other thing I want to point out is that I think it's always dangerous to have perspective in the singular, because the perspective in the singular can be very mistaken for a position. We've always tried to emphasize perspectives, a wide range of views on any one of these questions. Not just this view or this model, but what might be the ensemble of models that might be appropriate here.

So it's more appropriate when we look at our work, we try to speak of perspectives in the plural.

Let me see if I have any other particular points. I think the other thing, looking ahead, because we're not going to come back or may come back to this at the end, but the point is that in some of the talks and Tim's talks and Britt's talk, I think appropriately they're trying to do this risk significance high and medium and low in some cases. That's good, but keep in mind that possible, it's certainly possible that two medium consequences taken together could be a high significance. So while there's a certain setting of priorities and high and so on in terms of these type of questions, is an appropriate first step, I don't think again we can allow it to languish there and not consider possible other factors, mitigating factors that if taken together, could really change the conditional dose.

The last thing I'll say about this is when we did our initial analysis, on the igneous extrusive event and then the igneous intrusive event, when we looked at these and took all of what we considered favorable factors in our view, we ended up with a dose out of this of zero. The expected dose was zero. Now what we then did was basically turn all these switches that might have said yes to no, sequentially, so we could recapture each step in terms of what difference did it make in terms of the dose that might occur to the RMEI.

And what we did at the end is when we set all of our parts of our model in these perspectives, these questions to zero or to null. We were able to recapture exactly these current, again 2004 safety assessments that we felt were very conservative. So we were able to, in our same models certainly verify that we could capture the same calculated consequences by denying all these mitigating factors, but when we started to assemble them together and we took the ensemble of those, we were led to basically no impact.

Okay, the point of this meeting, the point of the overall talk is to focus on in the ACNW reports, what we felt were key points in terms of -- that we're in agreement with and also key points that we think are maybe missing from the report as it stands now. In some of these, I think we very much agree with what we've seen from the NRC and DOE on some of the missing issues.

I won't talk in great detail here. This was presented by people far more knowledgeable than I on this idea that in the dike intruding preferentially outside of the Yucca Mountain block or the emplacement drifts, in addition to the event probability, there were things, very minor things such as maybe the topography. That's certainly touched upon in the report, but also maybe more important structural controls as these dikes reach the surface to what degree are they captured and preferentially moved to areas in which there's pre-existing structure or in situ stresses that are guiding that emplacement.

Again, these could be important in terms of affecting the final overall probability of the event hitting the repository, because it's not just the event, it's the event hitting the repository that is of importance for this risk sensitivity.

ACNW is aware that EPRI in a quite different area is looking at one of these issues about how might more fuel, conceptually, get into a Yucca Mountain area repository, looking at features such as stack repositories or group drifts or even extending the footprint as a potential.

Just point out that it may or may not affect the probability if you expand the footprint if, for example, your conceptual model is that there's strong structural control in terms of capturing rising dikes from below than extending the footprint may not necessarily lead to a higher probability of intersection. It's just something to consider that ACNW reports states that as a fact versus again, to me, it's a modeling assumption.

Okay, we just touched upon this. It's sort of explosive dike decompression and can this possible be attenuated. The ACNW report talks to a number of these factors in terms of shock waves. Our analysis again also showed that we don't think that in some ways the occurrence of that was based largely on sort of the boundary conditions of one dimensional model that was being used. Certainly, this idea of pyroclastic dog-leg previous to our work, previous to the ACNW, this -- I've got this spelled wrong -- igneous consequence panel review, not the ICRP.

CHAIRMAN RYAN: I was going to say you've given ICRP a lot of credit.

(Laughter.)

MR. APTED: Well, they hired this guy Mike Ryan who is very good.

CHAIRMAN RYAN: That was magmamite.

(Laughter.)

MR. APTED: Certainly, it's a contentious point and it needs some evaluation and so on in terms of what are the mechanisms that might lead to it and what would be the consequences if it occurs. So it's quite correctly noted in the ACNW report.

Now will magma penetration into the drifts be attenuated? There's a variety of reasons presented in the ACNW report that suggests, as we heard from Bruce Marsh's talk which suggests that the type of penetrations would be very attenuated and minimized. We looked at that, both as what we gave as our reasonable expected value, but we also allowed it to say okay, let's allow this magma to flood the entire drift, what's the consequence.

So again, it's important to try to identify some perspectives on it, but also to allow the consideration of other perspectives in terms of trying to assess the overall consequence here.

Britt, about this, maybe he might know more, or John, in the May Center report of 2005, they talk about this early natural backfilling. I'm not sure whether that's still the Center's position, but the Center was talking about backfilling an order of hundreds of years and then they looked at what is that consequence on possible magma penetration down in drifts. It might be just an additional part of the work that's going on at the center that the ACNW report could at least touch upon. And probably Art Montana would be thrilled to hear about natural backfilling as a topic.

Okay, can waste packages survive contact by the magma? Again, the ACNW report touches upon this. I think I would agree with both the Center and DOE made this point to a lesser degree, that this has a high risk significance. There's not a lot necessarily in the ACNW report at this time and it might be an area to try to consider more of what are the models out there, what are they telling us. Maybe Dr. Montana's work will eventually -- I don't know if he's meeting a March 1 deadline or well.

I think you're properly in some ways open to uncertainties and unclear about what could happen here. But also, I read a certain amount of maybe skepticism of the packages coming apart into this particles of respirable size. So it's not only just is the package breaking and I will point out that Dr. Montana's analysis and basically he called me in terms of we supplied all the EPRI reports to him and so on. His point is he wasn't able to fill the packages ever. I mean he's talking about pinning down the packages and changing the strength properties of the packages, but in none of his calculations was he actually failing the packages. He just got close and that made him uncomfortable in terms of his view. But sort of half glass half full, half empty. He was very much half full versus it being -- anyways, he was very pessimistic about his results, even though none of his calculations that he was able to calculate led to failure of the packages.

