Official Transcript of Proceedings

NUCLEAR REGULATORY COMMISSION

Title:	Advisory Committee on Nuclear Waste 176th Meeting
Docket Number:	(not applicable)
Location:	Rockville, Maryland
Date:	Tuesday, February 13, 2007
Work Order No.:	NRC-1440

Pages 1-203

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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE (ACNW)

176th MEETING

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TUESDAY,

FEBRUARY 13, 2007

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The meeting was convened in Room T-2B3 of Two White Flint North, 11545 Rockville Pike, Rockville, Maryland, at 10:00 a.m., Dr. Michael T. Ryan, Chairman, presiding.

MEMBERS PRESENT:

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

ACNW STAFF PRESENT:

NEIL M. COLEMAN
JOHN TRAPP
JACK DAVIS

ALSO PRESENT:

STEVE SPARKS

BRUCE CROWE

EUGENE SMITH

KEVIN COPPERSMITH

KEVIN SMISTAD

A G E N D A

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

(8:36 a.m.)

CHAIRMAN RYAN: If I could ask everybody to take their seats, please, we'll go ahead and get started.

(Off the record comments.)

CHAIRMAN RYAN: Come to order, please. This is the first day of the 176^{th} Meeting of the Advisory Committee on Nuclear Waste. During today's meeting, the committee will 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. 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 session. 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 they can be readily heard. It's also requested if you have cell phones or pagers that you kindly turn them off.

I'd like to begin with an item of current interest. Mrs. Sherrie Meadower, who has been with the ACRS/ACNW for 11 years has left the ACNW/ARS to join the commission staff on February 5th, 2007. She made numerous outstanding contributions to support ACRS and ACNW activities. She was an exceptional technical secretary to the office. Sherrie's enthusiasm, patience, and dedication to support the committee and staff are very much appreciated, and we surely will miss her good humor, and hard work, and thank you so much, and good luck in your new assignment. Thank you very much, Sherrie.

I will briefly make a couple of comments, and then turn the meeting over to Professor Hinze, who's going to lead us in the next two days. I want to first start with a note of appreciation. We have a large number of folks here that are participating from the NRC staff, from the center and the experts with a wide range of views on this subject, and we really appreciate everybody bringing those views here, expressing them, and exploring the range of views that we're trying to document in the White Paper. I especially want to compliment the NRC staff that have interacted with us in an ongoing basis; one, to develop this meeting; and two, to give us feedback.

And we really appreciate the feedback that we've gotten, and I just wanted to start on that note, that everybody here is really contributing, and we really appreciate it. It's going to help us do a better job of documenting the range of views on this important topic, and presenting that to the commission. So without further ado, I'll turn the meeting over to Professor Hinze.

MEMBER HINZE: Thank you very much, Mike, and we appreciate those comments. For the record, it is my pleasure to welcome you to the ACNW's Working Group meeting on the Igneous Activity White Paper. We realize that this is a very busy time for many of you that are participating in the working group, because of your role in preparing, and preparing for the license application for the reposed repository at Yucca Mountain. All of you have overburdened schedules, so we are grateful for your participation and interest in the objectives of the working group. We especially want to thank those of you who have prepared presentations. We are well aware of the effort that it takes to prepare these kinds of talks.

My introduction of each of the speakers will be limited to a brief statement of affiliation. I will apologize for that now for that limited introduction, but let me assure the committee and the audience that each of the speakers is a first rate expert in their subject matter.

Before we begin, I want to say a few words about why the ACNW is holding this meeting, how we intend to conduct the meeting, and our vision of what we will achieve for the NRC as a result of the meeting. Roughly, a year ago, we've all heard this before, but roughly a year ago, the committee received a request from the Commission to, and I quote, "Provide the Commission with an analysis of the current state of knowledge regarding igneous activity, which the Commission can use as a technical basis for its decision making." That's why we're here.

In response to this, the committee embarked on an effort to prepare a White Paper that would capture, as Dr. Ryan has pointed out, the full range of current views pertaining to the potential risk from igneous activity at the proposed repository. An initial preliminary, if you will, draft of the White Paper was completed two months ago, and distributed for review and comment.

The White Paper hopefully presents the ACNW's summary and evaluation of the principal views of the committee, the NRC staff, Department of Energy, State of Nevada, EPRI, and other stakeholders on the nature, likelihood, and potential consequences of future igneous activity at the repository. In its final form, we envision that the White Paper will summarize the principal views on igneous activity, and highlight key areas of scientific agreement and disagreement, and the basis for these disagreements.

We have worked diligently to capture all the major current views that are held, but I think you can appreciate this is a difficult task because of the evolving views, and the multiplicity of sources and documents which contain these views. However, it is important to have captured all of these in the White Paper, and to make them current, and to make them correct. This gets to the very heart of the objectives of the working group.

The main issues to be addressed today and tomorrow are, first, has an effective understanding of the various views on igneous activity and their technical bases been identified in the draft White Paper. Secondly, considering the current state of science, have the risk-significant topics regarding igneous activity been identified and addressed? And, finally, are the technical bases for positions that are presented scientifically sound? And if they're not, why not?

Comments on these issues from interested parties during the working group meeting will be woven into, and I promise this, that they will be woven into the revised White Paper for the Commission. This is your opportunity to set the record straight before submission to the Commission and public release of the document.

We look forward to receiving your comments on substantive issues dealing with the content of the draft. It is important that these reviews and comments be linked to specific sections of the document, as much as possible. Hey, give us a break, you know. It will be helpful to us. References to particular supporting published documents and articles in the reviews are important for establishing an adequate paper trail for the comments.

Understand that the current version of the White Paper is a draft; and, therefore, it contains editorial glitches, and they stand out to all of us. They certainly do to me. And even last night, I found another one, so these will be addressed in preparing the final version of the report. If you have suggestions for editorial revisions, we will appreciate receiving them, of course; preferably, later in supporting documentation. We will appreciate written comments and reviews of the draft White Paper, but in the interest of maintaining the rather demanding schedule that we have for getting the report to the Commission, we must have these within the next two weeks; that is, we are looking forward to any written comments by March 1st. Please alert Neil or me if you intend to submit written comments, but that is not a necessity.

In my experience, and I didn't say long, my experience with the ACNW, this is a unique working group. We are inviting, and we may regret this by Wednesday afternoon, but we are inviting scientifically-based criticism and recommendations for improving the draft White Paper. The bottom line to us, and to all of us, is that we are seeking your assistance in preparing the best possible report for the Commission.

In terms of procedures for the working group, the first day is directed toward the first two questions of the risk triplet, what is the nature of igneous activity, and how likely is it to happen. These questions have been the subject of extensive debate for a couple of decades among those involved in evaluating risk from igneous activity.

The second day, we will focus on the consequences derived from igneous activity. There are recognized differences in the views on this portion of the risk triplet based on varying professional judgments. It is important for us to identify these differences, their sources, and if at all possible, their importance to risk.

We ask your assistance in maintaining the separation of the topics to the specified days in your presentation and discussions. This will help members of the audience who will be attending only those segments of the meetings that are of interest to them. I will endeavor to maintain this separation, although I assure you, at the end of tomorrow, we will open the discussion, a roundtable discussion, to all of the topics covered in the working group.

Discussion of each of the topics will begin with a presentation by experts that are established to provide background for the committee and its revision of the White Paper. Following these background papers, we have asked stakeholders to brief the committee on their views of the ACNW draft White Paper. We ask those of you that are making comments on the White Paper to give first priority to those that deal with your point of view, with your views that are expressed in the White Paper. As a second priority, you may wish to comment on the views of others. Any remarks that you can make regarding the importance to risk, and I want to emphasize this, any remarks that you can make regarding the importance to risk of the igneous activity issues will be very much appreciated.

Time for questions to the speakers and discussions of the presentations will be made available as indicated in the agenda. We will have time - we will not have questions during the presentations or immediately after, but after a couple of speakers, then we will open it up for questions and discussion.

After the committee and invited experts at the main table have had an opportunity to ask questions or make comments, the floor will then be open to other experts and public, as time permits. We will have some flexibility in the time in the agenda, both this afternoon and tomorrow afternoon.

On a more personal note, many of the issues we will be discussing are hot button topics that have been subject to strong personal feelings and intense deliberations, and I look at Bruce Crowe to smile. We look forward to lively discussion on these topics over the next two days, but it is important that as we do so, that we maintain our discussions at a professional level, and I'm sure that we're all going to have your cooperation in accomplishing this.

A complete transcript will be made of the proceedings, that will be publicly available from the NRC website shortly after the meeting. We hope that you can use this to trigger any further written comments to us.

With that, I'm going to turn to Neil. Neil, are the Japanese group here? I wanted to acknowledge them. I have not met them. Excellent. Before we begin, I want to acknowledge the presence of our colleagues from Japan that are attending this meeting, Mr. Hayka Tushi, a General Manager of the Nuclear Waste Management Organization of Japan; Mr. Junichi Kuto, Manager of NWMO; and Mr. Hideki Karwar, General Manager of the Oshia Obiyasha Corporation. We welcome you, and we trust that the proceedings will be of significant interest to you.

Finally, I personally want to acknowledge the assistance of the ACNW staff, and particularly Neil Coleman, in pulling this meeting together. Thanks to all of you.

With that, I'm going to ask my --

CHAIRMAN RYAN: There's one other housekeeping item, we have folks on the bridge line, and I think we'll certainly include their opportunity to ask questions in the general question session. And if I could ask the folks on the line just to identify yourselves for the record, and let us know who's there.

PARTICIPANT: We have the Center for Nuclear Waste Regulatory Analysis on the line, from CNWRA we have Roland Benke.

MR. WITTMAYER: Gordon Wittmayer.

MR. PATRICK: And Wes Patrick.

PARTICIPANT: That's all from San Antonio.

CHAIRMAN RYAN: Okay.

PARTICIPANT: May I interrupt to ask for a copy of the presentation materials be faxed to us?

CHAIRMAN RYAN: Yes, sure. I think we can get something arranged. We might even email you an electronic copy and have you distribute it on that end.

PARTICIPANT: That would be fine. If you do need the fax number, it's 210 --

CHAIRMAN RYAN: I'm sorry. Hang on just a second.

PARTICIPANT: Has anyone downloaded it

already? I think we sent it to them already.

CHAIRMAN RYAN: Okay. I think the download to you is in progress. If that doesn't happen maybe in the next little while, you could just break in and let us know that's not happened.

PARTICIPANT: Thank you very much.

CHAIRMAN RYAN: Okay. Is there anybody else on the line? All right, great. Thank you very much. Sorry, Bill. Just wanted to --

MEMBER HINZE: Okay. Excellent. Well, we're almost on time, but with that, we will start the meat and potatoes of this working group, and we will ask Dr. Steve Sparks from the University of Bristol to give us a keynote address, and give us words of wisdom on the state of the science of volcanology. I can't think of anyone that is more capable of doing that than Dr. Sparks.

DR. SPARKS: Okay. Thanks very much. It's a pleasure to be here, and thank you, Bill, for inviting me on behalf of the NCNW. I was asked to give some sort of general oversight about the state of volcanology, and also, I guess in the context of the White Paper, so when I developed the idea of how to present this, I decided that I'd actually abbreviate the state of the science to a very short early section, and then I would move on to some eruptions that have happened, which have analogies to Yucca Mountain, particularly Lothrop Wells, the style of activity in the Yucca Mountain area, and draw some inferences you can make from direct observations of what happens in volcanoes. This, incidentally, is Parucatine eruption, the center cone of Parucatine erupting in 1949, and it's a painting by the serialist artist called Dr. Atl, a Mexican artist.

Okay. So this is an outline of the talk. I'm going to talk very briefly about advances in volcanology and prediction. By prediction here, I mean the sort of short-term predictions, when is the volcano next going to erupt, or what it's going to do, not your long-term prediction. I'm going to emphasize the importance, I think, of case histories. I think really detailed studies of volcanic eruptions have been where most of the major advances in the field have been made. And I'll illustrate that by what I'm familiar with, the Soufriere Hills of volcano in Montserrat. And then, of course, what these case histories allow you to do is to gather a lot of monitoring data, and excellent data, and then you can apply modeling, and see how the models can give you insight into how to interpret that data-rich set. And then you can move on to prediction, or at least understanding the volcanism.

Then in Part Two, we're going to look at eruptions which I think are analogues for volcanic activity in the Yucca Mountain area, and this includes 1973 Eldfell eruption, Iceland, which turns out to be almost a dead ringers for Lothrop Wells, remarkably similar. Then I'm going to talk a little bit about Etna lava rheology, and an eruption of an andosite in Chile, and finish off with a pyroclastic flow on Montserrat, and you'll see what the relevance of these is.

So let's begin with a case study. So this is really more general about the state-of-the-art of volcanology. It's an island of volcano in the Caribbean. This is the volcano that's been erupting since 1995. It's been a fantastic eruption to study, because over the last 12 years this has been monitored in enormous detail, and so we've gained huge insights into how these volcanoes behave. It's a Hornblende Andosite lava dome. Since 1995, .7 cubic kilometers have erupted so far at an average rate of 3 cubic meters per second.

I make a couple of comments which are relevant to the White Paper. We've done some estimates and draft measurements in the laboratory of the rheology of this lava dome, and the sorts of figures that you get are around 10 to the 10, 10 to the 12 Pascal-seconds. And I assure you, this does not look like Lothrop Wells in any way, at all. This is the sort of viscosity of an andosite lava dome, and in the White Paper there's a development that this might be the typical viscosity of lava at something like Lothrop Wells. Well, that's about, as I'll show you later, six or seven orders of magnitude high viscosity than you would expect in volcano of the Lothrop Wells type. And here we see a volcano where, in fact, we've got these sorts of very high viscosities.

I'd also make the point that the minimum crystal content of this lava is 65 percent, and it sometimes extrudes with a crystal content of 90 percent. So, again, just referring to the White Paper, these limits or thresholds on crystal content are quite a variable feast, and you can erupt lavas with extremely high crystal contents.

This is the data, the sort of data we've got. It's just one example, but we've monitored the volume of this lava dome with time. This is 1995 to 2001, and the volcano is still erupting, so we have this record right through to, more or less, today, And this is some GPS data of the deformation. This is a station on the flanks of the volcano, which is going up and down. And what you can see is that when the lava is extruding, the ground is subsiding, exactly what you would expect with a magna chamber under the volcano. And then there's a pause for a couple of years before the activity starts again, and you can see in this period the ground is uplifting, because the chamber is pressurizing. And then as soon as the lava starts to pour out again, the pressure goes down in the chamber, and the ground collapses, so we've got very good data we can compare with models. And you'll also see a very characteristic feature of this sort of volcano, which is episodic activity. These volcanoes erupt in pulses, or sometimes quite periodic pulses, so that's the sort of data one can get.

And then just moving on to modeling - well, modeling is rife in the earth sciences, and certainly in volcanology. And I could have probably chosen 30 or more different sorts of models, so I'm just going to choose one, just to illustrate the point. This is a model we've developed with colleagues in Moscow State University. It's a magna chamber with a magma flowing up and erupting. It's a dome. And the important point, which, again, is some relevance to Lothrop Wells, that during this ascent up the conduit, the magma decompresses, and it degasses, and it crystalizes, and this changes the viscosity enormously from the magma chamber to the earth surface. And this has a huge effect on the dynamics. And what we found through numerical modeling is that we see that it's very easy to get this sort of behavior, flow rate out of the volcano against the driving magma pressure, which is kind of typical of a non-linear system with, in fact, more than one possible eruption state for a given set of conditions. And so, in this sort of system, it's very easy to produce episodic or periodic behavior.

The cause of this episodicity in this case, we believe, is the kinetics of crystallization. If the magma comes out the conduit too fast, the kinetics are too slow, it doesn't crystalize, so it erupts quite - it's a relatively low viscosity, so it can erupt rapidly. That's this upper state. You could say this is the disequilibrium branch. And down here, where the flow rate is very low, then, basically, as the magma rises up, it can go to thermodynamic equilibrium, it can crystalize as it decompresses, and the viscosity becomes very high.

And the system can oscillate between these two in periodic fashion. This is a rather generalized model, and you can see down here, again, flow rate against magma pressure, some more detailed numerical models. So this is - the major point of this, really, is that we can use models to give us insights into data in volcano behavior.

Now these are some of the numerical models, and it's just to give you an example, that rather than anything else, this is discharge rate out of the volcano against time. This is, again, done with our Moscow State colleagues, and the mathematicians. And what we find is that the magma chamber size is the biggest control on the episodicity of the volcano. So we have a small magma chamber of 1 cubic kilometer, and we run these models, we see spikes of extrusion. This is time, this is flow rate out of the volcano. We see episodic activity. If we make the magma chamber bigger, it's got more capacity; and, therefore, the time scale of the cycles of extrusion goes up. And so, again, we can use models to gain insights into how the volcano behaves. And we can also use these same models to look at issues like over-pressure on the magma conduit, and this is depth, this is the earth surface, this is the magma chamber, this is the over-pressure, which we define as the pressure difference between the rocks outside the conduit, and the magma in the conduit. And the different values of the curves here represent different values of permeability of the magma, because the gas is always coming out and trying to escape, and exactly how it escapes depends on the permeability, and this feeds back into the results. But that doesn't really make much difference, the main point is all these models show that we get a very strong over-pressure in the volcano of a few hundred meters below the vent. And the reason we do that is very simple; the magma has come up, it's degassed and crystalized, become much more viscous, and this means all the friction is in the top of the conduit; and, therefore, we get an over-pressure. And we believe that this is why we get shallow near-field deformation, and why we get shallow earthquakes all the time in these andosites, because of this over-pressurization.