But I think additional comments are warranted here in terms of the packages that are in the conduit, in particular, sustained magma contact at elevated temperature, at elevated magmatic column pressures. What's going to happen to those packages over time? Will they over-pressurize and crush? Again, Dr. Montana was basically implying that the load on those packages will lead to a buckling, if you will, of that package and a sealant, I think he used shrink-wrap type of analogy. Or will it lead to some sort of fragmentation into respirable-sized particles? Or might there be some model in between here? I mean this may be Britt's apples and oranges, but the question is is it more like a banana, more like an apple, more like an orange?

I think another question is thermo-mechanical simulations, again agreeing with some of the points that Britt brought up. In corrosion tests of Alloy 22, and we did our corrosion tests up to 1200 degree Centigrade. This is again back in 2004 when that was sort of the perceived wisdom about temperatures of this event.

Dr. Montana actually had it wrong. In our EPRI report, in our studies, at 1200 degrees an immersed Alloy 22 shows no sign up to a month of any sort of pitting or attack.

So again, just some simple simulations that we did in terms of looking at dike impacts up to 10 meters per second, up to 100 meters per second. What is the robustness of that package in its performance? As it heats up, what happens? We've done a number of tests, trying to indicate could there be some mitigating factors here.

Factors that mitigate radionuclide uptake, again, the ACNW report mentions a number of the uncertain factors about this. What happens with the quenching, the magma quenching, of course, seems to be a key theme throughout the ACNW report. One question would be does the ACNW perceive some credible mechanism for waste mobilization into erupting magma in the conduit from packages that are down the gallery, down the drift. Is there some way to draw back? Even if those packages fail, can that waste somehow be re-entrained and then go back up into the extrusive case or should we consider those more as damaged packages for the intrusive consequences?

Minimizing ash, a lot of this has been talked about very recently. Xenolith issues that Neil Coleman mentioned, the relative volumes of ash is something that we paid a lot of attention to in terms of much of the eruptive material actually falls very close to the cone rather than going to 18 kilometer compliance boundary and so on. So we have no particular additional comments in that area.

Again, our initial question about characteristics of the radionuclide-bearing ash. If it were to occur, there are characteristics about that that mitigate the expected dose. Quite frankly, one area we did not spend a lot of effort on is sort of the remobilization. We noted that certainly people who live in volcanic ash fall, the first thing they do is clean up, they just don't simply wallow around and live in their ash that's falling. But we haven't done the very good studies that have come out of the center and possibly now the Department of Energy looking at this issue, partly because in our analysis we are getting essentially no dose out to this compliance boundary anyways. So we treated it in a more stylistic way, abstracted way.

Okay, in summary, we broadly concur with the ACNW's sequential and structured approach regarding this igneous activity at Yucca Mountain and in the report placing reasonable assurance into the context with conservative analysis. We think that's very important to make that discriminative view and to keep that in front of ourselves as we evolve in information, as we improve in our understanding and concepts and so on. If we to -- from conservative analysis and compliance, fine, job done. But if not, then what are the additional factors that are certainly reasonable to consider based on analogs and material science or analogs and geology that help guide us.

Risk-informed performance assessment is essential. Just joining the choir on this. It helps to identify the processes, the assumptions. You see a lot about sensitivity of parameters and so on. We've got to identify the assumptions that are also very critical in terms of sometimes where our fork is between conceptual models.

So it's not just the processes, but also the assumptions behind how those processes are treated and eventually uncertainties in the data, uncertainties in other aleatory and epistemic uncertainties.

I think when side of risk-informed and risk assessment, there are a number of things that come out of that. We can look at performance confirmation priorities, looking ahead. It was mentioned earlier about requests for additional information. I think Tim pointed out one of the advantages of a risk-informed approach is that it will eventually give the NRC staff who was reviewing some idea of what are additional performance confirmation issues or topics that they have.

We look at design options to the degree that they exist and eventually also, sufficiency of data. When do we stop collecting enough data and move on to the next topic in terms of closing out some of these? There will always be, there will never be perfect understanding, but sufficiency of understanding so it certainly would be a reasonable goal to keep in front of ourselves as well.

A lot of talk of Lathrop Wells. It's certainly a reasonably representative analog. It's not the single most perfect to the exclusion of other analogs, but certainly since 2004 in the Nicholis and Rutherford paper, that's become more and more sort of the agreed focus among all of the stakeholders in this area, if and I should underline and bold this word if. If the judgment is that further study is warranted regarding the compliance, regulatory compliance, what EPA has proposed, probability weighted, mean, annual, peak dose rate criterion, if that is the judgment, then I believe that the greater assurance of safety is more likely to be gained by examining event consequences, rather than further refinement of event probability, of course, following the completion of the PVHA update.

The reason I make that is twofold. One is that we heard yesterday from Bruce Crowe and I think Chuck may dispute me on this, but that the -- we're getting to the limits of data limitations meaning we're not able to further reduce the uncertainties and the spread of our estimates on the probability.

When we look at the consequences, the consequences could in some cases, with all these factors considered, go to zero. That's quite an improvement. And so that's why we say that looking ahead, if further study is warranted, that I think today's discussions, we've heard a lot of suggestions about work that could be done, factors that are high, risk sensitivity that either by judgment now or by calculations are already shown to be the case. There's considerable safety margins to be gained by looking at the consequence side.

Lastly, there was a recent January review of the EPRI analysis by the NRC and the Center. I want to thank Neil Coleman who is not here, but he made us aware of it last week which we otherwise would have not known it was out there, but it's certainly of interest to us. I haven't had a chance to look at it, except last night I just skimmed it. We certainly want to address it and welcome an opportunity to come back, ACNW and sort of talk about it.

Three things strike me about it in terms of some of the comments. One is that some are sort of distinctions without a risk significance, that if you actually look at the difference, it won't make a difference to the actual risk calculations that we made. Secondly, when we look at our train of analysis, where we basically look at factors and then turn them off sequentially, a lot of sort of the concerns about certain factors are alleviated because we move on and say okay, assume this factor doesn't occur. So I think again some of the concerns about this are overweighted in terms of we're not necessarily relying on any one factor to say that's it. That's the thing that will make the signal-only difference that's important.

I think the third thing that's going to be very helpful for us is the ACNW report itself because it looks to me like many of the technical issues and disputes that are in some of these comments here are actually supported in the ACNW report itself. So we're certainly looking forward to using that as part of the type of response that we could develop to these comments.

Thanks very much.

MEMBER HINZE: Thank you very much, Mick. Very meaty and logical presentation. Thank you. Comments, questions.

Mike?

CHAIRMAN RYAN: Mick, thanks very much. And just on your last slide and your last point, I'm guessing based on the timing that you're probably in the midst of your review of comments on your report.

MR. APTED: I am in the midst of talking to you, but tomorrow I'll be in the midst of looking at that, yes.

CHAIRMAN RYAN: In the generic sense.

MR. APTED: Yes.

CHAIRMAN RYAN: But it might be helpful to think about some sort of follow-up with you as you react to any comments you've received because that might help us understand again and I'm in the spirit of thinking about what Britt and others have said earlier and Tim and so forth. And if we can explore where we got -- your thinking right or maybe we misunderstood or NRC's got some useful and different views, that would be helpful for us to hear. So I might extract from the thought that you might come back and tell us about that, hopefully, relatively soon.

MR. APTED: We're planning to respond, both to the ACNW report in terms of typos and other sort of detailed analysis by March 1st and we'll include a preliminary look at this, but it has to be preliminary given the time.

CHAIRMAN RYAN: In fact, we might have to ask you to come and present it in this form so we can actually receive and look at it. So if we have it in writing ahead of time, somebody will have to tell me if that's okay or not, but hearing from you and interacting with you frankly on it would be real helpful to us at some point when you're ready to do that.

MR. APTED: Sure.

CHAIRMAN RYAN: Ruth?

MEMBER HINZE: Thanks again, for a very informative presentation. I wanted to ask you a couple of years ago when EPRI made this similar presentation to our Committee, you had concluded that there would be no disruption of the waste package. Have you looked at or are you planning to look at what might happen way down the line when theoretically the waste package would be gone, corroded away, whatever?

What if the igneous event occurs then?

MR. APTED: Much later time. Well, actually, in our models and I think in maybe some of the more modern DOE models on the corrosion of the Alloy 22, those packages that survive that sort of early thermal pulse and so on, if they're experiencing just general corrosion, it certainly occurs and it's sort of written into the new standard, take it out to a million years, considering general corrosion. But it's very, very slow. So these packages out at 800,000 years in our models, we only have about 10 percent failing in the first million years.

MEMBER WEINER: Thank you. That's very helpful.

MR. APTED: A lot of them are very robust still and based on our models.

CHAIRMAN RYAN: Allen, John, any other comments?

(No response.)

If not, thank you very much. Mick, appreciate it very much and John and his group.

With that, we turn to the last presentation of the day and Engelbrecht deferred to Gene Smith and so I understand from Gene that he has a few comments to make.

DR. SMITH: I just have a couple of comments. I'll try to do this in the old-fashioned, pre-PowerPoint style.

(Laughter.)

MEMBER HINZE: One of the things, if I may, one of the things, if I may, would you come up to the front? If we throw at you, we're going to hit Chuck.

(Laughter.)

One of the things that I personally would appreciate and would help us hearing about and would help us in the white paper is the change of heart regarding the isotopal composition that you and Neil Gadowski presented back about 1995-1996 in an JHR paper in which you indicated a difference between the lunar crater revelry range and the Yucca Mountain.

Clarify that for us, if you would, please?

DR. SMITH: I'd like to make a couple of points and try to clarify.

MEMBER HINZE: You're going to have to have that volume turned up. My hearing is not the best, but there are people that are even further away.

Why don't you do that, use the microphone at the desk, unless you want to wander?

DR. SMITH: How about this? Is this better?

MEMBER HINZE: That's better.

DR. SMITH: Okay. I'd like to make a couple of points. I'll try to answer your question. One of the things I'd like to say is I'm going to provide you with some written comments.

MEMBER HINZE: Great.

DR. SMITH: With specific places in the white paper that I would like to see changed or errors or whatever. However, in general, I think it's really important that -- like I said yesterday, that the petrologic model that we choose is really important in determining what the volcanic future for Yucca Mountain is. It's very important for the probability studies. And also very important for consequence. And I noticed in the white paper various models, the deep melting model is given, is mentioned many times and the more traditional model that has been used for many years is also mentioned, but the reason why it's important to select a model, to use a model is not really emphasized.

And I think it's very important to bring out the point that depending on the model you choose, there's a different volcanic scenario for the future of Yucca Mountain and whether you choose a deep melting model or a shallow melting model, it's important that you select a model because that's going to choose a direction that you take in terms of probability modeling. I think you have to take a bottom-up approach. You have to look at what's happening at the source. And you simply can't look at patterns. I think you have to input geology into the models.

It's really important not to forget the really detailed fuel work that's been done and all the really detailed petrology and geochemistry that's been done, that has to be input into the model. It cannot be forgotten. So I think that has to be emphasized in the white paper.

Also, ideas change with time and I noticed that eery time the deep melting model that I proposed is mentioned in the white paper, not every time, but most times it's countered by saying well, on the other hand Smith 1995, along with Yogazinsky said this and guys, give me a break. Don't use my own work against me.

(Laughter.)

If you want to counter, if you want to provide a rebuttal, Frank Perry in an EOS article, I probably shouldn't even mention this, but Frank Perry in an EOS article, I believe it was 2006, Frank?

MR. PERRY: 2006.

DR. SMITH: Yes, in 2006 had a very interesting discussion of my EOS article which I published in 2005. So if you want to provide a rebuttal, if you want to provide a counter-argument, use Frank's EOS article, rather than my own work to put down the deep melting scenario.

(Laughter.)