So that's a kind of whirlwind tour through a case history. And, really, what I'm trying to get at is doing very detailed case studies, coupled with very good data, and then models to gain insight into the data is a way that the science has progressed.

Now I'd like to turn my attention to

something more pertinent to the Yucca Mountain issue. We're going to look at Yucca Mountain. I think there seems to be a consensus, reading all these reports, that something like a Lothrop Wells, a monogenetic trachybasalt volcano, is the sort of thing that we should be concerned with; and so, I'm going to look at two volcanos which erupt trachybasalts first, and then I'm going to draw some analogies from a couple of volcanoes which are not trachybasalt, but I think we can learn some things. And this is picture is actually the Eldfell Eruption of Heimaey in Iceland in 1973, nice cinder cone and fire fountain jets next door to the town. And I'm going to talk about this one first.

What's the setting? It's Iceland. There's the Island of Heimaey, where the - this picture is before the eruption. This is where the eruption is going to occur. It's a typical monogenetic basaltic eruption remarkably similar to Lothrop Wells in many respects, and it's in a region at the south shore of Iceland, where it's not typical Icelandic volcanism. This is alkaline volcanism in transformed fracture zones, and so it's the sort of low partial melt type of volcanism that we associate with Lothrop Wells.

And this eruption occurred in 1973, and it's -- unfortunately, quite a lot of the information about this is in Icelandic, and I'm fortunate enough to have an Icelandic colleagues in the University of Iceland, who knows a lot about this, and he gave me some of this, augmented my knowledge of this with some information. And this is the chronology of the 1973 Eldfell Eruption, and I'll illustrate this chronology with some photographs a little later.

22nd of January earthquakes, not very well constrained, but appear to have come from something like 20 kilometers depth, 1.6 kilometer fissions opens at 1:40 in the morning, and we get a fissure eruption. The 23rd of January, the next day, the active fissure starts to focus into one place where the cinder cone is going to grow. After two days, 24 th of January, the eruption is its most intense, eruption columns of 8 to 9 kilometers, discharge rates of hundreds of cubic meters per second. But even early on, lava is -- degassed lava is emerging out of the vent at the early stage of the eruption. These things go on simultaneously.

The 26th of January, the fissure lengths into 3 kilometers, but the activity remains focused. The 31st of January, a week later, the cone is already built to 180 meters high, intense fire fountains, eruption rates at this stage have declined a bit, estimated at around 50, 150 cubic meters per second. The 4th of February, lava flows into the harbor, part of the explosivity starts to reduce. It becomes dominantly a lava eruption. The lava has covered 2 square kilometers, and the eruption largely extrusive, but still persistent Strombolian activity. There's a temporary halt on the 25th of February. The lava starts to flow into the town, and the Icelanders start pumping seawater onto the lava front to make it stop, April the lavas flow to the east. Interestingly, on the 26th of May, there's a rather poorly documented eruption in the ocean, some sort of extension of the fissure, a second eruption in the ocean, which nobody really knows very much about, except it occurred in the sea, so a new eruption started somewhere else.

So that's the chronology of the eruption. Let's have a look at some pictures of it. This is the first day, the 23rd of January, the classic curtain of fire, activity all the way along with fire fountains. Very quickly, the eruption focuses onto the cinder cone, within a day, we get this flow focusing phenomena, and you can see at this stage it's pretty explosive, fire fountains in the fissure region, and you can see the ash plume going up.

Just focus on the -- here. I hope you can see this. It's not perhaps as good as I'd hoped, but you can see here, here's the explosive activity building cinder cone, but you can also see a lot of steam, and that's because a lot of lava is already coming out, degassed lava, so very early on. And the extrusion of degassed lava and explosive activity are simultaneous, and there must be some mechanism, very effective and fast time scale mechanism, separating gas from magma. And we don't really -- I should say right at the start, we don't understand these processes very well, at all. The only person who's done anything serious on this I think are the French, and a group, a chap called Yuri Slezin, a Russian, and he thought there was a sort of possibility of an alternation between a fast flow with small gas bubbles, where the gas bubbles and magma don't separate, and the case with slower flows, when these gas bubbles can amalgamate and form big gas bubbles, and then we can start separating the gas from the magma efficiently.

And these are some models which show the flow speed versus a parameter which relates to the width of the conduit, volcanic conduit. And you can see the only important point to notice here, is it shows this sort of same non-linear behavior as we saw in the Montserrat models, so we can get -- therefore, we've got a possible mechanism for models which account for the pulsatory activity, and also, the separation of gas and magma. So, again, I'd just emphasize that we don't understand those processes very well, at all. I think we understand these basalt volcanos less well than we do Mount St. Helen's, and Montserrat, the andosite volcanoes.

The 31st of January, you can see that the cone - this is only after a week - the cone, the new cone is pretty substantial. There's the lava going into the sea. Again, just pictures of the activity, still a lot of ash. I'm afraid it's a bit darker than I'd have liked, but it's February in Iceland, it's a bit gloomy, and there's some pictures of the activity.

I'll spend a little bit of time on this, because I think it's quite important for our discussions. The reason for being interested in Eldfell, is that it is a lava which has remarkable resemblance to Lothrop Wells. This is, I think, from Frank Perry's work. This is an average of, I think, 25 Lothrop Wells trachybasalts. Eldfell is a trachybasalt, and if you scan down these columns, the differences are very minor, indeed. They're both typical transitional alkaline basalts.

Etna is another one I'm going to use, and that's also trachybasalts. And this is an Etna composition, little bit different, but not much. And this is an Etna quenched glass from a lava flow. So these are the -- if we're going to understand something like Lothrop Wells, these are good places to go.

The eruption temperature was measured for Eldfell at 1030 to 1055. Just in passing, I'll notice that in the White Paper, the idea is that these eruptions should be at around 1000, but these calculations do not take into account latent heat for crystallization, so magma that rises up and crystalizes will erupt hotter surface than it does when it's deep in the crust. And so, these increased temperatures of tens of degrees Centigrade are pretty well what you would expect from latent heat effects.

The atomospheric -- one atmosphere liquidus is about 1105. This is Icelandic work, phase equilibria, that's the estimate. It's an Aphyric lava with flow-aligned microphenocrystals up to 40 percent plagioclase olivine oxide. As you know, Lothrop Wells, we'll recollect that's not too different. And it's also got Kaersusite erection rooms on the xenoliths between the magma at some depth, was reacting with xenoliths and forming Kaersusites, so Kaersusites is there. And the inference is that this is a water-rich alkaline basalt evolved with a high water content. And, again, taking the sort of Nicholas and Rutherford work for Lothrop Wells, and it's very similar, so one would infer that, again, we're dealing with high water content, possibly the order of 4 percent water. And these assemblages are a decompression assemblage, because of the rise of the magma. This would be the inference, so it's pretty similar.

This is data on discharge rate with time, and like many of these, wherever you have detailed data on these eruptions, they're not that many, you usually see some sort of broadly exponential decline in extrusion rate with time, so these don't come out at a constant rate. They decline because pressure in the source is declining, and so it's a bit like an oil field, the extrusion rate declines. So this is a very interesting case, and we can, perhaps, learn quite a lot about it.

This is a map of Heimaey. You can see the cinder cone here. You can see the ice pack map of the tephra, and you can see a map of different vintages of lava extrusion. It's on a very flat, really flat area, a bit, again, like Lothrop Wells. It's extruding to flat terrain, and you can see that the early ones cover quite a large area. And this reflects this exponential decline. A lot of the lava comes out early, and then it declines exponentially, the extrusion, so these are the tephra volumes.

It's a bit different from Lothrop Wells in the sense that there seems to be less tephra and more lava at Eldfell, reading Greg Valentine's very nice paper that he's just published, looks like more half-and-half in Lothrop Wells. I suspect this study, the Icelanders may have under-estimated the amount of tephra, because a lot of it fell in the sea.

These lavas don't do structures much good. These are houses being demolished by the lava as it flows out. It continues to degas, and cool, and crystalize, and so the magma viscosity does go up as it extrudes, and it crushes houses. And this is what the Icelanders did to try and protect the town. They squirted seawater on the front of the lava flow. That seemed to bring it to a halt, and then the lava flow started to go out to the east, and so the Icelanders thought this was a success.

We also have, serendipitously, one of the few cases where very detailed measurements have been made of the gas jet from such an eruption. This is work I did very early on in my career. We took film from the jets from Eldfell, and we measured small particles. We trapped the particles and got estimates of velocity versus height, above the fountain. You can see here, the gas velocities of the order of 100 to 200 meters per second for the jet coming out of the volcano. You can also see that the jet decelerates very rapidly, because it's an unconfined environment. It's working against gravity, it's going upwards, and it's entraining air, which basically entrains momentum and slows it down, so it slows down very rapidly in an unconfined environment. Obviously, we'll be discussing what something like this might do when it's going horizontally, where gravity isn't such a factor, and where we're in a confined environment. And we could imagine that the fluid behavior might be rather different in those circumstances.

So here we've got some data, which actually tells us that what we actually observe at these volcanos, and now I'd like to go on to Etna. Now Etna is not quite so good, because Etna is trachybasalt, 1975 Etna, but it's rather phenocryst-rich. And these crystal contents, it's about 50 percent phenocrysts and microlites erupted at 1070 degrees Centigrade. And these are from samples that we collected actually out of the lava flow, and quenched in situ during our study, so this is the actual properties of the lava as it emerged, rather than after it's completely frozen. And with Harry Pinkerton at Lancaster, we built a field rheometer, and this is Harry up here. You can't quite see him, but he's sticking this thing into the lava, and you stick it in several times. You've compressed this cylinder onto a spring, and then you release the spring of known force, and it pushes the piston into the lava, and you get a shear rate curve. And you can calibrate that back in the laboratory. And this is what we get, shear rate versus shear stress. This is some sort of idea of what the rheology of either like a trachybasalt, a rather more crystal-rich trachybasalt than Lothrop Wells would be.

And this slope is around 10 to the 5 Pascal-seconds. That agrees pretty well with petrological estimates of viscosity, independent estimates, so I think you can pretty well say this is for the degassed magma, trachybasalt coming out of a volcano, the viscosity is very unlikely to be more than 10 to the 5 Pascal-seconds. It's probably going to be lower for a number of reasons, but this is likely to be a sort of upper bound.

So the next one is to make another point. This is a volcano, which again is not quite so close to Lothrop Wells, but it's a mafic andosite from Lonquimay in Chile in 1989. I worked on this with Chilean colleagues. You can see the strata cone here. It's a satellite vent. It's a bit like a monogenetic eruption that formed a cinder cone, and a long lava over about a year, and my Chilean colleagues tracked the advance of this lava, and the thickness of this lava. It's a mafic andosite of 1,000 degrees Centigrade. You would expect Lothrop Wells-type magma trachybasalt to have lower viscosities than these. And this is an insightful case, because what we were able to do from that measurement of thickness and speed of the lava, was to get approximate estimates of viscosity from open channel flow equations. And these aren't precise, but they're certainly of the order of magnitude precision. And you can see that as the flow went from the vent outwards, the velocity declined on a log scale, and you can see that you can turn this data, and thickness data into viscosity, and you can see that when this lava emerged from the vent, the viscosity was just over 10 to the 5 Pascal-seconds.

We would expect Lothrop Wells to be lower than that, because this is a more -- it is essentially a cooler, and a more silica-rich magma. And you can see here coming out of the vent, this is what it looks like. Often these lavas, even these andosites, actually emerge first as pahoehoe. Certainly, it's the case in Etna, and it's the case in Heimaey. They emerge as pahoehoe, and they moved and developed after quite a lot of travel distance, so they get more viscous as they're implaced by orders of magnitude. So yes, they eventually end up at 10 to the 9 Pascal-seconds, more or less when they're stopping a year later, but it takes a long time to get to that sort of rheological state. It's not something that's instantly developed.

Okay. Last case history is the -- obviously, in the consideration of the repository and the interaction, it would be quite interesting to know what a high-speed multi-phased volcanic flow does to structures. And I just put this on as an example in Soufriere Hills in 1997. We had a volcanic blast where we - from the destruction of the seismometers, we were able to estimate speeds. And we know that the peak speed of this was around 90 meters per second. And we also can use the sort of work that Greg Valentine used from bomb blast damage to look at dynamic pressures, and infer velocities, and pressures on some of the structural damage. So this a flow with a peak velocity, something like a half the jet of a Strombolian cone at Eldfell. So let's see what this does to structures.

When it's going around 20 meters per second, the house doesn't fall out, but the roof gets blown off, and the windows get blown in. When it's going 40 meters per second, sorry this is a bit dark, but the flow is going from right to left, and the house - the top of the house, all the standing part of the house above the surface where the roof was knocked off. And you can just about see a big block here. I'm afraid it's not as spectacular, it's a bit dark, but this a block which was going with so much momentum it implanted itself in the side of the house. And this is what happens at 60 to 90 meters per second. This is the police station in the Village of St. Patrick's where the peak velocity was, and the police station, a concrete building, is gone. And that village, cars, bridges, buildings, were completely stripped from the land and pushed into the sea when the flow was going at 90 meters per second.

Now this is, of course, not exactly analogous, of course, for a wide range of reasons, but the flow density of this is approximately the same as the flow density of a Strombolian jet coming out the vent, and it stops, as I say, about half the speed. So this would be at least an example, one could at least say that some structures, at any rate, can have serious damage from these very high-speed flows.

So what can we learn for the lessons for Yucca Mountain? We can certainly say that intense explosive eruptions in the sort of -- we're using the Lothrop Well as an Eldfell analogy, and this is supported again by some of Greg's work. We see early on explosive activity, but there's early lava effusion, as well. We can see discharge of explosive jets into the low pressure atmosphere at hundreds of cubic meters per second, and speeds up to 200 meters per second. The magma starts wet, and quite happy to accept the experimental evidence of Nicholas and Rutherford, to get cursory type we might need 1,000 degrees Centigrade or less, but it erupts hotter because when magma ascends, crystals - the magma comes through the saturated crystals, and it releases latent heat, and that dominates over any adiabatic cooling effects of the gas. That's always the case, and so we can get really quite extensive heating. So the magma, in fact, cannot erupt as solidus. It will erupt somewhere between the solidus and liquidus, because of this effect. And wet trachybasalt lavas extrude with viscosities, and certainly 10 to the 5 Pascal-seconds seems to be an upper limit, and that's for pretty thoroughly degassed magma. And the flow, when the magma extrudes, of course, the viscosity is a strong function of time and distance as the lava extrudes, so you can't use a constant viscosity in trying to model the lava. And you can see that we can, of course, eventually build up to very high viscosities when the lava eventually grinds to a halt.

We can see that when the lava has become quite viscous, buildings can be destroyed. And we can see, also, from Montserrat, that at least some evidence that high gas particle flows can be highly destructive to some substantial structures.

Okay. Thanks very much.

MEMBER HINZE: Thank you very much, Steve. I notice your disclaimer here at the --

DR. SPARKS: Oh, yes. Sorry.

MEMBER HINZE: And so we will put that on the record, as well. Thank you very much, Steve. You hit right on the button right correctly, and we are anxious to hear discussion of that, but we'll hear the next talk first, and then we'll take both of them up in discussion.

Our next speaker is Bruce Crowe. Bruce, of course, was in charge of the DOE's volcanic program for a decade or more. He now describes himself as an interested observer, I think, but he's far more than that, and has been involved in the PVHA, as well as in the current PVHA update. With that, Bruce will be speaking about the volcanic history of the Yucca Mountain region, and implications for the risk triplet. Pleasure to have you here, Bruce.

I want to ask, are there any people that have joined us on the telephone bridge, before we get started? Okay. If not, then we'll proceed.

(Off the record comments.)

DR. CROWE: Well, while we're waiting for this, I'll just tell you what I'm talking about. There has been a lot of -- there are a lot of interesting and familiar faces out there. Okay, here we go. So I'm now with Battelle Memorial Institute. I've been with them since October, so they're a new organization. They did pay for my trip out here, which was nice of them to do. So now how do I flip through this? Okay.

What I'm going to talk about is really three parts here. It's just some background on the evolution of volcanic hazard models for the Yucca Mountain region. And then the major part of the talk will be really talking about the setting and volcanic history of the region, focusing on what I call the post-caldera basaltic volcanic cycles. And then looking at the cycle patterns, and seeing what they tell you, you can look at for options for future volcanic activity, focusing on the risk triplet of what can go wrong, and what's the likelihood. The effects will be in a later talk.

And then the third thing is, for the last 10 years, I've been working on environmental problems, and basically doing modeling, mostly probabilistic modeling on environmental problems. And there's a lot of parallels between dealing with multiple conceptual model uncertainties, and the work we're doing for volcanism.

So, as Bill mentioned, I'm a former Yucca Mountain participant, and now I'm unfortunately a distant but interested observer. It's been -- I've been doing other things over the last 10 years. As some people told me, there is life after Yucca Mountain. And it's been nice to be off the hot seat for 10 years.

What I've been doing, just real quickly, is I'm the Science Advisor for the Environmental Cleanup of the Test Sites of the EM program, a different side of the DOE house, and mostly I've been working on probabilistic performance assessments, transuranic waste, low-level waste. And right now, we're in the middle of trying to develop effective modeling strategies for dealing with contaminate transport associated with underground testing. And the common framework really is that probabilistic modeling is a risk tool to try to facilitate decision making under uncertainty, and clearly, there's a lot of commonality with the problems we're facing with Yucca Mountain.