I still believe in that work that was done in 2005, in 1995, I think the Yogazinsky and Smith paper which proposes the Armagosa Valley isotope province is still a valid concept, but my interpretation of the isotope province has changed with time. So I just wanted to mention that also in that same regard, Greg Valentine and his colleagues have published a couple of very nice articles on crater flat and the lake of wells cone. I believe 2006 and 2007. But I know it's difficult to go far back and I've been doing work in this area for a long time, but back in 1994 in Earth and Planetary Science letters, myself along with Tim Bradshaw, published an article on crater flat volcanoes. We discussed the geochemistry and the geology and came up with geologic maps of Red Cone and Black Cone.

Greg, in his article, mentions our article many, many times. However, he disagrees with some of the interpretations. But I noticed by going through the white paper that the 1994 article is not even mentioned, but up until Greg's article, it was probably the only description of the crater flat cones.

Also, there's a geologic map produced by the Nevada Bureau of Mines by Jim Falls and I forget the co-authors. It was published in the early 1990s, which has a good description or a good representation of the geology of the volcanoes in crater flat. And my reading of the white paper, I could not find a reference to that either.

So I think there's some work that was done back in the late 1980s and the early 1990s that really has to be cited in the white paper.

I guess the last thing I want to say is I'd like to see the white paper emphasize the need for additional data. Chuck made the comment that we really need a good -- we have to really improve our geophysical knowledge of the area and I think in terms of testing the deep-melting model, we need some good seismic tomography. That might not be something we can do as part of this project, but I think the need for this data is something we really need very badly.

Also, there's been some really interesting core taken from the buried volcanoes with buried basalt and I've seen very little information about the age of this, of the buried basalt, the chemistry of the buried basalt. This information is very important in terms of developing the petrologic model and I think we have to realize that this information is forthcoming. DOE has collected samples. I've collected samples, but this information may not be available for many months and it's very important that this information be eventually incorporated in a white paper.

Also, something that I'm personally interested in and that is the Yucca Mountain, the volcanoes around Yucca Mountain including the buried centers are very close to another volcanic field in Death Valley and I think I showed, I believe I showed in my talk yesterday that the southernmost of the aeromag anomalies is less than 12 miles south, is less than 12 miles north of the Death Valley field. So it's possible that the Death Valley field may, in fact, be part of the larger field that encompasses Yucca Mountain, but we don't know anything about it.

There was some work done back in the 1980s, some work done back in the 1970s, but as far as I can tell and I might be missing something, there's no modern geologic work done on the pleioscene perternary volcanoes in Death Valley, something we need to know about.

I think that's essentially all I have to say at the present time.

MEMBER HINZE: Thank you very much, Gene. Appreciate it.

Any comments, questions? Let me ask you a question, if I might. You brought up the question of data. One of the things that has kind of bothered me is the fact that we only have one seismic line across Yucca Mountain and I'm sure it was done very well, but it's strictly two-dimensional and in today's world that wouldn't even rank consideration.

And yet I've heard over the last two days the importance of looking at the structure in terms of the occurrence of volcanoes and it's controlled upon the movement of volcanos. And we look at Tom Broker's paper and its beautiful interpretation of the seismic, of the faults of the seismic data, but I venture to say that there are several geophysicists in this room that would interpret that data in a considerably different manner. It doesn't even come close to being a unique answer.

And this is really critical data. If you're going to understand the tectonics and therefore you're going to understand the seismicity as well as the occurrence of volcanos you have to have that structure and you have to be pretty close to being right or at least you have to know how close you are to being right. You have to have some idea of uncertainties.

And with the current data, I don't think that's possible. And I don't think there's anyone with a geophysical background that would argue with that and if there is, I'd like to take them on.

The seismic topography that you mentioned, Chuck, originally, is -- it's very frustrating. We heard that PVHA-U, a couple of different discussions and they were kind of anecdotal on this with different sets of data and someone showed an additional, I think it was Gene showed an additional set of data from the Wyoming chap, I forget his name.

And so it would almost be better not to have that data because we have conflicting data and that's -- that opens up a range of uncertainty that probably isn't worthwhile.

Thank you, Gene, for giving me the chance to say a few words about geophysics and wave the flag a bit.

At this point we have promised that we would leave time at the end of the day, which we have, for discussion of any items that were not adequately discussed in the last two days. I would like to open it up to that and then what I'd like to do is I'd like to ask the Committee for any summary statements that they have and anyone else.

So first, John Stamatikos.

MR. STAMATIKOS: Mick, I've got a couple of comments, questions on your presentation. The first one is that I just want to let you know we are still working internally on the backfill question, the natural backfill question, rock-fill question. It's just that that's not yet ripe for any public discussion, just so you know that that is something that is an important issue that we're addressing.

The second comment I have is your system analysis. I would just point out that the way that you set up these questions is a little bit biased to lead you to your initial conservative igneous equation because from our perspective, you know the reasonable expectation is a two-edged sword and you could ask a number of those questions in those triangles the opposite way. So for example, are there factors that minimize ash dispersion to the RMEI. We started that question actually the other way. Are there factors that could maximize ash dispersion to the RMEI. So there are some missing links in there that would actually probably get you to, if you had them all in there and the answers were in the right word, might get you to conservative. I do not necessarily agree that it's an initially conservative assessment, simply if all of these answers turn out to be no. I would just make that point.

And then a final third point I have which goes to something that's also in the white paper. Nick pointed out. He cites a sentence in the white paper that says "no dikes have been found in the potential repository footprint at Yucca Mountain."

This is a key observation and it's on page 76 of the white paper. We've commented on this a number of times before. There are two points, I think that need to be considered when you make that kind of a statement. First one is that there are dikes in that block. There's a dike at Solitario Canyon. Now whether or not you consider that to be significant because of its age doesn't change the fact that there's still a dike there. Everybody recognizes it.

The second point that I want to make in relation to this and how this kind of logic has been permeated is that it's true that in the ESF and in the ECRB and in all the bore holes that have been drilled at Yucca Mountain, no dikes have been encountered. But first point we all know looking at the magnetics that magnetics are not very good in the area where there's exposed tuff and differentiating between tuff and basalt. So we don't know based on geophysics whether or not there's any basalt dikes in the block at Yucca Mountain.

The second point is you can do a simple calculation. Take whatever good representation of the Yucca Mountain block you want at three dimensions, calculate the percentage of area that you actually can see and touch from the bore holes, from the ESF, from the ECRB and it's less than a tenth of a percent. So the direct observations of rocks at Yucca Mountain that you can actually see, touch and smell will tell you that there are no dikes at Yucca Mountain, constitute less than a tenth of a percent of the total volume of rock, even though it's exceedingly well penetrated, lots of bore holes, lots of observations. The truth is that we still have seen very little of the actual rock that makes up Yucca Mountain.

That's all I have.

MEMBER HINZE: Are you saying that if we had the results of the EM study that it might help?

MR. STAMATIKOS: Well, I'm not convinced that that EM would have worked given what I know about susceptibility of the material. I just think it's unfortunate that we have remnant, strong remnant magizations in the tuff and strong remnant magizations in the basalt and so in those areas where there is a lot of exposed tuffs, they just mask any possibility of being able to see the basalts using magnetics.

MEMBER HINZE: The fault at mountain wastes do that.

MR. STAMATIKOS: Yes.

MEMBER HINZE: You're absolutely right.

Britt?

MR. HILL: Britt Hill, NRC Staff. I just want to provide a very quick clarifying point to what John has just said.

Even with all that being said, there's still been no model or evidence presented that the presence of a past igneous event is a precondition for future activity. In fact, what we see is that events, past events in the Yucca Mountain region are coming up with no regard to a specific five square kilometer having activity in that area in the past. There is clustering. There are, of course, tendencies, but in the absence of past activity in a given five square kilometer area in itself has no demonstrated meaning for future activity. It's not a Bayesian condition that has to be satisfied before activity could occur.

So I just want to make sure that we're not giving the impression that additional work is needed to gain confidence that past events may or may not have occurred in that specific repository block.

MEMBER HINZE: Let me throw out something there that I hope will provoke a little discussion. When I came into this yesterday morning, I thought that probably the most provocative topic would be that of the viscosity of the magma, that is associated with an eruption. And I've not been disappointed in terms of that, but what I have heard here and I think we haven't carried it far enough, what I have heard here is that there is kind of a movement towards a 10^6, 10^7 values and I'm wondering if that kind of thinking -- I've got to go back and mine the transcript, if you will, but I'm wondering if Bruce, Steve, Andy, Britt, anyone else, could help us make certain that we're on the right track with that. And I guess the second aspect of it to me is are we approaching a point where the differences are not risk-significant? I think that's a very fundamental question. And I don't mean to exclude my former friend, Greg there, but he too should enter into this.

Could -- I don't want to raise this whole thing, but let's not be too buddy-buddy here. This is a chance for us to hear each other and I'd like to know are we coming together? Is that just trying to make certain you don't stiff my next proposal, but are we really coming together and what does that mean from a risk standpoint?

I guess I'll ask Steve?

DR. SPARKS: Thanks, Bill, for raising that issue. In the spirit of what you suggested, I think there's a very good published, scientific data evidence, experimental measurements, both in the field and the lab which suggest that the viscosity range for the magma underground under most circumstances of magma will be in the range of something a few tens of pascal seconds up to about 10^5 pascal seconds. It's very difficult to go above 10^5 pascal seconds. Certainly agree with a number of speakers, including Bruce that, of course -- and Greg made this point too that as lava propagates across the surface, once it's erupted it cools and crystallizes further, you can to much higher viscosities, but as far as the magma repository interaction is concerned, I would make the statement that there's some -- the viscosity you would expect, based on the current scientific knowledge, as well as direct measurements the viscosity has been in a range of maybe 10 pascal seconds at the lower end of things, up to 10^5 pascal seconds. That's what I would state.

CHAIRMAN RYAN: Bill?

MEMBER HINZE: Greg Valentine from DOE, would you like to give your opinion? Where does DOE stand on this?

MR. VALENTINE: Yes, I agree with what Steve just said and as Eric Smistad mentioned in his talk, we are in the process right now of doing modeling of magma flow integrates with heat transfer and invection-coupled and temperature-dependent viscosity. So we're refining the work that's out there. We're not ready to talk about it yet. I just got the first cited calculations on Friday and I haven't had a chance to really go through them yet. So you know, I would point out that these volcanoes are very complex. We all I think are raised as volcanologists with this view that a scoria cone is a very simple thing. I've referred to it in the past as a lot of times people do sort of -- instead of drive-by shootings, they do drive-by physical volcanology. They say there's a scoria cone at Strombolian and that's -- in reality when we look at these things we see, for example, at Lathrop Wells and this is consistent, not just at Lathrop Wells, but we've done detailed studies at all the Quaternary cones now and these are either published in the literature now or will be in the next few months.

They show a range of activity that indicates everything from sort of very, I would say viscous with tightly coupled bubbles or bubbles that are tightly coupled to the magma, driving explosive activity that gives you sustained jets or eruption columns, perhaps a few kilometers right. We see that type of behavior.

We also see behavior where the melt and the bubbles are able to segregate and you get large slugs of gas that erupt out and what's traditionally called a Strombolian eruption that throws out coarse material that's very fluid spatter when it hits the ground and it welds together. And then we also see these lavas. I would point out that these arguments about the lavas and that they're aa and that they didn't go very far is an indication of their viscosity.

One of the things that's been missing is a well-known relationship where the effusion rate is really the primary control on the distance that the lava flow will go for these types of lavas. These are not blobs that are set on the ground and then spread radially under gravity. They fuse from bocas, around bases of the cones. We think there's evidence that they pulse, that there's many different flow units that are stacked. There's components of internal flow and both the flow length and the textures, aa textures are very closely tied to effusion rate. And that's something that hasn't really come out in the discussion

MEMBER HINZE: Thank you very much, Greg.