So here's just an old approach that I first developed back in the late 70s, early 80s, which partly still pertains, I think. It's basically looking at the event probability, what's the hazard of an event. It has two factors to it, the recurrence rate, what I call E-1, and then the spatial disruption probability, which I call E-2. And what I've shown here, and I wanted to be purposely slightly fuzzy, because the details aren't important. It's an influence diagram that I've drawn. It's just one of multiple influence diagrams, as we all know that can be drawn. And, in fact, every time I redraw this, it's always different. I've never been able to keep it stabilized for more than six months at a time.

So a few things to emphasize about this model is it's an empirical model versus a process model, where most people would prefer dealing with process models, like we deal with contaminate transport where we use basically conservation of mass, and solve the problems on a process-base using the physics and chemistry. Instead, we have an empirical model where we used the record to try to forecast what future things might occur. And we've been cursed with this limited data problem, what I call a data paradox, where we have a small number of events, which keeps the risk low, but the uncertainty is large because we don't have enough data to really be very quantitative with how we design the models. And what that ends up, by necessity, you have multiple suites of permissive models, model assumptions, and parameter ranges, so for any of these boxes, the basic structure can change, and how people will parameterize these boxes in here varies dramatically. And with limited data, it's hard to say what is a right model, or what is the range of right models. The emphasis really should be on multiple permissive models.

So looking back from the perspective of

being away for a while, I thought it's kind of interesting, in the early 80s when I first developed this probabilistic model, there was a lot of discussion about whether that was appropriate or not, and that kind of dominated the database for the first I'll say four or five years, from the late 70s into the early 80s. And then in the early 80s, there was more acceptable of the probabilistic approach, but a lot of debates over exactly what are reasonable ways to set up the model, what are ways to do stochastic parameterization of all the little boxes I showed in the previous ones. And we went through a lot of phases of refinements, modifications of model assumptions, and focused a lot on probability ranges with some key questions being asked, as which model is right, or is there such a thing as a right model? And then, what is the role of conservatism?

I have some biases here that I'll go ahead and note. I think in probabilistic modeling, I think you should do everything you can to avoid conservatism, because it ends up biasing the output, and making it very difficult to do true sensitivity uncertainty analysis. But saying that, and actually doing that, is not an easy thing to do, and the experience with our PA models that we've been working on, is it's very hard to keep all conservatism out, but you have to really - I think you have to almost religiously try to avoid conservatism. So I left the program about `95, and the parallel I think is very interesting to 2000's, is that where people are doing a lot of probabilistic modeling for complex environmental problems, where we're trying to quantify the multiple components of uncertainty, look at trying to reduce uncertainty through data gatherings, through iterative model cycles. And the key thing that comes out, that I think is new and really important today, is that modeling, concept model uncertainty really dominate many of these problems. In fact, where we can do tests, the uncertainty in modeling, particularly conceptual model uncertainty dramatically exceeds parameter uncertainty.

So my current opinion I wanted to express, is that the volcanic hazard models are relatively mature models. We've been hacking at this, and arguing with each other for decades, and I think it's ended up being improving the modeling dramatically. I think consequences has a ways to go, but I think we've gone a long ways in the probabilistic side of the model. In my opinion, the remaining challenge is to try to do the best to agree on quantifying the uncertainty, and reaching agreements on kind of the range of uncertainty for the different model components.

So with that as kind of introduction, I thought I'd actually talk about the volcanic record. This is a bit more yellow than I'm used to. Here's the location that I'll be talking about, what's been called the southwest Nevada volcanic field, with the basaltic volcanic record being kind of the late ending phase record of this complex volcanic field. And I just wanted to emphasize that it is in the basin range, in the great basin portion of the basin arrange province, including both the southern basin arrange, the northern basic arrange, and here's Las Vegas, and the arrow points to Yucca Mountain there.

I always like to use this diagram, and I've been using this for so many years, it's really hard to see, actually. I've forgotten where I got this diagram, but it's basically a physiographic map of the southern Great Basin. And the key things here, this is where the southern Nevada volcanic field is located. Yucca Mountain here, is that not only are we in the great basin portion of basic arrange extension, but there's also an overprint on what's been called the Walker Lane System. And this has a strong overprint of right slip faulting associated with basin extension, so you get very complicated basins. And physiographically, you can kind of see that the most active parts of the Walker Lane right now are between Death Valley over to the crust of the Sierra Nevadas, whereas, where we are in this area, we've passed the major peak of tectonism, but there still is potential - well, there still is ongoing tectonic activity, just at reduced rates.

I've drawn kind of the boundaries. I followed the Las Vegas shear zone. I offset to the kind of northeast along the rock valley, Mine Mountains series of less slip faults, and then trace it up here. This is basically defined from work that I did with Will Karr and Gary Dixon back in the early 80s, where we argued frequently. Everybody draws slightly different parts of the Walker Lane, but I think there is agreement that we are in this area overlapping strikes than extension deformation.

Okay. And another key thing to note is, and I took this from - there's a really great web page that they've been archiving ages for volcanic intrusive rocks, and put together some really nice animation showing time space patterns of volcanism. The things I want to just point out is, I took some time slices out of that database that Alan Glazner actually developed the animation. What you see back about 30 million years ago, there was a sweep of volcanisms, mostly solistic volcanism that's preceded from north to south, kind of along an arcuit front across Nevada. Then another sweep that moved across the southern basin arrange, kind of sweeping up into here, and they both meet somewhere around the Lake Mead, Las Vegas area. But the key thing is that the southern spread at 20 million years, and about 11 million years marks the - right about 11 million years was the peak of this volcanism in the -- representing the Nevada test site volcanism, the southwest Nevada volcanic field. Following about 11 million years ago, volcanism transitioned to mostly basaltic volcanism, and then has restricted itself mostly to the active margin of the basin arrange along either sides of the province. But the key thing is that the Yucca Mountain site where we're looking at is at the south end of this migration of volcanism.

Okay. One thing I want to mention that's been kind of an interesting thing I've been doing for the last 10 years, is that there's an amazing amount of data for the Nevada test site region. There's been multi-decades of geologic and geophysical studies from the 50s into the 90s, largely related to weapons testing at the Nevada test site. The location of drill holes were obviously clustered in testing areas, and weren't as well distributed as a geologist would like them to be, but it still gives you a lot of three-dimensional data. And then, obviously, Yucca Mountain has been doing a lot of work since the late 70s, and continuing, with even some specific volcanic hazard holes that has been drilled more recently. But, also, there's been ongoing studies in the geology and hydrology of the test site from the environmental management program that I've been involved with, and they're continuing - there's expiration drill holes, geophysical studies, modeling, and contaminate transport that's ongoing, and we have almost an unprecedented level of knowledge of the geology and hydrology of this really complicated volcanic field, volcanic and tectonic field. And they've put together a 3-D earth vision model, that helps them for contaminate transport.

What always amazes me is, even with all this data, having mapped in a variety of volcanic fields, I'm always amazed by every drill hole, we find something new. And we find things that we couldn't explain. And like just recently, they've come up with a new caldera in part of this complex. So despite tons and tons of data, we still are constantly surprised.

Okay. So here is a satellite view of - a black satellite view of the southwest Nevada volcanic field. It's dominated by the Timber Mountain caldera, which still is expressed topographically. It has a large resurgent dome, and a series of clustered calderas associated with it. There were just multiple phases of large volume ash flow eruptions that built up big igmembrite plateaus, Pahute Mesa and Rainier Mesa, and Yucca Mountain actually is part of this in the south. But kind of right about at the waning stage of solistic activity, and then somewhat younger, a lot of these basins developed on the fringes that predicted the Crater Flat Basin that you'll hear a lot about in the next couple of days, Jackass Flat Basins, Yucca Flat, and Frenchman Flat Basin, so extension occurred. We think that there's some phase of extension early in the volcanic cycle, say from 11 to 15 million years, but most of the extension is late stage and postdating the major phases of solistic activity. And the extension seems to be also closely tied with a transition to balsatic volcanism. That's what I'm going to focus on next, but I just wanted to kind of show you the landmarks of the area, with the town of Beatty here, the Mercury area, which is the entrance to the Nevada test site is here.

Okay. A key concept that's been kind of coming in and out of vogue over the years, is what's now called the Amargosa Trough, and it's basically - O'Leary described it in a recent USGS paper, where - and I think most of the TVH panel members are pretty intrigued with what you see is a nice gravity divide, that's also a structural high between high-standing Paleozoic rocks along here, roughly following the trace of the belted range and CP thress, and then also the real highs along the bare mountain of the range. This has been a structural trough, and then a trough that's localized volcanic activity, both locations of caldera complexes within this zone, and then also, it influences the location of basaltic volcanism since the Miocene.

And what's interesting is, I first heard about this kind of trough concept when Will Karr and I went on a field and visited with Bennie Troxel and Loren Wright down in the Death Valley region. They were looking from Death Valley northward, and Loren was one of the first - he called this the Amargosa Rift. Will Karr picked it up, and we wrote some papers extending it possibly up as far as Lunar Crater. I later changed my mind on that, but you'll be hearing from Eugene Smith later today. He's kind of resurrected that, so this trough concept has played an important role in kind of the structural setting of volcanism.

So moving on to what I really want to talk about is the basalt cycles. What you see is, roughly at about the waning phase of the major solistic volcanic activity, there was continuing activity at Black Mountain about 9.5 million years, but roughly about 11 million years is the major activity. There was a switch from the Timber Mountain complex. There was a switch to bimodal volcanism, and an intense phase of basaltic volcanism occurred, mostly in the southern part of the trough here. There's a big shield volcano that developed, the mathic lavas, the dome mountain. And what we see in the subsurface and locally along mesa-capping ranges is that there are big volumes of basalt were erupted synchronous with basin development. You see slide blocks coming off of Bear Mountain that incorporate this roughly 11.3, 11.5 million year for basalt here. We know now flora is a large part of Jackass Flats, so it was an intense phase of basaltic volcanism, kind of in the late

Miocene, at about the time of succession of solistic volcanism. And most of this, if you look at it from a cycle standpoint, this lasted on the order of almost 3 million years. All of the basalts that we look at are pretty large volume. Some of them are greater than 10 cubic kilometers, but they all exceed about 3 cubic kilometers.

And then following that activity, there was a jump in activity that really occurred in two phases. There was continuing basaltic and volcanic activity associated with the Black Mountain caldera. And I originally had a couple of basalts that I thought might be separate parts of the cycle. People now are thinking that it's more likely tied to this basaltic volcanism associated with the waning phase of Black Mountain. But what also developed is there was a jump in activity out of the trough here, over to the Frenchman Flat basin here, and the Yucca Flat Basin. Yucca Flat, best we can tell, looks like almost a pure extension basin; whereas, Frenchman Flat is a strike zone basin. It's a pull-apart along the left slip rock valley system here. So the next diagram that I have shows you the cycle of activity associated with this phase of basaltic volcanism.

So in this area, there were three events, and this is pretty dark, so it's pretty hard to see anything, so I'll just arm-wave over them quickly. They range in age from about 8.6. There was a group of basalts called the Basalt of Pahute Ridge, which received a lot of study and discussion. And it is the same age as some plugs down in Scarp Canyon. And then we know from drilling and aromag data that roughly about an 8.6 million year basalt covers most of the floor of the Frenchman Flat Basin over in this area. The edge of the pull-apart is about right here. We've intersected a few, just a couple of spots. There's over 700 drill holes in the Yucca Flat Basin, so we think there aren't many basalts there, but in two sites they've intersected 8 million year old basalt. And then there's a cluster of three volcanic events associated with the Night Canyon 7.3. They're all about the same age, and these are actually - two of them are actually - two of them are mar volcanos and the other is just a normal little scurry cone in lavas that's been largely eroded away. But what you see is a cycle duration of about 1.3 million years. We think there's a decline in volume to the cycle, but we actually haven't put together the volume data on the older events. But, empirically, I think it's likely that there was a volume decline to that cycle.

There was roughly a hiatus from about 7.3 million years to 4.9, which is a barriel edge, an anomaly that hasn't been drilled down in southern Amargosa Valley. We know there's a 4.6 million event here, but volcanism switched back into the Amargosa Trough, actually in the Pliocene, and there's a sequence of events that are actually quite widely spread across this region, ranging from the Thirsty Mesa, Buckboard Mesa is the youngest, the basalts of southeast Crater Flat, and anomalies that we've learned a lot more about, and Kevin may be talking a bit about, in the southern Amargosa Valley. But what we see, again, is roughly a cycle duration of about 2 million years, and I think pretty strong data shows that there's a volume decline to that cycle.

The next cycle follows at a hiatus between Buckboard Mesa in the 1.1, to roughly about a 2 million year time gap. And then, again, still staying in the trough, you see the Lathrop Wells, a series of basalts down the middle of Crater Flat that we thought were all about 1.1, and just recently kind of a controversy has re-emerged on the age of the little cones, and I think that still remains unresolved. And then there's the two Sleeping Butte basalts, but this had, again, the same duration. I think you can ask a question of, is this cycle over? Are we still in this basaltic cycle?

So what I wanted to summarize here, and to kind of draw this together is, what we see is, and I don't want to go over all the details here. A lot of people here are very familiar with this, but there's four distinct pulses of activity. And what I think is important is looking at these styles of cycles, their typical durations, the volume decline through time, the time gap between cycles, you can use this to try to constrain somewhat the different models of what you think might happen in the future. And it depends a lot about where you think we are in this latest cycle, and what your compliance interval is, either 10,000 years or 1 million years. And so I drew this kind of complicated diagram, trying to tie this back to the two parts of the risk triplet, what can happen, and what the event probability is.

The most likely thing that could happen is a future volcanic event. And we're all trying to decide what that future event could be. But you actually end up with multiple options for defining the future events, and you can assign different probabilities for those.

What I've shown here is that this is the interval between the 1.1 in the Lathrop Wells, here we are at current. This is not to scale over here. And based on this cycle, you would predict that the next event would have a recurrence interval somewhere around the range of around 300,000 years, with a 50 percentile value here. And if we're still in this cycle, you might expect another Lathrop Wells, possibly a Sleeping Butte-type event that you'd forecast. If you go out for longer time frames, it runs - you can look at the possibility of either a second event, or possibly a sufficient amount of time that you could make one of these cycle switches. And we might switch to a whole new volcanic field, which would increase the uncertainty of what could happen.

What I really want to emphasize here is not that we know really well what's going to happen, but that we have to deal with multiple permissive models, and multiple ways to look at the probability data to try to forecast what might happen, and so we're back to this issue of multiple permissive conceptual models. And what I want to just emphasize is, what we've been working with recently is using Bayesian model averaging for fluid transport models. And there's a really great summary of this in a NUREG paper that the NRC put out, that Slomo Newman and

Biringer wrote, where they're basically proposing that when you have these multiple conceptual models, you can start to look at techniques, what they call ensemble assemblage techniques for looking at ways to treat the uncertainty of these multiple models. So with contaminate transport that we're doing now, we're looking at multiple alternative transport models that include variable boundary conditions, boundary flux, recharge, and hydrostratographic framework models. And what the Bayesian perspective gives you is a method of integrating that data in a way to both quantify the uncertainty, and to try to assemble your best prediction.

And then kind of a fun thing that I've been doing on my home computers was, there's this distributed climate change model that's run by an English group, where they've been sending out components of the global climate change model to home PCs, and they've been doing huge amounts of distributed computing. And what they do is very interesting, is it runs through a calibration phase, and then a stability phase, and if the models pass these two, there's a tendency in this stage to spiral off into a frozen globe, or a fiery globe, and the model becomes unstable. But they use kind of a screening criteria to go through these phases, and then they're basically mapping the model output space. And what's the real interesting insight is that you can - basically, they're contouring the number of models that converge in different areas of this model output space. And when you're dealing with multiple conceptual models, in this case they're not averaging the models in a Bayesian approach. They're treating them all as equal probable, when they go through the screening process, and so bringing that concept into the volcanism problem for Yucca Mountain, I resurrected an old diagram I did back in `95, where I was trying to wrestle with how do we constrain something? What I've used here for an example is the recurrence rate, or E-1. And what I tried to look at, is if there's a natural bound over here if you take the regulations of one event in quarterner is enough to bring you into regulatory sensitivity. So I just put this - this is a probability equal to one event, and I used 2 million years. The people have slid the quarternary around quite a bit, so you can move that left and right here.

I also looked at quarternary field limits, and Chuck has done similar sort of calculations, where boxes move around a little bit, but they're all fairly close in this area, where these are some of the more high activity fields in the Great Basin. And you can kind of put a limit over there. Since the volcanic fields we're dealing with in the Yucca Mountain region are smaller volume, toward the end of the end-member structure, or end-member magnitudes of volcanic fields, we probably have to sit somewhere to the right of this. And so, in `95 what I did is, I compiled all the alternative models, equally weighting all the models, and this is the distribution. And then if you take the typical rates out of the cycles that I showed you, this is the kind of midpoint values you get. I'm a little bit biased with the low end, because the way I've done my event definition. And as you'll be hearing for the next two days, there's lots of different ways to define these events. But what I think is interesting here is that you can actually use some physical limits, and as much data as you possibly can, to kind of constrain this probability field. And an interesting thing that I noted, I've been working a lot with decision analysts since I've been doing environmental modeling problems. And when I showed them this diagram, what they were amazed by is that they thought that this was not very uncertainty. They're used to look Superfund cleanups, and dealing with remediation options and decision options. And they said gee, as a order of magnitude uncertainty, that's pretty minor. They were completely unimpressed that we should be slaving away trying to reduce that uncertainty, so that's an interesting perspective.