Bruce, please.

DR. CROWE: The important thing I would like to say in terms of all this and in terms of Greg's mention of the modeling stuff is that the idea and like Steve is mentioning starting at 10^1 poise, for example, that's a deep system and as you get up near the surface, we go through these transitions, 10^5, 10^6 or wherever we are. These kinds of things, how the problem is set up when you're doing the modeling, DOE is doing modeling where crystallization and heat transfer is involved, the initial conditions of how it's set up is really important in this transition. This happens to be a very, very critical region. There's a 300 meter area.

So the key is not necessarily just to take something that you start out with viscosity of 10^1 and you let it go down. It's the whole process coupled together of how it gets there and where it's coming from and if you start out with something with 4 percent water in it, be true to the system. Start out with it at proper depth where it is under-saturated and then bring it up and see what it's like in a dike-respective size, et cetera, and see what that's doing for cooling.

Degassing is a terrible problem. We don't really know how it exactly happens, but evidently it does happen and so the key is is then the prestaging of the lava or the magma before it goes into the drift is very important. There are two very important characteristics. Not only is it the viscosity, but it's also the driving pressure, enormously important.

And also the crystallization then, just as soon as it gets in there in terms of how it locks up and things like that. So these are a couple of problems, so it isn't just bringing it up and shooting it in there and worrying about. And also, the periodicity is very important, as you just mentioned, it's not just a flow that's actually turned on and just let go. These things evidently, you know move for a bit. We say the same thing in the sills, we can see actually internal discontinuities that they actually work for a while, almost like a large volcanic system. They go into repose for a bit and they work again and come back and forth. So they inflate back and forth, some of like what Steve was showing too. And these things have to be incorporated into the model somehow.

What happens often, of course, in numerical modeling we tend -- these are hard to put in and they've got to -- you've got to actually get the geology into it and that's difficult. There's a gap in terms of conversation between the people doing the numerical modeling and the people putting the geology into it. So this I would really emphasize, but I would also think that we are getting closer together I think here. I think Steve and anybody -- we're getting better and we can do some better modeling of flows instead of just a viscous blob. The geology really does help. We can model these things and there's channel flows and we can look at these.

MEMBER HINZE: And would you go so far as to say the differences probably are minimal in terms of risk significance?

DR. CROWE: That's a bigger question. I mean it's hard to put it on just one single number. This is an integrated problem, so you want to look at the periodicity of the flows and all of these are together. It would be nice to see when you've got a bunch of these results, these newer, I would say kind of higher level results, then you can actually maybe answer those questions.

MEMBER HINZE: The point is well taken. I'm looking around. Is there anyone else?

MR. MELSON: On this issue or any issue?

MEMBER HINZE: On this issue right now.

You don't want to say anything?

Britt or John Kessler, Mike, okay. Thank you very much.

I think that helps me and again, I think this will be useful material for us.

Other over-arching issues?

MR. APTED: Just a quick response to John's earlier point and I think it was a good one about indeed, we have set this up in a very -- simple, sort of decision tree diagram and the yeses and nos, purposely are trying to get through all very high level message of trying to consider additional factors. And you're right, some factors actually that were ignored might be adverse and again, that points up that in the absence of looking at alternatives we're sort of hiding possibly the real response and it could be significantly change it one way or the other, so I think the important message we're trying to is not that all factors have to be necessarily favorable and I'm not saying that, but I'm saying in the absence of trying to identify factors, we can sometimes mislead ourselves of what could potentially be important.

I think the other reason that I tried to put it in that sort or structure is in a sense of transparency, sort of a communication in the sense of trying to organize the discussions that can be wide ranging over a wide range of fields to allow different people access to the same conversation that we have. So I think in the same way and somebody mentioned this about Kevin Coppersmith. I think one of the new updates of their probability is they're going to have a way to sort of transparently show what are the factors, the experts are considering, what weights they gave to them, all that will be to the benefit of everyone, sort of understanding why there are these ranges and probabilities because it will be a much transparent type of format, if you will.

MEMBER HINZE: John Kessler, I believe you had a comment?

MR. KESSLER: Yes, it was on a different thing that John Stamatikos said. John made a comment about how apparently only one tenth of one percent to the volume of the rock has been looked at directly and from that, there's been no evidence of dikes.

What does it take, John? Does DOE have to dig up the whole darn mountain and then you'll be happy? I mean the question is how much evidence is enough here is my concern.

MR. STAMATIKOS: That wasn't the point.

The point Britt made is the correct follow-on. We're not asking for that at all. But the point is that this particular line of reasoning has been cited as the reason why probability can't be higher than 10^{-8} for example. This kind of logic has been used in the adverse without to argue against activity and I just want to point that out that it's, in my view, it's not a critical observation compared to the other kinds of observations that we have.

MEMBER HINZE: Chuck?

MR. CONNOR: Just following on that again, you know solitary canyon dike is sometimes ignored because it's part of the Miocene, but at the PVHA-U meeting, we had a very protracted discussion between myself, George Thompson, involving Dennis O'Leary and other structural geologists and the point I took away from that conversation was that the structural setting of Yucca Mountain has not changed substantially in the opinion of the structural geologists since solitary canyon dike was in place.

Solitary canyon dike is a gift. It's a scenario from which we can learn about the mechanisms of intrusion, very close to the proposed block, but more importantly essentially into Yucca Mountain. So instead of sort of worrying about whether there's exactly a dike within the footprint of the repository now or not, I think we should pay attention to what were the conditions of injection and of solitary canyon dike at the time since we assume it was basically the same sort of structural conditions, as far as I can tell from conversations and literature, the same topographic conditions, as we see today.

So what's the deal with solitary canyon dike? That's an event that has occurred which I think should have profound -- not necessarily impact on our probability models, but it should be considered in a very, very serious way. It's the key to igneous intrusion into the system.

DR. SPARKS: This may be a very obvious point, but if you'd been at Lathrop Wells 90,000 years ago and you were making a decision about a repository, you presumably would have had no evidence of volcanism in that particular area, like the footprint was on Lathrop Wells and you were 90,000 years ago. You wouldn't have any evidence that there had been volcanism there before.

So the absence of volcanism in this sort of very low recurrence rate isn't really a good argument. It essentially doesn't seem to be a very good argument. That's why you go to methods like

Chuck was describing, sort of probabilistic analysis informed by geological or geophysical models.

MEMBER HINZE: Is there any feeling about what kind of surface manifestation we might expect from that dike and this was triggered by your comment Chuck that the topography isn't much different than it was then.

What kind of eruptive scenario might be associated with the solitary canyon dike?

MR. CONNOR: I would love to walk north along the solitary canyon dike because on the PVHA-U field trip Dennis O'Leary said yeah, there's near surface spatter associated with the solitary canyon dike, so apparently people have found vents there already and of course, they're deeply eroded like the Miocene and Southern Crater Flat. The cones are gone, but you still see the spatter associated with those events. So there's probably more information to be learned there.

I've never actually walked up there. I don't know if anybody else has in the room, but it looks, at least anecdotal -- I've got anecdotal information that it vented and people can even discuss where those vents were.

I also know from the past that at the prow area when the dike was excavated, it was something like two meters wide or something like that in that area, so along these dike systems, you sometimes see what Paul Delaney referred to as buds. They're sort of incipient conduits developing and maybe there was one at Prow, but I don't think that outcrop exists any more. I think it was covered over.

MEMBER HINZE: Mick?

MR. APTED: Chuck and maybe Steve and others actually, in some ways, the solitary canyon, let's take that and the dike there. One of the other sort of implications then of it that the dike is structurally controlled within the mountain and if they put the repository and don't cross such large noticeable map faults and again, we're sort of avoiding direct intersection with drifts. Is that a reasonable hypothesis to develop from that?

MR. HILL: Britt Hill, NRC Staff. I think again, I'd go back to the conceptual model that Greg Valentine briefly mentioned, that it really depends on where the magma is rising up. And if it's intersecting a structure, pre-existing structural and favorable orientation for capture, localization, it could well happen. But certainly we see, for example, out in Crater Flat the lack of magmatism along the

Bear Mountain fault. It's coming off on the hanging wall of Bear Mountain. So I think again it's a very complicated picture of if magma were hypothetically rising to the east of Solitario Canyon fault in the footwall, water wouldn't be captured by a pre-existing structure or would it be responding to the existing deviatoric stress at that time.

I think these are good questions to ask, but again --

MR. APTED: Sounds like a good alternative conceptual models.

MR. HILL: Magma capture on structure, it's not like the faults capture all the magma all the time.

MEMBER HINZE: Bill?

MR. MELSON: I just wanted to make a few comments and be presumptuous here and maybe two things you might want to emphasize in your summary.

MEMBER HINZE: Please.

MR. MELSON: One is I've been following the PVAH-U. I've been to all their activities and it's been -- it's quite a process. I mean really, there's nothing quite like what they're doing. I'm very impressed and I do hope the transparency is maintained because there are a lot of issues. Some people are using tomography. Some people are using George Thompson's stress model and that influences their estimates of disruption and what not, so if it's transparent and I assume it will be, I would say that's the big step forward is getting that number and having that be considered an extremely important thing. That's coming along. It's an expensive process. It's taken a lot of brain power and a lot of work and I can't imagine and maybe I'm biased here, doing it any better than this. I can't imagine an individual doing better than this or pooling the experts.

But the second thing is then looking at that and see where you go from there, but if we, in fact, have a probability that indicates you have to go forward, what I'm impressed with is I think the magma properties are all interesting and all of this, but the main thing I'm concerned is the magma waste package interactions and in particular, what happens to that waste?

We've had lots of different points of view, all the way from creating the dust to heavy high temperature melting uranium oxide pellets that are just going to get encrusted and just sit in the magma and sit in the lava or the tephra deposits.

So I think that, to me, is the most important thing. And is it risk-oriented? It can be approached and I think Montana and other people have mentioned these things. I think his comments about he really would like to see some real tests done, I assume canister-sized vessels, but carrying that forward as far as possible I think would be really very useful.

The third thing is a process-oriented thing. I think Mike has talked about this, but how do you assess alternative hypotheses. I mean that's what we're all concerned about. We have these different hypotheses. How do we actually do that?

I don't have the answer to that, but the formal addressing of that problem is definitely a way forward. I think the NRC especially is going to have to continue dealing with that.

MR. MARSH: Let me mention one thing in passing, just a little calculation. If we took a sphere, just from judging from John's comment about the inspection area, if you took a sphere, one kilometer and radius and held it up and were able to walk around, look at the whole outside of it down to a depth of one centimeter, you'd only see 3^{-3} percent. So it's trivial kind of thing, but however, it just gives you your perspective that there's a lot of it that you don't see, of course, interior, but --

MEMBER HINZE: Well, we -- please, Eric.

DR. SMISTAD: Just a few comments here. Just maybe one correction of fact and mixed talk. We have had a need to include it in our work before, the eruptive probability. We just didn't elicit it. This time around we are eliciting it. Okay, so just a point of clarification.

And on the topic of waste package damage, we kind of heard a couple of different folks, Art and Nick to some extent, talk about the damage to a package and intrusive case under these magmatic conditions. Just a reminder, I think these guys know this, but it didn't seem to come out to me in this discussion that there were not, we're not talking about a coupon and a drip. we're not talking about two pieces of metal laying on top of each other and a drip. We're talking about a package. It's got endcaps. These endcaps are a fix and a welded fashion. There's internals to this package. It's just not material in a drift, so it may or may not change what analyses these individuals have done, but I don't feel that that didn't necessarily come out in today's conversation.