So what I want to say kind of for final comments is, particularly from being away for 10 years, is I think that we have very evolved and mature volcanic hazard models. The model structure assumptions still continue to evolve, but I think the basic approach has been reasonably stable. And we're starting to converge, I think, on some agreement over exactly what those probability ranges are. There's always the possibility of surprise, but we've had multiple decades of data gathering, and so that reduces what the decision analysts call the unknown unknowns.

We're faced with significant remaining uncertainty. I think that there's no way that you're going to be able to reduce much uncertainty further. And we may actually be approaching the limits for the data sets that we have out at the Yucca Mountain region, of our ability to reduce uncertainty. So, I mean, what I could make as a finish comment is that I think the key thing is to try to do the best we can to quantify that uncertainty using a variety of techniques. I think that you need to look multiple permissive models. I think it's really key to calibrate to the volcanic record to make sure that you don't get so caught up in your model that you end up with physically implausible values for different components of the model. And that you assemble multiple models, and really look primarily out the model output space, and focus your analysis on the results and impacts of these multiple alternative models. And I'd also suggest that it's really going to be worth paying attention to a lot of the parallel developments handling conceptual model uncertainty, and other complex environmental problems across a range of disciplines. I think they're all converging on fundamentally dealing with the same kinds of problems, sparse data, multiple models, and how do you then collapse that into uncertainty components that you can deal with in a decision making format. And I'll stop there.

MEMBER HINZE: Thanks very much, Bruce. Steve, if we could ask you to return to the front, we'll open this to questions and comments. I'll first ask the committee, and I'll start over to my left with Dr. Clarke.

MEMBER CLARKE: Thanks, Bill. Very interesting presentations. I think I'm going to hold some questions until later. Thank you.

MEMBER HINZE: Allen.

VICE CHAIRMAN CROFF: As with Jim, I think I'll hold mine until later.

MEMBER HINZE: All right. And my colleagues here.

CHAIRMAN RYAN: Thanks, Bill. In the interest of time, I'll do the same.

MEMBER WEINER: I just have one very quick one for Dr. Crowe. How do you reconcile your statement that you need to get more realistic, and not include uncertainty, not include conservatisms and uncertainties with the quantification and reduction of uncertainty?

DR. CROWE: It's very difficult. Can I sit here?

MEMBER HINZE: Please. Those are live.

DR. CROWE: It's not an easy problem. I mean, we built - I worked with a multi-disciplinary group, and we built a probabilistic PA model for low-level waste disposal. And we thought we were doing a good job of staying away from conservatisms. We brought in a philosophy of mean-centered probability distributions. And then once we'd run the model and finished it, we went back and looked at it, and we were surprised that we had some hidden assumptions built in. We just psychologically had been so used to doing conservatism that we forced it into there. And what my decision analyst colleagues that I work with argue, that you want to stay as mean-centered as you can, and just widen your distributions. But then at the end when you're summarizing your final distributions, then you can look at like upper percentiles if you want to bring conservatism in. But I've been surprised at how difficult it is to keep conservatism out of your models.

MEMBER WEINER: Thank you.

MEMBER HINZE: I'll follow-up if I may, Bruce, with a question regarding the present data set that we have, and the PVHA-U was really prompted by the addition of data to the set, and re-evaluating the conclusions on the basis of that. And you stated that with the data sets that we have today, that we're pretty well bracketing in, at least on our probability aspects. Is there any data, given a blue sky situation where we have the money, where we have some more time, which we probably don't have - are there data sets out there that we should - that could be collected that would help to constrain the uncertainty over and above what we're looking at today? And I guess I'll pass that on to you too, Steve, when Bruce finishes.

DR. CROWE: It's an interesting question. I mean, when I put together that recurrence diagram, that was kind of going through my mind - what might change those bounds that I was putting up there. And Jean Smith's comments on Lunar Crater possibly could. It would break us out of the past cycles and say, maybe the future is a little bit more unconstrained than we thought. That possibly could pull you forward.

We had a lot of debates back in the early 80s of whether Yucca Mountain should start a monitoring program to look at like geodetic data, variations in the gravity field, just a whole series of things, and we could never get enough momentum in the program to start funding it. I mean, there was always interest, but not enough priority to start funding. I think that would be - one thing would be to get a baseline of kind of how the mountain is responding to modern tectonism, but the problem is quite difficult. I mean, we're close to drilling and exploring almost every bit of information we think is out there, so that's why I ended up saying that I think we're at the limit. I'd like to hear what Steve has to say, whether we think volcanology might advance enough to give us some new insights.

MEMBER HINZE: I'd also like to add to the question there - Steve, for you - looking at precursors, what's the limit of our ability to do a reasonable probability estimate on volcanic events with precursors? And do you see anything in the state of science moving ahead to where we might be able to affect a better precursor for long-term predictions?

DR. SPARKS: I'm inclined to agree with what Bruce has said, that we may be reaching, given all the studies that have been made, to - if you like, a limit on how much you can reduce the uncertainty of this issue, very low occurrence rate, monogenetic volcanism. I mean, the case that I cited of Eldfell, the earthquakes occurred - started about 24 hours before the event, and I don't see any possible observations within the current knowledge and technological developments that would likely forecast that an event of that kind was going to happen, so I'd be sort of rather pessimistic at the moment. I mean, we can do tomography of the mantle and find where bits of melt are, but, of course, those are -- the resolution of those is very poor to solve the problem. So for this sort of monogenetic volcanism, I think it's really pretty difficult, and the sort of work that Bruce is describing, of looking at very good dating, and trying to see if recurrence rates are random or clustered in some ways. Probably the only thing you can sensibly do.

As far as the consequences are concerned, I think there is quite a lot we can do, and I think - the main message of my talk really is that we actually do know quite a lot about these trachybasalt eruptions. And I think it wouldn't take a lot to reduce the level of disagreement that there appears to be in all the different reports by just looking at the data of where eruptions have actually happened, and where we've got good data on eruptions, which are broadly similar to the sort of Lathrop Wells case. So I think that there is - at least, I think we - one could imagine approaching this where there's a measure of agreement about rheological properties, about some of the constraints on dynamic processes, which are narrower than the current range of opinions on those that are currently in various reports.

MEMBER HINZE: Thanks very much to both of you. With that, let's move to others at the table.

Chuck, can we ask you if you have any questions or comments about these presentations?

MR. CONNER: Yes, thanks for the presentations. I actually wanted to follow-up on your question a little bit, because I think it's worth talking about. Back in `94, the Center CNWRA wrote a report saying that we really needed to pay attention to high resolution magnetic data, and seismic tomography. And currently, DOE has gone out and done great work gathering some high resolution magnetic data, which have really helped probability models quite a bit in terms of the nature of events we're dealing with, not so much the probability calculation, but the nature of events.

The seismic tomography data is in disarray, not to put too fine a pun on it. There's never been a high resolution seismic tomography survey. There are other places in the world where seismic tomography is used very, very effectively in looking at volcanic processes, that we just not invested in that in the Yucca Mountain area. I don't know if every expert on the PVHA-U panel wants to use seismic tomographic data, but I think quite a few do. But the fact is, even given the existing data, there's very bad agreement on the interpretations, or the models developed from the data that we have.

It's really, really unfortunate that we're going to go ahead and essentially complete probabilistic assessments without state-of-the-art geophysical data. And I think it's going to leave a door open that could have been closed by more data gathering.

Also, I would say that the aero magnetic data that's been collected has identified several anomalies that have never been drilled. I think there is a wide misconception that it's not worth drilling those anomalies. In fact, it is worth drilling those anomalies, because they'll tell us a lot about the nature of volcanic events in the Yucca Mountain region, and they may constrain the nature of temporal clustering of events that Bruce has referred to very well.

For example, there's one anomaly that's normal polarity that's not been drilled. Well, either that's a new cluster, or it happens to be at the boundaries of magnetic polarity reversal, so there's definitely a lack of state-of-the-art in those areas, I would say.

MEMBER HINZE: Thanks very much, Chuck. I think this whole data, additional data is something that is ripe for further discussion, and in the other periods, we'll have a chance to come back to that. Dr. Melson.

DR. MELSON: I was going to speak to Steve mostly. And in the case where you're showing these examples, as you know, these are, in a sense, within science, they are anecdotal. Is that correct? I mean, these are examples where we want to have a large population of things. And I'm speaking specifically of the behavior of water. You mentioned where you had a water-rich basalt, but you said it was erupted, I believe pretty much degassed. Is that correct? And it flowed kind of evenly, and we developed - from pahoehoe, we developed aahaah. And my assumption is you're speaking of a degassed basalt at the moment of eruption.

DR. SPARKS: I think the question involves quite a range of different phenomena, so it's - I'm not going to answer it in a simple way, because the nature of the process isn't simple. I think what you can say is that for the lava flows, they erupt in a degassed state, as we - I think everyone would agree.

DR. MELSON: Right.

DR. SPARKS: And from there phase equilibria and presence of minerals like Kaersutite, and in some cases, not necessarily, but in certainly the case of Etna from melt inclusion data, you can make some direct observations or inferences about water content, which I think are pretty robust. So the cases I described are all cases where we've got a wet evolved alkaline basalt, and we can then observe the phenomena that take place. And I'm certainly of a view that multiples in volcanology are not sophisticated enough on their own to get us to where we want, because the process is so complicated, without a good dose of empiricism. And volcanos themselves are telling us the story of what happens in these eruptions, so I'm not quite clear about the drift of your question, but I would say it's reasonably robust that we're dealing with water-rich magmas in these cases.

DR. MELSON: Well, yes, not debating the water-rich. The question is where is the water as these come out? And I would contend that if this basalt you say had 4 percent water coming out pahoehoe, I'd have to say nonsense, because 4 percent water is going to generate an incredible over-pressure in the atmosphere. You're going to have fountaining and degassing, violent degassing, so that's what I'm concerned about. I don't think you're being clear about where - you're using this water-rich repeatedly, but to me, you're not being clear about where that water is at the time it comes out as pahoehoe.

DR. SPARKS: No, I think -- okay. I can answer that in a number of ways. The magmas start out water, with water dissolved in them at several kilometers depth. They come up to the surface, and then during that process of eruption, the observation that they come out in highly explosive character in fire fountains, and asdi gas magma. That observation shows that those gases - there are processes operating which segregate the gases in a dynamic way to produce gas-rich and gas-poor magma.

If you ask well, is it pahoehoe - I'm afraid that's what's observed. I can show you photographs of Etna, which is trachybasalt with the melt inclusion data suggested it originally contained at least 3 percent water, and it comes out as pahoehoe. You can see that happening, so it's not a theoretical idea. It's an observation. Now how you explain that observation, sort of taking your point, and taking my point, is we don't have very good models for these. I perfectly accept that, but that's actually what you observe. And I think that the empiricism in these cases where you don't understand the processes terribly well, it takes a high --

MEMBER HINZE: I think we're going to have to move on. I'm sure we will be coming back to this more than once during the next couple of days. Bruce, for a few moments.

MR. MARSH: Great. I'd like to talk to Steve a little bit, too, maybe carry-over on this a little bit. I know you'll agree, it's really hard to box this in, but I think we are boxing these things in a bit. And if you'll actually look at some of these eruptions like Bill's talking about, like Heimaey, for example, I mean it does have cursor tied in, but cursor tied in, if you had the entire magma was cursor tied, and you only have to have 2 percent water in it. And as we know from phasic equilibria, the appearance of an affable really is a temperature indicator, not a water indicator, so you can have a magma that has a dome, for example, many, many domes will grow affable really late because they get below 1050, 1050 is the critical temperature really, so in and of itself, I mean, it is kind of anecdotal. For example, at Heimaey, Iceland, in general, is a very, very dry area. I mean, Bill did water on the Wright Counties Ridge and submarine things, as you get up there, I mean there's .3, .4 percent water submarine. And if you look at the Rhyolites in Iceland, for example, there are very few pyroclastic, if any big pyroclastic flows that come out of there. So I was struck - we can go through and talk about some of the other ones, too, in this manner. But I was struck, Steve, and also a little bit in Heimaey about the - could you enlarge a little bit on the interplay early in the sequence between basi-tephra eruption and lava eruption back and forth, playing back and forth, which, in some ways, makes you think that maybe water wasn't all that important in there, didn't have a big high water content. But did you find this curious, too? I mean, you didn't get very explosive events that blew down the town, for example, things like this.

DR. SPARKS: Not particularly. I mean, I'd sort of like to go back to a point about Iceland. You may well be - of course, you're right about what happens in the Raycants Peninsula, but that's not the volcanic environment we're dealing with. It's a transformed fault basalt volcanism where the basalts are really quite explosive, a lot of monogenetic volcators on the Raycants Peninsula. They're very similar to sorts of - from physical volcanology, a petrology point of view, to alkaline volcanism. So the drawing in the sopholitic volcanism on the main ridge is not really relevant.

MR. MARSH: But the rhiolinic volcanism, like at Tofra Yoca, where we worked, is 200 cubic kilometers of rhiolinic material.

DR. SPARKS: Yes.

MR. MARSH: Very dry, enormously dry.

DR. SPARKS: Well, I --

MR. MARSH: That's just right on the same rift system you're talking about.

DR. SPARKS: Yes. Well, I mean, there is 1362 eruption of Arifia cooler, there is the Tophia Cooler will detox around there, around the aspirating 75, which are all highly explosive variety production, so I don't accept your point that the magmas, the Rhyolitic magmas are not explosive. There's lots of examples of explosive activity from Rhyolites. That's probably not the most pertinent point, because I'd go back to the point about Kaersutite. Kaersutite tells you it's 2 percent water in the amphibole but that's not the relevant point. And if everyone - well, I think my reading of the consensus is that people have bought into the Rutherford and Nicholas work, and I'd sort of accept that. And that's telling you that if you Kaersusite precipitation from a alkaline basalt, you've got a sort of minimum of around 4 percent water. That's the phase equilibria. It's actually quite consistent, because if you take those observed, and I stressed the observed eruption temperatures of 1030 to 1050 degrees Centigrade, that is exactly what you would expect from thermodynamics, from a magma saturated in water at the high depth, with 4 percent water, coming up to the surface and degassing, crystalizing out, raising its temperature, and with one atmosphere liquidus of 1150, also 1105, so it's more or less what you'd expect. So I don't think that the petrological community would be -- see this as a sort of a controversial issues. These alkaline basalts are, in a sense, observed with some inference and things like inclusions to have high water contents.

MEMBER HINZE: With that, I'm afraid we're going to have to cut off discussion, Steve and Bruce. We will, I promise you, come back to this, because this is at the very heart of some of our problems and the disagreements.

With that, I would like to suggest that we take a 15-minute break. Please keep your questions. We'll come back to them, if we have to stay here all evening. We will reconvene at 10:20.

(Whereupon, the proceedings went off the record at 10:07 a.m., and went back on the record at 10:24 a.m.)

MEMBER HINZE: With that, we will move on to the next speaker. If we could please, Charles Connor. There's Charles.

I do want to tell you that the handouts for the next two speakers, I understand they are not back from reproduction. They will be available shortly but they are not currently available. And we do apologize.

With that, I will introduce Professor Charles Connor, who has been involved with the Center for Nuclear Waste Regulatory Analysis Investigation of Igneous Activity for the NRC for many years. And is currently a member of the PVHA update. And he will be discussing with us one of his very favorite topics, probability assessments. Please.

PROF. CONNOR: I don't know, Bill, I'm pretty tired of it.

(Laughter.)

MEMBER HINZE: Don't give me too many straight lines. I try to be a gentleman but there is a limit.

PROF. CONNOR: My wife and I have been working on probabilistic assessments for various volcanic hazard problems around the world. And modeling volcanic processes, tephra dispersion, and that sort of thing. And, you know, when we get together, you know, drink a beer with our neighbors, they always ask, you know, why do you study volcanology in Florida.

And my wife has come up with fairly stock answer that is in 25 years, the more we learn about volcanoes, the farther we want to live away from those volcanoes. And I guess there is a lesson in there for this project somewhere but I'll leave that to you.

Okay, a disclaimer, the topic I'm presenting here today is all about my work and Laura's work. As Bill mentioned, I'm a member of this PVHA-U Expert Panel but it certainly, what I'm presenting, does not represent the views of the panel as a whole in any way or people involved in the PVHA-U process other than me.

It does represent DOE in any way or former employers like the CNWRA. So this is all me. And Laura. She can't defend herself here today but that's it.

Okay. I thought I'd better talk today

about the probability of igneous disruption of the repository from a probabilistic perspective. I want to warn you that I have included turgid detail in terms of the text on these slides. I'm not going to go through the text now but the object is that you will have the presentation eventually, I guess. And you will be able to read about this in more detail.

We've already heard a bit about the tripartite nature of the probability. What is the nature of igneous events? What areas do specific events impact? What is the spatial intensity of volcanism? And what is the estimate of recurrence rate of igneous events to the region, which Bruce Crowe just concentrated on a minute ago.

Inherent in all of this is a specific definition for volcanic events. And I would make one comment about the white paper at this point. In the white paper, the white paper follow the logic that is presented in the literature. And that is an inconsistent definition of volcanic events.

So we need to shed the past a little bit and be very specific about event definition because when we discuss the different probabilities the different working groups have come up with, they often involve different definitions of volcanic events.

I don't know how you guys can help that. But that's the fact. So it has to be very, very clear.