And I think just one final one, if I might, Bill, just on the topic and I think Bill just had mentioned it here and I don't know if it came up in another talk or not, but when you're looking at the situation we've got here and you're looking at different analyses and you approach an analyses, say you have a process like the eruption of waste, there will be a conduit. It's a very complicated process. It's not data-rich. You're forced with making perhaps simplified assumptions. Some might call them conservative.

But if you do that and propagate it through your TSPA and you get doses that are extremely low , at that point as a project manager, you have to sit with that piece of information and make a decision. Do I try to continue to go get information, and do testing and spend money when I've got the risk from the analysis, the DSP that's very, very low. So these decisions that you face when you're doing these pieces of analyses and I think it's something I wanted to out the committee.

MEMBER HINZE: That viewpoint is lost occasionally.

Please, Neil.

DR. COLEMAN: I wanted to, in fairness to the white paper respond to a number of the comments that Britt Hill made earlier. I believe it was the third conclusion, had to do with risk insights and sort of an expectation. The staff would like to see more in the way of risk insights.

Some of the things that we were talking about today, such as the fluvial redistribution. None of that was addressed in the risk insights baseline report that the staff had prepared. This is based on model runs. The version of the code did not take that into account.

And we know that's not going to happen between now and when the white paper will come out.

CHAIRMAN RYAN: Neil, just let me interrupt. I think it's important to recognize that if there is something that we don't have and we're going to work on it, we can always say that in the white paper too.

DR. COLEMAN: I absolutely agree.

CHAIRMAN RYAN: Okay.

MEMBER HINZE: In fact, one of the things I have in my notes here is organic white paper. It may be a growing thing.

(Laughter.)

DR. COLEMAN: And I have just one last

thing. My friend, John Stamatikos was critiquing the observation about no dikes being present, none being found in Yucca Mountain and he used, as an analogy referring to the volume that's actually been characterized.

There are very few places in the world that have been characterized to that extent. If you were looking for point objects like a coffee cup buried in the mountain, that would be one thing. But in fact, as the staff had pointed out, dikes are kilometers long, there are very few degrees of freedom to hide a dike in between the tunnels, bore holes and the surface of the mountain.

And I agree with Chuck, who is not here right now, the Solitary Canyon dike is a gift. It is one of the things -- the fact that it was found. That adds to the confidence that if others were present, they would have even found also.

MEMBER HINZE: Thank you very much, Neil. With that, I'm going to ask Dr. Weiner to make some summary statements and then I'll move to my colleagues.

MEMBER WEINER: Well, as a non-geologist -- brief, very brief.

First of all, I would like to thank

everybody who participated in spite of the awful weather. And the fact that you sat through this whole long meeting and I really want to thank you.

As a non-geologist, I can only say that I have learned a tremendous amount. I didn't even know what scoria was when we started this. And I think some very excellent points have been made, especially Eric just made a very good one, which is when you -- when all the analysis is done and the risk is very small, how much further do you go?

I'd also like to as a final thing, go back to something that Britt Hill and John Trapp said about there being the regulator and the regulator's perspective just like the applicant's perspective is a different one.

Speaking as neither a regulator now an applicant, the many members of the public who are interested in this see what you, the regulator say as truth. Whether you see it that way or not.

In other words, they don'[t modified by saying this is a conservative view. And I think it is worth simply being aware of that.

What they are looking to, to us and to you, is to tell it the way we as scientists, if you will, think it is, and recognizing that you are the regulator and recognizing that you have -- you take a conservative view in regulation. Still, I think we ought to do what we can to get at the truth. And I really appreciate the exchange of views. I think nothing is settled with this workshop. That's one very interesting thing.

We have opposing views about the interaction of magma with the waste package. We have a lot of different views about crystallization and the magma, but I thought it was very helpful and I hope that the public sees it that way also.

MEMBER HINZE: Thank you. Dr. Clarke.

MEMBER CLARKE:: I'm not a geologist either, much less a volcanologist.

MEMBER HINZE: You're in the Department of Environmental Geosciences or something or other.

(Laugher.)

You say stuff against me, isn't that right?

MEMBER CLARKE:: I think it comes down to consequences. I think it's been a good discussion and I'm encouraged to go back and forth. I like Bill's observation that we need to find a way to incorporate alternatives and use them in a way that makes sense.

MEMBER HINZE: Allen?

VICE CHAIRMAN CROFF: What's the question?

MEMBER HINZE: Brief, I look forward to the PVAH update.

And the transparency. I think that will be very interesting after having looked into PVHA and I see where they got to, how they got there and what they thought about and what they didn't think about is lost to the miss of time, I guess.

Dr. Ryan, you have the last word.

CHAIRMAN RYAN: Thank you, wow. Well, I'm first of all pleased to know there's another Ryan who is a magma physicist who I can call on. That's a good thing.

Seriously, though, I would like to say I think we'll have a very rich transcript from which to mine information views and opinions and facts and figures and everything that we've heard in the last two days and it will help us, I think, produce a much better white paper. So everybody's participation, those who are still here, and those who have departed, I on behalf of the Committee really appreciate everybody's participation. The High-Level Waste Project Office staff have interacted with consultants for lots of different folks and us and everybody else have really contributed in an excellent way to our information gathering to make this white paper a useful tool.

So sincere thanks all around to everybody for doing that.

One final word, I guess it's more of a mechanical thing. Well, one other comment. Apart from the transcript, I think we have a path forward, particularly working with the staff on identifying a technique maybe and even some key areas where can probably improve our mutual understanding on issues and particularly with regard to the view of a scientist, the view of an applicant, the view of a regulator, which overlap a lot, but there are points of view that we need to be sensitive to and understand and I think we've reached a good communication on those views of where all those three perspectives intersect and where there might be slight differences fora very reasonable and good reason. So that's a very important for us to think about.

And then finally, if anybody needs any help again with travel-checking, with hotels, if your travel plans have fallen through, staff is still here and happy to help you. So let us know.

With that, if there are no further items of business for the ACNW today, we will adjourn the record here.

Thank you very much.

(Whereupon, at 5:00 p.m., the meeting was concluded.)