In this analysis, I'm going to assume that the repository itself does not repository itself does not impact in any way the probable distribution of future events. That is something that we can discuss in more detail.

And I'm going to present a method for looking at scenarios based on volcanic mapping and volcanic terrains, basaltic volcanic fields in several places. And you can see how we develop a view of volcanic events that is consistent and usable in the context of PVHA and ultimately the hazard assessment.

Okay. One thing that Laura and I have been working hard on lately is the development of an event simulator.

I've written papers about Yucca Mountain through the 90s and terminating with a paper I wrote with colleagues in 2000. And in each one of those papers, we've always said look, we're not doing a complete analysis because we haven't paid enough attention to the structure of the igneous events itself.

And so I'm trying to rectify that lately by simulating volcanic events as geology dictates they likely appear in the substrate beneath the Yucca Mountain region.

So here would be a good example of a single event which consists of multiple dike injections, multiple vents or vent-like structures, and in some cases, as drilling has indicated, we should probably include sills in the analysis as well.

So with this event simulator, what we've done is taken actual geologic data derived from geologic mapping, as I'll elaborate on in a minute, digitized those events, built a library of those events, and essentially then we can draw on that library to create literally millions of simulated events by which we can look and see the frequency of intersection of those events with a proposed repository boundary, for example.

I really will say that this has been quite a eye-opening experience for me because for the first time, I can see how these events relate well to the observations we have in the field and how the probability models relate well to observations from the field.

So here is an example from the field. This is one from San Rafael, Utah. It is a pliocene volcanic field on the eastern margin of the basin and range. Quite a similar environment in the sort of the gross geologic scale of things.

Paul Delaney and colleagues mapped there quite a bit in the mid-90s, late 80s and mid-90s. Here you can see events. A system developed in the San Rafael associated with a four and a half kilometer long dike swarm. The photograph is basically looking in this direction so you can see one of these dikes and that vent complex in the background.

Zooming in on the vent complex, you can see that it is actually a large zone, complicated in nature because this is maybe an eruption that evolved over time, one which is similar to events like Paricutin or perhaps Heimaey, which we observed historically.

So there are some observations we can make about the nature of dikes which we can feed directly into our event simulator. Dike segments that rotate as they rise through their complex structures. Dikes can be mapped extending up to about ten kilometers in the San Rafael region from vent areas. But commonly these dikes forms are shorter.

Dike orientation is consistent with regional structural patterns. Paul Delaney mapped the relationship joints. And multiple dikes are most commonly associated with single igneous events.

We could do the same for vents and vent-like structures. So here is a picture. It is pretty dark here. I guess that is going to be the theme for the day but you can see that there are vents here and screens of sedimentary rock attached to those vents still. But you can see that this alignment events formed and the rocks rounded and subsequently eroded.

If we maps of these structures, like this one, you can see dike sets going through here with vents forming. Along that dike set they have complicated geometries and so on.

Paul Delaney first pointed out that all of these vent-like structures probably didn't form cinder cones or scoria cones at the surface. So we don't necessarily know that only one scoria cone was associated with this alignment. But that is certainly a possibility even though there is more than one vent-like structure.

Sills are also common in the Yucca Mountain area. Much less common that scoria cones but you can see here in the Pauite Ridge maps sill anomaly A appears to be sill or sill-like. We don't know of anomaly C or D are sill or sill-like either.

And we have example of sills and basaltic volcanic fields where these things are exposed.

So the bottom line is, the geology ought to incorporate this -- or the probabilistic models should incorporate this diverse range in geology.

So let's look at an example event simulation, one example. Here is a single center with multiple dikes and vents shown in map view here. So actually this is somewhat similar to the Pliocene Crater Flat. And I've drawn it here to be consistent with the orientation of faults, fault patterns in the Yucca Mountain area.

You know there is an idea that dikes are going to be North 30 East in the Yucca Mountain area based on regional stress. And I think that is true if you are looking at the lower crust. But if you look at the near-surface region, and certainly the repository falls in the near-surface region, Pliocene Crater Flats, the Thirsty Mesa vent alignments are north-south. Lathrop Wells is elongate north-south. And so on.

So it looks like almost all the evidence we have is shallow north-south intrusions through here so that's why these dikes have that sort of orientation.

We can add complexity. We can add sills to these scenarios to help forecast the likelihood of events. And then we can develop alignments like this one, which would be aligned on a northeast trend with multiple dikes and volcanic conduits or vent-like bodies in this case associated with that.

So if we go through our analysis with this library of geologic structures, and we marked across say this map area at grid points and do thousands of simulations using a parallel computing platform to describe what is going on here, then we can get an idea of the frequency of events intersection.

The main point here is that we've attempted to inject geologic reality into the analysis. That is, this looks to me like San Rafael or other exposed volcanic fields in Utah. It is consistent with the surface geologic information we have in the Yucca Mountain region. So this is an example of trying to develop this sort of simulation.

You have to develop PDFs for sampling this library, which can be pretty complicated and give volcanologists plenty to argue about, say the numbers of centers per event may be a uniform, random distribution between 0 and 5, number of dikes per center, half normal distribution with mean and standard deviation one and five, et cetera.

So it is possible to develop or infer some sort of distribution there. And you can develop a map that looks this. This is for dikes. And this map contours in percentile from 90 to 10 percent. So the likelihood of dike intersection given this event simulation -- so this is based on thousands and thousands of simulations. And it gives us an idea that based on the library of known geologic structures, that would be frequency of dike intersection at the repository given an event centered on any grid point within that area.

And we can do the same thing for frequency of vent intersection with the repository given an event and frequency of sill intersection given an event.

So we can draw from this -- and you can see that it is becoming bumpy here because the frequency of silver injection is very low in my model. And, in fact, probably 1,000 simulations per grid point weren't quite enough in the Monte Carlo simulation to extract that.

Now we can combine that with information about the spacial intensity of volcanism and here is a statistical model for spacial intensity of volcanism in the Yucca Mountain region based on the past frequency of events. And one of the main problems, I think, in probabilistic assessments has been --and I think we are finally overcoming some of those problems -- is that you have to use a consistent event definition here that is consistent with the information that I showed you previously.

So in other words, we have to -- it treats all of Quaternary Crater Flat as one event shown here, if I'm going to use the type of simulator I showed you in the previous slides.

That is not always done consistently because people often focus on pieces of the puzzle, naturally enough, but again, you have to be very careful when you are comparing all these past probability results that the event definition is consistent that you are using. And that is not always the case.

So this is a non-parametric model. It is a Gaussian kernel function. Non-parametric statistics is the rage. And I think it is appropriate to use this kind of approach for the Yucca Mountain region.

Basically the probability depends on the Gaussian kernel function, the distributions of past events, and some estimate of a bandwidth, which you can think of as the standard deviation of a Gaussian function about that. So we can develop these sorts of models for the region.

When I combine the output of the event simulator with that map I just showed you, we get a map that looks like this. So this is the likely location of events based on spacial intensity and the results of the event simulator that would impact the repository. And you can see this region down around the Solitario Canyon fault in easternmost Crater Flat would be the zone most likely to impact the repository.

And if you integrate these results, you get a probability of intersection, given an event in the region, given that volcanism occurs, of something like five percent. I don't want you to seize on numbers here because it is just not appropriate in this venue. I'm giving these as examples.

They are going to change. There is a lot of code involved. Our code is not qualified. All those caveats pertain. So these numbers are given as examples.

But you can see that the general pattern sort of makes sense. That given the much higher probability of volcanism out to the west on this picture, and given our understanding of the distribution of events, it would be an event located here, southwest of the repository that would be most likely to lead to intersection.

And we can do the same thing for these other kinds of structures, igneous vents and vent-like structures, and sill injection as well. And those probabilities, just for example, are around say one percent and .02 percent.

Bruce made a big point of uncertainty. And I concur with that completely. It turns out -- it has been five or six years since I went through an entire calculation from start to finish for probability of igneous disruption of the repository.

And I was absolutely struck in doing this analysis over the last few weeks that it is incredibly -- the output is incredibly sensitive to input assumptions. It is unbelievably sensitive. I can change the result by an order of magnitude in a flash by changing some assumptions.

And the reason is -- or one of the reasons is the Yucca Mountain is located at the edge of this volcanic field. We are dealing with the edge of the system and it is very sensitive to those spacial distributions and probability.

So what Laura and I have done is tried to assess the impact of uncertainty in the spacial intensity. And I just want to spend one minute going through this because it is really quite important. If I have a limited number of events, the few triangles on the map which represent volcanic events in the region, and I construct a probability density function from that distribution, then I must be uncertain about its form, right? Because I only have a very small sample.

So what is the cost of that uncertainty? Bruce presented this in the context of uncertainty and temporal recurrence rate but what about spatially? Well, we can borrow methods from geophysical inversion of other types of data to really understand the uncertainty in that surface.

And so what we do is if we've got say a surface composed or defined by 11 events and we construct a probability density function from that, we resample it. We draw 11 more events from that surface, reconstruct that surface, a new surface from that new sample, and recalculate the probability of disruption of the repository or recalculate the spatial intensity of volcanism at the site.

And if you repeat that over and over and over again in a Monte Carlo fashion, you can get a sense of the uncertainty in your spatial intensity.

Obviously if you have few events, your uncertainty becomes very high. If you have a lot of events, if you are in the seam of a volcanic field in a different part of the basin and range, you should have a lot of certainty about your surface.

And so this is the graph that I want to show. I changed bandwidth in this direction on my Gaussian kernel. I can look at the likelihood or the spatial intensity in that direction.

And the mean values follow a nice distribution like this. So for short bandwidths, I'm saying that volcanism is most likely to cluster very stronger in Crater Flat.

And as it moves out, the probability of disruption of the site or the spatial intensity at the site increases because that probability surface is spreading out and encompassing the site.

The point is is uncertainty drives the entire analysis. If I say choose a bandwidth of seven -- or six or seven, something like that, you can see that here is my quartile distribution spanning several factors here but if I go out to the 99th percentile, I'm spanning almost an order of magnitude of uncertainty.

Okay. I don't see how we can get around this in the statistical model. Since we are basing them on few events, we are going to have high uncertainty. I don't think it surprises anybody in the room.

If we look at temporal recurrence rate, I won't belabor this because Bruce went through it in some detail, you can get a maximum likelihood estimate of something like two events per million years that also has uncertainty associated with it.

And if we turn the crank as an example only, this is the kind of output we get for probability of dike intrusion in the repository, that .05 number times two to the minus six gives you about one times ten to the minus seventh per year, lower probability for vents, lower probability for sill.

The point is don't fixate on these numbers. They are examples. But there is something like an expected value based on this specific analysis. Well, the uncertainty is what drives it. If we look at a likelihood ratio since we have very few events to choose from, we don't our recurrence rate very well as Hope pointed out a number of years ago, so we've got a recurrence rate that varies from something like two to the minus seven events per year or six times ten to the minus six per year. That alters the probability somewhat.

But then the uncertainty in the spatial intensity, for example, increases our uncertainty by something like a factor of five. So you wind up with being pretty sure that the probability is somewhere between zero -- or approaching zero -- and ten to the minus six per year.

We can introduce a lot of geologic data. I think it is really crucial to interject geologic data into this kind of analysis. There are various methods for doing it. But I think the point is is that we are going to live with uncertainty in these kinds of calculations and the types of order of magnitude are slightly larger than order of magnitude uncertainty that Bruce was talking about is going to exist in these analyses.

So I think I can leave it there but I've got some comments on that. Specifically I want to say that the analysis I just presented is not complete. I could do a lot more things -- and I'm not trying to circumvent the PVHA process, which I think is very, very important. I presented this as an example of where we are going.

So the take-home message, I hope the white paper emphasizes event definition because inconsistency in the use of event definition, as it exists in the white paper now, is confusing to readers, to a casual reader. It would be extremely confusing.

And second I hope the white paper emphasizes uncertainty because although I'm not willing to quote you an exact expected value today, I think that range of uncertainty is something we're going to wind up living with. So I really hope that the uncertainty is emphasized at some point.

MEMBER HINZE: Thank you very much, Chuck. And thanks for your comments regarding how to improve the white paper. We are looking for that from everyone and encourage you to make those comments.

With that, Chuck, we'll have discussion of your paper after Gene Smith's --

PROF. CONNOR: Sure.

MEMBER HINZE: -- presentation.

And with that, I'll call upon Professor Eugene Smith from the University of Nevada, Las Vegas, who is currently a contractor for the Clark County, Nevada program.

And Gene will be talking to us about the importance of understanding the process of magma generation for volcanic hazard studies. And as we've heard, we are probably going to be learning more about the Crater Flat, Reveille Range, Death Valley trend.

Thanks so much for being here, Gene. And you have a half an hour.

PROF. SMITH: Okay, can everybody hear me? I'm not sure I have the microphone on properly. How about now?

MEMBER HINZE: I think we need it a little louder. You may have to speak up, Gene, and lay it on the line. There you go, it's working now.

PROF. SMITH: Okay. We've got all the technical problems settled here. I want to -- I guess I have to do this myself. There we go. Okay.

MEMBER HINZE: Excellent.

PROF. SMITH: Now we have all the technical problems solved.

I'd like to try to take this discussion in a much broader -- look at a much broader perspective. Up to now, as you've noticed, all these speakers except for Steve have sort of focused on the Yucca Mountain area.

I'd like to broaden our perspective both geographically and also I'd like to take us deeper.

I'd like to take a look and see what the influence of the mantle is, the Earth's mantle, on all the processes we are looking at here.

And first I'd like to acknowledge support from Clark County and the State of Nevada for my work over the past several years.

Now the main point I want to try to give you today is that it is really important to understand the process of volcanism before calculating the probability of future events. Process is very important.

Now in the past several years, there have been several models proposed. And one that people are talking about today, at least most people are talking about today, I've called the traditional model. This is a model that is based on geochemistry that goes back to the 1960s and 1970s. And it is a model that focuses on Yucca Mountain.

It assumes melting in the -- this is sort of a picture of the upper part of the Earth's lithosphere and mantle. The crust is about the upper 30 kilometers. The green slab here is the lithospheric mantle. This is the rigid, non-convecting part of the Earth's mantle. It has been basically isolated from the convecting part of the mantle which is the asthenosphere.

And there is some debate as to the depth of the boundary between the lithospheric mantle and the asthenospheric mantle under Yucca Mountain. Some earlier studies suggested it was 100 kilometers. And I've heard some recent comment that it might be as shallow as 60 kilometers. So I'm just going to put 60 to 100 kilometers down for the boundary between the rigid part of the mantle and the convecting part of the mantle.

Now the traditional model assumes melting in the lithospheric mantle and basically implies that volcanism is waning. There is a very limited amount of material to melt in this area. And if you assume that the traditional model is correct and volcanism is waning and the probability of a future eruption is actually very small.

About seven years ago, I proposed a deep melting model. It assumes melting in the asthenospheric mantle, that is melting at depths greater than about 100 kilometers. Now this model has broader perspective. It focuses on an area extending from Death Valley all the way to Lunar Crater, including the Yucca Mountain area.

And the implication of this model is that a new peak of volcanism is possible. That volcanism is not dead. And in the future, we might have an upsurge of volcanism.

Now several speakers have already talked about this but this is the area of interest around Yucca Mountain from the Lathrop Wells cone, here is the repository block.

Several -- both Bruce and Chuck have talked about Sleeping Buttes and Buckboard Mesa and the Pliocene Crater Flat. So I won't discuss this in any more detail. However, I just wanted to show you that there are several different, in terms of calculating probability studies, there are several different interpretations of the area that should be considered for probability studies.

Back in the late 1980s, Bruce Crowe suggested this zone right here which he called the Crater Flat zone. It included most of the -- it included all of the Quaternary volcanoes and most of the Pliocene volcanoes.

Back then Bruce and I didn't agree with each other very much. So I had to come up with a counter zone. So I suggested a zone that was basically similar to Bruce's. I called the area of most recent volcanism, pretty much the same as Bruce's except it includes Buckboard Mesa.

Major difference between the Crater Flat zone and the AMRV is that the Crater Flat zone does not include the Yucca Mountain block. AMRV includes the Yucca Mountain block.

And Bruce mentioned this and I have the orientation of this a little bit skewed here but the -- this is something that goes back to Will Carr that Bruce mentioned, the Amargosa Trough, which many of the panelists on the PVHA update are considering is the area of interest for volcanism. All of these interpretations are pretty well focused on Yucca Mountain.

And there is another interpretation which Richards Carlson, a former member of the panel suggested. He suggested that volcanism is focused on the Timber Mountain Caldera. And with time, volcanism shifts inward and is focused more and more in the area around Crater Flat and the area just to the west and south of the repository.

This particular model is based on something that I did back in the middle 1990s with Gene Yogodzinski. We concluded that a portion of the lithospheric mantle was probably more susceptible to melting and was more likely to melt. And we termed this area the Amargosa Valley Isotope Province. And many of the panelists have used this as the area of interest and an area that probably would melt and produce magmas.

Now let's take a look at the traditional model and then we will try to assess it a little bit. The traditional model, again, assumes that melting is in the lithospheric mantle. Again, this is the part of the mantle that doesn't circulate and it contains material that has been isolated from the convecting mantle for perhaps billions of years.

And because of that, isotopic ratios have evolved to high initial strontium ratios and low epsilon neodymium values. Basically what has happened is that the isotopic ratios have changed with time from their original values.

Now melting in this lithospheric mantle is a difficult thing to do. The rock type is peridotite. Peridotite melts at a very high temperature. So two ways of getting around this are to add water to the peridotite. If you add as little as a half percent water, this lowers the solidus temperature and allows some of the peridotite to melt.

Another possibility is there might be fusible zones within this green slab, the lithospheric mantle. These fusible zones might be mafic dikes or hydrous components which were added to the lithospheric mantle a billion years ago, maybe even earlier. And that these fusible zones, which I tried to show by these little diamonds, are the most likely portions of the lithospheric mantle to melt.

So I'm just going to talk about these two possibilities. One, we added to the lithospheric mantle to melt it. And two, we have these fusible zones, these small, isolated veins or dikelets, which melt out first.

Now if you melt a water-rich lithospheric mantle, there are some things that we have to understand. Water in the lithospheric mantle is commonly hosted in minerals such as hornblendes and micas.

Now recent work starting back in the mid-1990s indicates that mica and hornblende are host for high fuel-strength elements. These are elements like niobium and tantalum. And I'll show you why that is important in just a second.

These particular minerals take these elements in and they are enriched in niobium and tantalum. Partial melting of a peridotite containing as little as three percent mica and/or hornblende will produce basaltic melts that have positive anomalies in niobium. That means they will be enriched in the high-fuel strength elements. They will be enriched in niobium and tantalum.

Let me just show you what the implication of this is. This is sort of dark. Sorry about that. What I have done here is I have plotted the concentration of elements normalized to ocean island basalt which is a very common thing done in petrology. You can normalize it to a variety of different parameters. And I've plotted it along the X axis element, from cesium to the rarest element, lutecium.

Now the black line represents a typical Crater Flat basalt. This is from one of the one million-year-old centers. Notice that it has a signature here of a negative niobium anomaly. And if you were to look at tantalum, tantalum would also show this dip. And we won't take a look at the other characteristics. There is not time to look at everything.

A typical mica-bearing peridotite, which may represent lithospheric mantle -- now this is an example of a mica-bearing peridotite from the Colorado Plateau. It is not from the area beneath Yucca Mountain. It shows a positive niobium anomaly.

Now if we melt just a small part of that, if we melt five percent of that, we wind up with a rock -- here this is the model rock -- that has a positive niobium anomaly -- very different from the actual basalt that we find at the surface at Crater Flat.

So if we have hydrous phases, if we melt the peridotite that has hydrous phases, we cannot produce the characteristic niobium and tantalum depleted trace element patterns that we see at Crater Flat. And a pattern that is also very common in many other continental basalts.

Now if we go to the second possibility about melting in the lithospheric mantle, that we have these hydrous material and mafic veins, most of this material we have to realize, as Bruce mentioned earlier, that volcanism in this area has been ongoing for a long time, ever since about 12 million years we've been producing first felsic volcanism and them mafic volcanism in the Yucca Mountain area.

And most of this volcanism has a very similar isotopic signature. And I think that most people would agree, at least people who believe in the traditional model, that most of this melting has occurred in the lithospheric mantle.

The first point I'd like to make is that if we have been melting lithospheric mantle for a long period of time and we have been melting these hydrous zones, then most of this material has already been melted, most of this material is already gone. So there is probably very little left.

And what I've tried to do here is I've shown these little diamonds. The white areas are the hydrous material that basically has been melted out. We only have the little diamonds to melt. Therefore, in the future, if you believe in this model, there is very little additional magma to be produced.

Now even if we do melt this material out, we still have the problem that is probably very hydrous, contains hornblende and mica, so it is probably not going to produce magmas that will have the right composition.

So we have a very difficult problem here. This production of this negative niobium anomaly, production of this high fuel-strength element dip that we see in Crater Flat in the magmas is unlikely to originate from melting lithospheric mantle compositions.

Now the problem is -- and this might be a more complicated situation -- we don't really know exactly how we produce this chemical signature. And it is possible -- and this is sort of scary -- but it is possible that this chemical signature may not be a simple reflection of the source.

So I think we have to be careful when we look at the traditional model because it is very difficult to produce the Crater Flat magmas by melting a lithospheric mantle.

Now let's take a look at the deep melting model. Melting a lithospheric mantle and melting of the asthenospheric mantle down in this area here below 100 kilometers, the lithospheric mantle does not melt according to this model.

The model focuses on a larger area extending from Lunar Crater to Death Valley. And we support the model by episodic patterns of volcanism and also depth of melting calculations. I have references at the back of this talk that you can take a look at later.

Now the area that I'm interested in -- and, again, this slide is dark -- is an area that actually Bruce and Will Carr and several other people suggested a long time ago and that is belt that extends from Death Valley up to Lunar Crater. Yucca Mountain area is right there. Here is Crater Flat.

Here is the area that almost everybody else is focusing on.

It is interesting that the Death Valley volcanic field is only about 20 kilometers south of the aeromagnetic anomalies they were talking about in the Amargosa Valley. And we don't really know that much about this. We don't really have good dates down in this area. We don't have a lot of good chemistry. It is something we have to find out more about in the future.

Here, for example, is one of the cinder cones, volcanic necks in Death Valley in the Greenwater Range. We don't really know how old this feature is. It erupted -- lava flows have cascaded down into the valley but we don't really know exactly what is going on here yet. There has been some work done but work was done back in the 1980s.

Okay, now what I want to do is I want to first focus on this episodic volcanism. And I want to try to go through -- and some of you have seen this before -- I want to try to go through a very quick animation that will show you the evolution of volcanoes from Yucca Mountain to Lunar Crater. I won't do Death Valley because we don't have a lot of dates down in Death Valley. We don't know what is happening down here.

Now the animation will have a bar on the bottom. I notice that it is operating a little bit slowly today. I have no idea why. But a little bar that will move from left to right showing you the age range that we are talking about.

So we'll start at 9.5. We're going to go to 6.5. We have volcanoes here, 5.5, this is the Lunar Crater, Reveille Range area. Here is the Yucca Mountain area. Very little happening in this age range here -- 2.5 to 1.5, activity down at Lunar Crater but that is about it.

And then one million years, we have the Crater Flat volcanoes being produced -- some activity up in the Reveille Range. And the most recent activity, Sleeping Butte, which I think I might have to revise the dates on a little bit. So we produced a very narrow chain of volcanoes. These are all the volcanoes that we have dated.

And here they are color-coded as to age. And since you probably went through that very fast, I'll try going through the animation once again. But in this case, I'll go through in terms of the color coding. Start at 9.5 and go up to .5. Here is the earliest activity, yellow, 6.5, green, 5.5, 4.5, 3.5, 2.5, 1.5, and the most recent activity both at Lunar Crater and down at Crater Flat.

Now I'll summarize this in this bottom -- number of events versus age and this is number of dated events. So in the Lunar Crater area, there are a lot more events that we haven't dated. So these peaks will probably be higher in the Lunar Crater area.

But notice something very interesting. After about four million years, there is a really nice synchronous pattern between Crater Flat, that is shown in pink or whatever color that is, and Lunar Crater, which is shown in blue.

We have a peak here, a peak here. We have a period of quiescence here. And another peak here. A really nice -- at least in my mind -- correspondence in patterns going from about four million years to the present. Prior to that, the activities were disconnected.

Now one of the questions that you might have is whether this pattern is common throughout the Great Basin or whether it is focused just on this belt. We've taken a look at two other areas, southwestern Utah and the Coso Volcanic Field in California. I've done a lot of work in the southwestern Utah Volcanic Fields so most of the information, most of the dates are my own. So I know that I'm dating individual volcanoes.

The Coso Volcanic Field information is information I've got from the literature. And I'm not sure whether the dates are from separate cones or multiple dates from the same cone. But let me just show you this.

Here is southwestern Utah. And see we also have an episodic pattern. But the peaks are at different places. There is very little correspondence between southwestern Utah and the Crater Flat/Lunar Crater Belt.

Now the Coso, there is a better correspondence between the two. But especially the one that stands out is this four-million-year-old peak. But the rest of it is -- we do have two peaks here but there is not a very good correspondence. So I put less emphasis on this one because I'm not really sure how many dates are from the same cone.

So I think that there is a nice correspondence in terms of patterns, very similar episodic patterns.

Now depth of melting, I'll try to go through this relatively quickly. This is based on over a thousand samples that were done at UNLV and also isotopes at the University of Kansas. They looked at basalts that are younger than about 8.5 million years old. And this work was published in the Journal of Geophysical Research back in 2002.

Now we have produced melting profiles beneath volcanic center. The top of the melting profiles were based on sodium contents and the bottoms of the melting columns were based on FeO, iron contents. And I won't go into the rationale of this. I can answer questions later or the reference that I gave you does provide the entire technique.

And we produced this very interesting profile across the Great Basin from the Sierra Nevadas to the Colorado Plateau. The purple is the crust. The blue is the lithospheric mantle. And the green is the asthenospheric mantle.

Now we have two different models for lithospheric mantle/asthenospheric mantle boundary. The blue is a boundary from Jones at the University of Colorado. He interprets a thicker lithospheric mantle beneath Crater Flat and the Yucca Mountain area. Zandt's 1995 model predicts a lithospheric mantle thickness at about 60 kilometers.

Notice both of these models predict or

show that the lithospheric mantle thins quite dramatically as you go to the west. But these arrows that I'm showing here, these are the melting columns that we calculated. Opposite the arrows, the tips of the arrows represent P_F or the top of the melting column. The bottom of the arrow represents P0, the bottom of the melting column.

The thickness of the melting column or the width of the melting column is very important because this indicates the volume of material that will be produced during that event. Notice that the tops of the melting column very nicely, at least I think so, correspond to the lithospheric mantle/asthenospheric mantle boundary.

Melting is really deep in the Crater Flat/Reveille/Lunar Crater area. It becomes shallower as you go to the west. It becomes shallower as you go to the east. In general, most of the melting is occurring in the asthenospheric mantle. Very little in the lithospheric mantle.

Now the deep melting model must explain several things. It must explain -- now I have to mention this -- that in order to get this deep melting, we need mantle temperatures about 200 degrees higher than what you find, for example, in the western part of the Great Basin, in this area right here. Temperatures have to be about 200 degrees higher in the mantle in this particular area.

So we have to explain the hotter mantle temperatures. We have to explain this very narrow belt of volcanism. And we have to explain the episodic pattern. And even more importantly, we have to explain why volcanism has been occurring in this area, in this same belt, for 11 million years.

We know we can get a chain of volcanoes like we see in Hawaii. But why is volcanism occurring in the same place for such a long period of time? And I just want to show you this belt again. It is a pretty narrow belt going from Death Valley up to Lunar Crater.

We don't get any Pliocene or Pleistocene, or recent volcanism, basaltic volcanism from this belt until you reach Utah. And to the west, we don't get any until we reach eastern California. So it is a very narrow belt extending into the central Great Basin. It is an isolated belt.

Now we have to take a step back here and take a look at the history of Nevada for the past 400 to 500 million years. One thing that we noticed, here is the Lunar Crater/Crater Flat Belt right here.

It is just to the east of the boundary between the North American Craton and younger accreted terrains. That is this is the boundary of the stable core of the North American continent. And that boundary goes just to the west of the Lunar Crater/Crater Flat Belt.

Also notice there have been a lot of mountain-building episodes in Nevada over the past 400 million years. The most recent of those are the Sevier Belt just to the east of the Lunar Crater/Crater Flat Belt and the Central Nevada Thrust Belt which actually goes right through the area of the Lunar Crater/Crater Flat Belt.

So there is ample opportunity for thickening of the lithosphere during Paleozoic and Mesozoic tectonic events and as I showed you in that earlier cross section, we've had thinning of the lithosphere beneath the Sierra Nevada. And I think this has developed over this period of time a keel in the mantle lithosphere.

So what I'm basically saying here is that we have to consider, and this is a very simplistic view, but consider the mantle lithosphere moving through the asthenosphere as a boat moves through water. When a boat moves through water, it kicks up turbulence. You develop eddies.

And these eddies and turbulence actually move with the boat. You also have the weight following the boat. And that weight follows the boat as it moves.

So in a very cartoonish fashion, I'm suggesting that lithospheric mantle -- here is the western boundary of the North American Craton. Here is the thinning of the lithospheric mantle. I'm not sure exactly where this occurs. It depends on which model you like to use. It could occur slightly to the west of -- I believe this volcano is supposed to represent Yucca Mountain and the Crater Flat volcanoes. I'm not exactly sure where this offset occurs. We don't really know exactly.

But in the mantle, in the asthenospheric mantle, we have areas that are hotter than other areas. And I'll show you some seismic topography evidence of this in the next slide.

The mantle of lithosphere is moving in this direction here. It is kicking up mantle eddies. You also have edge effect where asthenospheric material is moving up along this boundary from high pressure to low pressure.

Now one thing we have to do is we have to find some way of getting these area of hot mantle that exist in the asthenosphere to melt because they are below the solidus temperatures, probably very close to the solidus temperatures. So one way we can get them to melt is to have them interact with mantle eddies. And have them pulled up to lower pressure.

And if we move magma from high pressure to low pressure, we can melt magma adiabatically. That means with no additional input of heat. So I'm showing that happening right here. We have a mantle eddy in a very cartoonish fashion moving this hot mantle up, partially melting it. And eventually producing a volcano here in the Lunar Crater/Crater Flat Belt.

Now notice that this buttress is sort of fixed in space with respect to the volcanoes in Yucca Mountain. The eddies in the very simplistic view are moving with the plate. So any time we get an area of hot mantle intersected, we may, we have the potential of producing volcanic activity.

Once we reach an area of colder mantle, even bringing it closer to the surface probably will not be enough to cause it to melt. So we get a period of quiescence. You won't get another peak of volcanic activity until we reach another area of hot mantle.

Now do these areas of hot mantle actually exist? And as Chuck said, we don't really have very good seismic tomography. And the seismic tomography we have is very low resolution.

Ken Dueker at the University of Wyoming produced this diagram several years ago. Basically this is looking at relative P-wave velocities. The red areas are areas of low P-wave velocities or areas that might be hotter lithosphere or hotter asthenosphere.

Now one of his sections, BB [1] goes from Wyoming down into southern California. It is shown here in cross section.

And the red areas are areas of hot or hotter mantle. The green and blue areas are areas of colder mantle. Even in this low resolution seismic tomographic image, you can see that the mantle lithosphere, we're going down to about 200 kilometers -- this first dash line is about 200 kilometers. So we're mainly interested in 200 kilometers up to the surface.

Notice we do have hot areas, red, separated by colder areas, green. Another hot area, cold area. The blue areas are the colder slabs. But apparently the asthenospheric mantle is thermally very inhomogenous. There are a lot of areas that are hotter than others.

And theoretically then, and this is speculation, if we had a good seismic tomographic image of southern Nevada, if we knew the direction of plate motion, if we knew where the next area of hot mantle is, and if this model has any value, we could predict when the next major phase of activity or the potential of the next major activity would be at Yucca Mountain.

Now also we have to realize that the shape of the volcanic field -- if we're dealing with these hot spots -- depends on the three dimensional geometry of the areas of hot asthenosphere. So if this is the buttress right here and this is the area of hot material, we'd start off by getting volcanism here.

As the buttress moves in this area here, we'd start getting activity along the Crater Flat/Lunar Crater Chain or from the south to the north. And this picture right here would mainly occur in the north. Here it would mainly occur in the north. But notice all of this activity is occurring along this black line which represents the Crater Flat/Lunar Crater Chain.

The volume of material produced at any one time depends on the lengths of the melting so it is theoretically possible that we can get another episode of high volume material erupted within this belt if, in fact, we intersect a hot spot that has a three-dimensional geometry that might be suitable.

MEMBER HINZE: Gene, pretty soon?

PROF. SMITH: Yes, okay. Let me go back to the conclusions here. I'll show this model later if anybody is interested.

So the implications of this -- probability studies, I think, should try to look at petrologic model. If we look at the traditional model, we develop a certain picture for the future. If we look at the deep melting model, this produces another potential scenario for the future.

We have to try to factor in petrologic models. We can't ignore this. Whether you accept the shallow melting model or the deep melting model, you know, is fine. But we have to understand these models better. We have to know how these models work. We can't ignore the petrology. We can't ignore the geology.

So the basic conclusions then, I guess the main point I'm trying to leave you here, it is important to know why in order to determine when. And

I think that is the most important point I'm trying to present. And I think probability studies are dependent on the petrologic model.

Thank you.

MEMBER HINZE: Thanks very much, Gene.

Chuck, could we ask you to join Gene at the front. And we have 15 minutes scheduled for questions and comments.

I'll ask the Committee, starting with Dr. Clark.

MEMBER CLARKE: I just had a quick question for Professor Connor. Early in your presentation you mentioned, almost in passing, that there was an inconsistency in the definitions of the volcanic events. And that the white paper would need to address that.

I wonder if you could be a little more specific about that?

PROF. CONNOR: Sure.

And I don't mean to imply that it is some error, oversight. It is a common problem. So, for example, when -- Bruce will correct me if I'm wrong -- but when he wrote a paper in 1980 about probability of volcanism in the Yucca Mountain region, he was talking about the probability that a volcano will form based on the distribution of past volcanoes.

And certainly in 1995 when Britt Hill and I wrote a paper, we were basing it on the distribution of volcanoes. So that gave us probabilities, I think, as a group on the order of one times ten to the minus eight, sometimes a little higher, sometimes a little lower. And no one thought those analyses were complete.

When the first PVHA convened, I believe they largely looked at the probability of volcanism but tried to tack on a probability or somehow account for the dike as well at the end of that analysis. So if you are not looking at probability of -- if you are not defining the event, you can get a very different probability out of the analysis is basically the story.

So what I tried to do is in my presentation is talk about the probability of dikes, the probability of sills, and the probability of vents and propagate that definition throughout the analysis.

It becomes most critical when you are calculating a spacial intensity based on the distribution of some event and then you are coupling that to a sort of a consequence model of well, what does the geometry of the event look like. That definition has to be consistent.

And it is not always easy to do that with the information in the literature because people rarely do or say they are doing a complete analysis. So it is really quite important.

And I think it is fair to say in the current PVHA, the plan is to phase that much more carefully. I don't know -- I still wonder if it is possible to get ten volcanologists to agree on what we are analyzing. But, you know, I mean it can lead to dramatic variations in the reported probability.

MEMBER CLARKE: Thank you.

MEMBER HINZE: Further questions? Allen? Mike? Ruth?

MEMBER WEINER: I'd like to ask Dr. Connor the same question I asked Dr. Crowe. How do you incorporate realism into your model? Or don't you?

PROF. CONNOR: Well, with the event simulator -- that is the whole goal of the event simulator is trying to incorporate realism into the model. So those event simulators are my geologic interpretation of what the Yucca Mountain region would look like if I could carve off the upper 500 meters of alluvium and tuff. And look at the igneous intrusion geometry.

And so that is based on a library of volcanic events that have been mapped. So in my opinion, that's geologic realism.

We can certainly argue if the events would be identical, if the trends would be the same, so on and so forth. But the core issue is that the libraries are actually based on geologic observations. So that is number one.

Number two, on spacial intensity, I choose to present a very data-driven model, that is a model that is quite simple from a statistical perspective but based on the distribution of past events in the Yucca Mountain region. And then look at the uncertainty in that analysis.

And then number three, I agree with what Gene said and Bruce said to a certain extent before that which is we need to look carefully at the geologic context of the recurrence rates we are using.

So if we track the development of models over 20 years, I would say more geologic realism is in those models. But, again, to get back to my earlier point that it would be really nice to have other geophysical data to use. And I find it very difficult to reconcile the fact that our view of the mantle is very low resolution compared to what it is in other parts of the world.

MEMBER HINZE: Chuck, a brief question. In the white paper, sills are mentioned. But they aren't given much attention.

You've talked about sills here today. And Greg Valentine and his colleagues have shown us at Pauite Ridge the importance and the occurrence of sills. You calculated some probabilities with sills. And I notice that they were up in the ten to the minus ninth range, something like that.

Can you tell us a little bit more about your thoughts about sills at Yucca Mountain? We have not seen any. Of course, there are problems in seeing them, too. But we haven't seen them. Are they likely? Why is the probability down there in the ten to the minus ninth range?

PROF. CONNOR: Well, that is a good question. First of all, I want to raise the caveat that my analysis, as I stated, didn't include the effects of the repository itself. So, for example, if sill development is more likely because the repository is there, that is not included in the analysis.

It looks to me like the interpretation of the drilling results from aeromagnetic anomaly A indicate that that is a sill. And if I recall correctly, it is something like 60 meters thick. So it is perhaps better referred to as a sill complex or something like that. And I haven't seen any update about that since the original drilling results were reported. But that's one.

Where we have exposure to the east of the site in the Pauite Ridge, there are lots of sills associated with that vent and dike complex. And, in fact, where these things are exposed worldwide, it doesn't seem like sills are particularly lacking in abundance.

Nevertheless, in this initial analysis, I assigned a much lower probability to sill formation based on the relationship between known sills in the Yucca Mountain region and the total number of igneous events. But it is fairly poorly constrained.

MEMBER HINZE: Another very detailed question. You mentioned that your numbers were not to be taken too seriously at this point. What about the patterns? Are the patterns significant?

PROF. CONNOR: Oh, yes. I think that it is -- again, I don't want to put too much emphasis on one analysis. There are a lot of people working on this kind of problem. But, you know, the patterns of volcanism, I think, have persisted even in the literature over a fairly long period of time. So I don't think the patterns are going to be too different.

For example, a significant source of uncertainty is that Yucca Mountain is located at the edge of this active basaltic volcanic field. Now, you know, you can do a cluster analysis and say well, based on the cluster analysis, it is essentially part of the field. Or, you know, so on and so forth.

But the fact is it is at the edge. So that leads to some uncertainty in probabilities as an example. And that persists through all the analyses.

MEMBER HINZE: I wanted to make certain that got on the record so that the probability -- I mean the pattern was realistic or as good as we can do.

Dr. Melson?

DR. MELSON: I was interested in Gene's presentation but I really think we have people here who if they want a comment on that, Greg Valentine has done a lot more work certainly than I have about this. So if we could, if they want to say something at this point? Or I'll go ahead with my question. If they want to. Is that appropriate or not?

MEMBER HINZE: Well, we'd be happy to have questions if Greg or his colleagues wish to ask a question. Not at this point.

DR. MELSON: Okay. Well, I just have a couple of things. This correlation you have of the activity in Lunar Crater and Yucca Mountain areas is, I assume, statistically significant. We have so few points there. I mean it looks like it is significant to me just on inspection.

Have you tested the significance of those peaks? Or how sensitive they are. If you add another peak randomly are they going to disappear? Or have you done a statistical test of that correlation?

PROF. SMITH: No, I haven't done any statistical analysis at all.

DR. MELSON: Because it is a really suggestive correlation.

PROF. SMITH: I mean visually it is very suggestive. We're adding additional data. We are doing more dating at Lunar Crater and Reveille. And hopefully we will add additional data because we only have about 60 percent of the vents in Lunar Crater and Reveille dated. And the plot that I showed you is just dated volcanoes.

And I try not to guess at the ages of volcanoes. Sometimes you can do that by saying this is geomorphically very similar to another volcano that is one million years but I tried not to do that on that plot. I only plotted volcanoes that we had good argon-argon dates on.

But no, I have not done any statistical analysis.

DR. MELSON: Just a real quick question, too. Assuming the asthenosphere and the lithosphere are moving relative to each other -- assuming that which normally is how we -- when we look at plate tectonics, we have, you know, lithosphere and we have the asthenosphere. And there is a relative motion.

And that relative motion can create, you know, disruptive distributions. In other words, maybe it is going to be east-west where as ours are north-south. Have you considered relative motion between the asthenosphere and the lithosphere in your geometric considerations of where these vents are falling?

PROF. SMITH: No, at the present time, my analysis is very cartoonish because we don't really know the geometry of this buttress.

We don't even know, based on which model we use, whether we use the Zandt model or we will use another model, the Jones model. We're not sure whether the buttress is located to the east or the west of the Yucca Mountain area. We are not sure whether its three-dimensional orientation is. So it is too early to actually do what you suggest.

I think, again, I have to emphasize a point that Chuck made is it is something we really need in order to evaluate this model is we really need some better seismic tomography. We need to know what the mantle is like. And as far as I know the new geosphere project -- EarthScope project is going to get that information.

So we have to find some way. I know it might be impossible. I'm not sure but we have to find some way to get the information so we can see what the mantle is like because in my view, the mantle is very important in producing the patterns that we see and in terms of explaining why volcanism is occurring where it is.

And I think it is really important to h ve better geophysical data, especially for the mantle. I mean right now, we are not even certain what the thickness of the lithosphere is beneath Yucca Mountain.

Again, I've heard models, I've heard -- at the last PVHA-U meeting, the 60 kilometers was thrown around. I've also heard estimates as weep as 100 kilometers.

There is a lot we don't know. And a lot that we should know before we come up with a final assessment of model.

MEMBER HINZE: I think we have a comment on this topic from Frank Perry from the DOE. Frank?

MR. PERRY: Since Bill invited this, I'm Frank Perry from the LANL. And I would like to comment on an aspect of this model.

There are two rebuttals to this model that I've written. One is in a framework AMR. And the other is in an EO's article that dealt with the aeromag and drilling data. So I just want to get that on the records that there are some written rebuttals that people can look at.

But I'd like to make one comment just on Gene's presentation. We've done a lot of work on these mantle reservoirs. But I don't want to talk about that. I'd just like to point out different patterns of volcanism, between lunar and the Yucca Mountain region, Gene showed the similarity in the timing of the episodes.

But what I think was a little misleading about that plot, that only showed the dated volcanoes.

So there is actually very few dated of the total populations up at Lunar Crater. So the height of the peaks at any particular age looks similar for Lunar Crater and the Yucca Mountain region which could lead one to believe that the recurrence rate is fairly similar.

And one of his conclusions was that it is possible to go to a place in the geologic future where the recurrence rate will drastically increase in the Yucca Mountain region. So I want to point out that in those two episodes since six million years ago, you know, 6 to 4.5 and then the Quaternary, in both of those cases, the recurrence rate was much higher in Reveille and Lunar.

In the Quaternary, for example, there is anywhere from 60 to 80 scoria cones compared to eight in the Yucca Mountain region. So it is about an order of magnitude difference.

So in my opinion, there's no actual volcanological evidence any time in the last five million years that the Yucca Mountain region has reached the rate of activity that you see at Lunar Crater. And no evidence why you would expect that in the future given the last five million years.

MEMBER HINZE: Thank you very much, Frank.

I'm afraid our time for discussion is up. And we have, obviously, more questions and more concerns that need to be addressed to this. And we can pick those up later in the day. So please retain your questions.

And we will move on then to a presentation on probabilistic volcanic hazard analysis by none other than Dr. Kevin Coppersmith, who has been the lead for PVHA and the update that is currently going on as well as in many other areas.

With that, Kevin, we are pleased to have you here and we are anxious to hear your comments.

DR. COPPERSMITH: Thank you. Can you hear me okay? Am I amplified?

(Whereupon, the proceeding went off the record at 11:39 a.m. and went back on the record at 11:41 a.m.)

OVERVIEW OF METHODOLOGIES IN PROBABILISTIC VOLCANIC HAZARD ASSESSMENT AND APPLICATION AT YUCCA MOUNTAIN

CHAIRMAN RYAN: Let's come to order, please. Now for something completely different. We have heard a lot about probabilistic volcanic hazard analysis and so on in terms of real volcanoes, real data, and discussions about how the models work, what key components of the models are. I'm going to change gears and talk completely about process, about methodology, about ways of eliciting expert judgment to quantify the assessments that you heard something about on the previous talks.

This goes beyond, of course, volcanic hazards. I'll talk a little bit. I want to get a history, get into where we are on this and seismic hazard and some other areas, and give a feel for the history of this activity, -- a formal structured expert elicitation started in earnest back in the early '80s for purposes of NRC-regulated facilities, I would say -- and talk a bit about how we got to where we are now, talk about what we did for PVHA-96 and what we're doing now on PVHA update.

I did want to make a point for those of us who are interested in this concept of earthquake volcanic forecasting. I heard last night a discussion of a forecast of what the weather conditions will be like for the commute this afternoon. They said it could be snow, it could be rain, it might be sleet, we might have frozen rain. And, finally, she said, "It's going to be very difficult to forecast this. And I think tomorrow you're going to have to watch our nowcast. We'll have a nowcast that you can get on that will tell you exactly what is going on right now."

(Laughter.)

CHAIRMAN RYAN: It seems to me you could look out your window and get your own nowcast. But it's something to think about as we go forward in the face of significant uncertainty. We'll try to avoid the nowcast.

What I will go through is, first of all, the summary of the evolution of formal expert elicitation methodologies. I speak for a very large group of people who aren't in this room who have helped develop these methodologies through time.

Many of them have been associated with the Nuclear Regulatory Commission, who has been involved in these types of studies for many years, mostly related to reactor regulation and to safety analyses for probabilistic risk analysis through the years, decision analysts who are involved in developing the process of gathering expert judgment and in aggregating multiple expert judgment, as we have in this process.

And for many subject matter experts, like myself, who have to span different sciences and to learn the terminology, the difference between a neodymium ratio and the B-value sometimes can be difficult to relate, the issues of uncertainty, though, and the lessons learned, what I want to talk about and what type of solutions have we developed for the last 10 or 20 years that we can take advantage of and we have tried to take advantage of in exercising this for the probabilistic volcanic hazard analysis for Yucca Mountain.

There is a common set of essential steps now that we would all say need to be followed in this type of assessment. I will summarize those; quickly go through the basic elements of a PVHA; summarize and focus on the PVHA-96, which will be the licensing basis for the licensing application; and review the methodology that is being used; and put the PVHA update, which will support license review.

Let me step back. Bill Hinze is here. So he can correct me when I am wrong on some of these issues. I want to talk about two large expert elicitations that were conducted in the mid 1980s.

One of those was sponsored by the Nuclear Regulatory Commission. The other was sponsored by the Electric Power Research Institute. And the goal of these studies was to develop estimates of probabilistic seismic hazard at the power plants east of the Rocky Mountains. So at that point I think NRC was looking at 69 sites.

EPRI ended up looking at a few more. And, again, the issue was to develop an idea of the probability of exceeding the safe shutdown earthquake ground motions at these sites, which have been developed largely deterministically.

And there were large issues related to the Charleston earthquake in 1886, whether or not that could occur elsewhere. Could Charleston break its chains, they say, and go on to ravage the rest of the eastern U.S.? Are there tectonic and other identifiers that allow us to say that hazard in one part of the Northeast, for example, is different than you might expect in the Midwest or in Florida and other locations? These basic issues led to the development of these two studies that were done largely in parallel.

I'm only going to talk about the methodology components to these. They differed in many ways. The data dissemination process was quite different one study to the other. One assumed that experts -- they both gathered panels of experts. One study assumed that experts were able to develop their own data and bring that to bear. Others tried to supplement the data that experts might have and to disseminate that information to them.

The EPRI study used expert teams for characterizing sources. The Livermore -- we'll call it Livermore-NRC study -- used individual experts.

There were differences in how much the experts were allowed to interact. There was a thought at that time that in an expert elicitation process, experts should not interact; in fact, they should be as independent as possible.

Other differences -- and I could go into a lot of detail -- in the way that experts were aggregated, one study said the experts should remain anonymous. They were identified only by number. The other had them identified by person. And the aggregation methodology was one that was either mechanical or behavioral in going through the process.

Well, the net effect of having two different studies also and two different approaches led to different mean hazard at many of the power plant sites.

The median hazard, the results of these types of hazard studies are usually couched in terms of a seismic hazard curve. It relates the ground motion, let's say, a particular ground motion parameter, a peak acceleration versus an annual probability of exceeding that. And that hazard curve can be used directly in subsequent analyses of risk and so on.

What we saw is that the mean hazard curve was quite different at several sites. And that difference was troubling. The medians, as I said, were similar. The mean, as you know, is largely a function of uncertainty. So, as we see in the skewed log-normal distributions and probabilistic hazard, both volcanic and seismic, the means can be, in fact, often at a very high percentile and very different from the median estimate.

The detailed sort of analysis of this, which we'll foreshadow to a study in a minute, really show that, in fact, the differences were largely due to process followed, the methodology, as opposed to fundamental differences in the earthquake identification process, the seismic sources, the assessment of ground motion, and so on, that that process difference led to a significant difference in mean hazard. That is troubling.

So what is needed, then, is a set of rules, if you will, or approaches that can be commonly considered as consensus rules for how these types of studies should be done so that we could do it all one way and look at the results and try to quantify the uncertainties that come out of the agreed-upon methodology as a way to proceed.

So that's what this study, the so-called SSHAC study -- it's a Senior Seismic Hazard Analysis Committee -- was put together as a group that was sponsored by EPRI, NRC, and DOE. All had the common goal of coming up with methodologies for dealing with uncertainties and for dealing with expert judgment in a consistent manner that would lead to more stable results in the future.

Some of the problems that were identified by SSHAC in going through this process in these earlier studies, this wasn't necessarily attributed to either of the studies, but it was a general series of problems. It was overly diffused responsibility.

Experts come in. They make assessments. And they leave. Do they own the results of that study? Do they say later on that they, in fact, made these assessments? Do they own the assessments made by others on the panel? Was it a consensus? Was it consensus-driven or forced? Did it have to happen? Did they sign the results, things like that?

Insufficient face-to-face interaction. It turns out in these fields, seismic, hazard, I would say volcanic, and many others, they are large. If you take the whole community that knows something about this problem, it's small.

And the issue of independence is one that's moot. The chances of keeping or having a series of independent experts on a topic like this is, number one, it can't be done. Number two, its' counterproductive. The interaction, the natural interaction, that scientists, earth scientists have is a positive influence on the process.

Now, there are other areas -- and this is an area of quite a bit of discussion now in things like global climate change and so on, where there is a large group of experts in the field. And they would like to select sub-samples of those experts to see how consistent their assessments are.

But in this type of field, in fact, we all go to the same meetings. We interact on a regular basis. And we challenge and defend each other. And that process is something that should be encouraged in these types of assessments.

Many other areas here. The issue of outlier experts was one that was very difficult. The Livermore study had one expert in ground motion attenuation who the rest of the distribution was over here, all the other experts, and he lay well out in this side.

Again, because they were anonymous and not defined by name, no one knew who this person was, but he had a distribution on uncertainty that was tight, narrow, and way out of the rest of the group.

And that issue of an outlier expert I remember caused quite a bit of difference. I remember Harry Seed at that time saying, "There's a very small difference between an outlier expert and an outright liar."

(Laughter.)

CHAIRMAN RYAN: Feedback is also something that's very important. We'll talk more about that. Often experts do not realize the implications of their assessments. If you're dealing with things piece by piece, if you don't put them together and show when I put together this A value and this B value in this recurrence plot, I get these results.

And we found, for example, some of that feedback showed that experts were predicting magnitude five earthquakes would occur in this area every other week with this combination of A and B values, with their uncertainties. So feedback is a very important component.

Finally, just some key aspects of the SSHAC group that came out after arguing for two and a half years. One of the key things that we could agree on is that all probabilistic hazard analyses, including a PVHA, should attempt to represent the center, the body, and the range of technical interpretations of the larger informed technical community that they would have had if they had conducted the study.

Well, it's not saying that you need to bring people in and you bring in 8 samples from a group of 100. You need to make sure you have carefully selected samples. In fact, members of that expert panel need to think about and try to represent the full range of views. And that was a different view of expert elicitation from the classic balls in an urn-type approach to selecting a subset of a larger population.

That means that they need to know what everyone else in the community thinks. They need to study alternative views. They need to know the difference between Frank Perry's model of Lunar Crater, Crater Flat, and Gene Smith's model. They need to be exposed to those, understand the range of interpretation.

I understand that's one of the goals of your draft paper, is did we include the range of interpretation. So this is something that, in fact, SSHAC is saying needs to be looked at. Not all experts are going to agree with the range of assessments. It needs to be something that's put in front of them.

It's not a typical expert elicitation issue. In other words, it's not something where the value is either known and it's just a series of experts are trying to quantify the uncertainty. In fact, our problem is one that requires a lot of learning and interaction and model building.

We don't bring in people and in a day ask them for their assessment. They actually have to construct models and do work and learn along the way. That's very different from a decision analysis view of expert elicitation.

A couple of other things that are important that came out. This view of the larger technical community obviously has to be hypothetical because the larger informed community means that they would have had to have gone through the same process that our experts spent two years on coming up to speed on all of the local Yucca Mountain data and so on to be able to make these assessments. But we do make a distinction between evaluators and proponents. And this is very important to the assessment.

A typical process of science, particularly the earth science, is one of having proponents make their views known. I know that Gene will go to a GSA meeting and present this model. And they will say this is still a cartoon characterization.

But here is why. Here is the data. Here is the model. Here are my results. And we then have discussion. And that will have challenge, will have debate. It may be public. It may be at lunch. It may be something that happens through a period of written responses to peer-reviewed journal reviews. It may be one that occurs in a private forum.

But that process of having a proponent present a view and people to understand and to develop their own views based on that is what we tried to use in this process.

So we bring in proponents and have them present their views. And we know that they are different. And we have liked to identify the differences.

But the members of the panel have to be evaluators. They have to evaluate the credibility of those hypotheses and those models relative to the data they have available.

And so we will hear what Gene has to say. And we have heard what Frank has to say. And people like Chuck and Bruce have to evaluate those hypotheses.

This is much more work than goes typically into an expert elicitation. In expert elicitation, the guy usually has to get ready, reads about the agenda on the way in the plane, and then sits down. And you elicit his judgment. This requires -- and they will attest to this -- requires much more work.

So to evaluate the hypothesis, to consider conceptual model uncertainty, as Bruce said, is a very important part of the total uncertainty.

Let me step through a couple of other issues on SSHAC. And then I'll move forward. There is a role that I have been able to play in a couple of assessments like this called a technical facilitator integrator.

Facilitator is obvious. You have to herd cats. You have to get through agendas. You have to make sure topics are covered. But integrator is also an important part of this.

As you saw before, some of the problems

with previous studies had to do with aggregation methodology. Did you start out at the beginning and say how you're going to aggregate across this panel? Is it equal weights? Are you going to use a behavioral scheme, a mechanical scheme? How will you do it?

And SSHAC recommends that, in fact, a goal of these studies should be equal weights, but you do have this issue of the outlier expert. You need to be sure that that outlier has considered the broad range of views in the technical community.

You need to have an opportunity to, in fact -- let's say that expert who is out here is one of five. Right now he's giving 20 percent weight in an equal weighting aggregating methodology. Is that appropriate relative to the community?

You have the larger community there and 100 people. You know, would you have 20 people who would agree with this view? If not, the TFI is able to actually apply differential weights to allow for that.

So this component of the integrated role of the TFI is something that was the most controversial aspect of the SSHAC discussion, the fact that, in fact, experts, individual experts, can be given different weights depending on a set of criteria is something that is worked into the plan. Fortunately, we have not had to do anything other than equal weights because that has been our goal.

A couple of things I just want to say on the steps in elicitation. I will show a couple of examples just to show that, in fact, now the basic steps in a structured expert elicitation are about the same.

If we go back to these studies back in the early '90s, they set up the concept. We need to have an explicit process for selecting the experts, organizing the assessments, deciding what exactly you are going to be eliciting very specifically if you can, preparing. This has to do with training of the experts, cognitive training. There's probability training as well as the technical process and the expert judgment documented.

This is the simple sort of set, minimum set. And then the NRC came out with its branch technical position on expert elicitation. This was being done about the same time that the PVHA-96 was done. And we feel in looking at this now that we're consistent with all the -- certainly with the spirit of this branch technical position, if not all of the detail.

But this lays out in better order and more detail this concept of working from objectives, to selection, to the issues, to getting information to experts, training, elicitation, feedback. We talked about how important that is and aggregation.

This is the process that was followed in PVHA-96. Jack basically has the same. We call them the seven points of light. That's basically the same steps. PVHA-96, you can look at it, the same type of process of working your way through from the selection to the data; in this case, workshops, a series of workshops that would introduce them to particular data sets, either in the field or in a workshop environment, bring in proponent experts; then training, elicitation; feedback; and finalization of the process.

I would say in the PVHA update, we're using the same basic process. It's one that now would be the minimum set of steps that are required to carry out an expert elicitation.

A couple of things that are also important. The NRC branch technical position on expert elicitation says, "When do you do these expert elicitations?" We had some discussion today about we have reached a point where the uncertainties -- maybe they have been narrowed as much as they are, but the next speaker says, "But they're huge."

What are the criteria that would say we should proceed with a formal structured expert elicitation? If there are sponsors in the room, people like Eric Smistad, and others who have to pay for these, it's a big decision. These take a lot of time, and they cost a lot of money.

And typically the criteria look like this. Empirical data are not reasonably obtainable. We can't go out and gather data and answer this question directly.

The uncertainties are large and significant. This is very important. Often we can argue that, "Geez, the uncertainties in certain aspects of TSPA are very large but not significant to perhaps the post-closure compliance case."

The one conceptual model can explain things. As we will see and discuss today and tomorrow, we have multiple conceptual models. And technical judgments are required to assess bounding assumption calculations.

Well, we started with that back in the early '80s, some bounding considerations on this, and found that, in fact, it's not a proper way to treat volcanic hazards. So, rather than just meet one of these, we meet virtually all of them, I would argue. And that's the reason we went forward with this in the first place.

I've got to say, you know, PVHA-96 was published in '96. These criteria came out in '96. But I know Janet Kotra and Norm Eisenberg attended our workshops. And we had interactions with them along the way, too.

Jack says basically the same thing. I'll let you take a look at that. I would agree with everyone that the risk triplet, we're covering two out of three of those things today.

I do want to point out that the issue of what can occur and the tieing, the linkage of igneous event definition, either dikes or eruptions, to recurrence and to spatial models is a key aspect.

It's well-recognized. No one should think, in fact, DOE hasn't gotten the message that that linkage is important. I think John Trapp was saying that about 12 years ago. So we've got that message. And that is something that is being considered very closely.

Again, these are the basic elements I want to move in. A couple of things that I do want to mention, we haven't had a lot of discussion about the variability, aleatory variability and epistemic uncertainty.

I want to just point that out, that in these assessments, we are trying to make a separation of these two to the extent that we can. Aleatory variability is random variations that are not reducible. If we say, for example, "At this location, what do you expect the distribution of dike azimuths over the" -- if you had 1,000 dikes, what would be that distribution?

If you're uncertain about it only but it will have a single orientation, then it will end up being a single number over 1,000 simulations. If, in fact, it varies, truly varies, and you might have uncertainty as well, but if it truly varies, that is variability. And that is aleatory. And we don't expect it to be reduced.

So some of the discussions that we have had and separations when we do feedback and look at sensitivity, the issue of aleatory variability, which is not reducible, and epistemic uncertainty, which potentially is, will be important.

We're always going to be hearing from

Chuck and Bruce and others that we should have drilled more. I mean, they're responsible for quantifying uncertainty. Why wouldn't they want uncertainties to be reduced? But the issue is, how much further would they be reduced? And how significant would those reductions be? And how much would they cost? I mean, I don't have to worry about that, but I know that that is what goes into these assessments.

Now, epistemic uncertainty is reducible. And the question of whether or not it's reducible and in a cost-beneficial way is valuable is something that we'll have to look at when we have the results. We can do value of information studies and other things to look at the potential benefit of reducing uncertainty. But for variability, it simply is not going to be reduced. It is what nature gives us. And we have to live with it.

I won't even go into the tools. There are all types of tools that we use for quantifying uncertainty. They have all been fairly well-developed now for this type of application. I do want to look a little bit at the PVHA-96.

I know we gave a summary to the ACNW, two summaries back, in '96 after this was over. I think, Bruce, you might have been there. There's a couple who were there at that time. And we went through the process at that time of what we did, how we covered it, and what the results were.

The product -- let's be clear. It's a probability distribution, the annual frequency of intersection of assaulted dike repository footprint. So it was a dike. We used a dike. And it was simple. And it had an orientation. It had a length. And because of the place where it was centered, if it was long enough and oriented properly, it would intersect the repository footprint.

If it certainly started directly beneath the repository, it would intersect. If it was some distance away, it would be a function of azimuth and dike length. And that was it. That was the focus of that assessment, was that type of event definition.

Here is all of the attributes. I just want to show this again to show you a couple of things to talk about all of them. One of them is the selection of the expert panel to start with, just an example of some feedback that was given to the experts to give them an idea of their assessments.

The first is this expert selection. How did we come up with this pool of candidates or how did we go through a process that got us to ten candidates, PVHA-96?

First, we had a pool of candidates that was established by sending letters to acknowledged leaders in the field. I think that larger pool was 70-some potential candidates.

We then went through a set of selection criteria that are of the usual kind with a couple of exceptions. I want to talk about those, recognized competence in the field, tangible evidence of expertise through publications and so on, understanding of the problem area, both with experience, both in the great base or other extensional environments.

This one, availability and willingness to participate as a panel member, including a commitment to devoting the necessary time and effort, willingness to explain and defend technical positions, is an important one. You wouldn't think so, but it turns out it is.

The people you get, the people of the caliber of these gentlemen over here, are very busy and have many things to do. And they can barely tolerate their existing schedules. When they, you know, are able to overcome the resistance to participate in this type of project, when they say, "Yes," then you need to turn around and say, "We need your commitment to this, all the workshops, all the work."

And we did end up losing a couple of members of the panel this year in the update, one who decided to take retirement seriously and to spend more time on his lake in Wyoming; and the other, who was simply over-committed and could not devote the time to this. On mutual agreement, these are the criteria for both selection as well as continued participation. And they had to separate from the panel.

The issues that related to personal skills, communication, interpersonal, are simply because a big part of this process is interaction. It's discussion. It's a process of not just sitting there but basically saying, "Here are my ideas. And here is why I think this uncertainty expression is better than yours" or "This is what you have left out in your discussion." That type of process, of course, it needs to be moderated and facilitated, is a valuable part of the learning experience.

Let me go to -- this was our panel at that time. Two members have passed away, unfortunately. It was a very strong group, very lively, contentious group on the outcrop, I found.

In fact, in other studies on seismic hazard and some international work, I always explain to them what goes on when you get a group of volcanologists together and try to deal with polycyclic and monogenetic on the face of a cinder cone. And everyone in the world enjoys that discussion because they look at these contentious volcanologists. But, in fact, it's the way they are. They're used to that type of argument.

Some of the important aspects here -- and they are still the same as we go through this -- are the temporal models and spatial models. In both cases now in terms of this update, we'll have a little bit more on the nominal homogeneous models. We talked a little bit about clustering and so on.

These basic elements have become fixtures in the PVHA process as we go through. And I think these are the types of assessments that we had in '96 and we'll have in this update. The way this is structured is we go through a process of elicitation, formal interviews.

We then take those preliminary models and run them through the whole calculation, not only the calculation, final calculation, hazard, the interim calculations of recurrence, spatial, intensities, and so on, and feed that back to them in a feedback workshop. In fact, we have one coming up on May 10th and 11th if people want to put on their calendar for the update. That is always the most spirited and most enjoyable type of discussion.

Here are just some examples of alternative. These are four experts, alternative source models. At that time people enjoyed the concept of separating spatial regions that might have one set of recurrence characteristics from adjoining regions with a different set.

The fact that the Yucca Mountain block, repository footprint was different at that time is not in one of these zones simply due to the fact that it's in another zone, none of the experts said that the probability of future volcanism at Yucca Mountain is zero. In fact, it's simply a process of identifying spatial variations and intensity of future events.

And that is a spatial part of this problem. The exact numbers in terms of the rate in this place versus that place is a temporal part of the problem. It's very similar to seismic hazard analysis.

Those were source zone-type models. This was a model that one expert has that says that fields should have a bi-Gaussian shape and they fit parameters to the events that would exist in this case in the Yucca Mountain area.

Others, this was a new approach. We had a young upstart kid come in from the center and give us a discussion of this, some Chuck Connor guy. It turns out this has now become very strongly endorsed by members of the not only -- I don't know if you realize in the seismological community, all of our national hazard maps now use spatial smoothing. It's now viewed as sort of measure of spatial stationarity.

Our degree of belief that the pattern of past events, either earthquakes or volcanoes, tells us about the future pattern is a function largely of elements of this model. Smoothing distance and other components are quantitative expressions of your degree of belief in those models. It's very appropriate.

The types of approaches that were used in '96 are largely logic trees for uncertainty, quantification. So for a given assessment in the model, there are alternatives. And those alternatives are weighted, discrete alternatives. They can be continuous. We had continuous PDS or discrete values depending on what the expert likes to use.

The advantage of the logic tree is it

allows you to have alternative conceptual models as well. We say, "Okay. I'm going to believe this model with this weight and that model with that weight and that model with that weight and not choose one but incorporate them all and then to be able to go back and look at the impact of those conceptual models on the final results." That is I think important of the comments that Bruce Crowe made about the importance of conceptual models.

Examples of sensitivity. We might say for different -- here's a case where all the events here this expert is showing are the dark triangles. The smoothing over those events as a function of different smoothing distances, this is basically like the standard deviation of a Gaussian kernel.

As it gets bigger, you smooth those over larger areas. And you can get an idea, then, of the impact in terms of the repository rate at that location as a function of the smoothing distance for your set of events.

Likewise, some of the sensitivity was given in terms of the actual hazard, potential hazard, results. Here that was a relative frequency at the site. What will it mean in terms of the annual frequency of intersection, which is plotted here, shows just probability mass functions for different smoothing distances.

So however many years ago, 10-15 years ago, arguing with Leon Reiter, who is here, that, in fact, the people doing work on the front end of these models don't need to see the hazard results. In fact, that may cause them to want to turn the knob a certain way. And, in fact, an expert on dikes and dike azimuths isn't an expert on probabilistic hazard results.

Through the years, I think Leon has proven me wrong and him right that, in fact, it's important for them to see the implications to the hazard results. I haven't seen anyone complain about it. And if you don't show it to me, everyone is going to ask about it.

So how important is this, for example? Smoothing parameter, what you could show me in terms of other characteristic, frequencies of events in certain regions, when you finally show it in terms of the bottom line, it tends to get their attention more. And, in fact, it allows you to be more risk-informed. You're really talking about the things that really move the needle at the hazard level. So we do show results in terms of hazard, but we try to focus on interim results as well.

And, of course, this is the final result you've seen many times of the overall study. These are individual experts, their means and medians. And I think it's probably 5th to 95th percentile ranges. And, lo and behold, we're dealing with two to three orders of magnitude variation uncertainty in this measure of hazard, not really that uncommon. It's a little bit bigger than a typical seismic hazard, but it's also at an annual probability that's lower than typical seismic hazard.

For PRAs, we'll go as low as 10^{-7} usually for seismic, rarely down into this range. And, of course, as you go lower annual frequencies, the uncertainties get broader. And we're down here in a place where the uncertainties are large and the probabilities are low.

A couple of things. I put in some slides in here which I view as more programmatic. Why did we do the PVHA update? There's a series of slides. And the references are given in the back of the decisions that were made along the way.

New data came out, the short of it. We did an evaluation of sensitivity. We didn't think that, in fact, PVHA-96 would change very much. The

NRC disagreed with that and said, "This could change conceptual models." DOE said, "We'll do some work. We'll gather some data. We'll do new drilling. We'll do Aeromag." And we'll reconvene and update the PVHA. So that's what we've done. That's what we're doing. I'm sure there's a lot more politics in it, but I will leave that to others to explain.

I understand that you have been briefed by Frank Perry on the Aeromag program, the drilling that's gone on. So I won't get into that other than this is one chance to show a couple of nicer pictures than I have been able to show up to now.

Like I said, there was a concerted effort to not just go out and start drilling. We knew at the beginning of this that it would be difficult in this project at this stage of development to justify a massive data collection program.

We were able to get as much as we possibly could from the dollars that were available to us. We prioritized those. We ran the priorities by the panel.

The types of drilling, the types of targets that would lend information not only to that particular target, to those adjacent clusters of Aeromag anomalies, and tried our best to get information; for example, you know, drill a hole here and have some information related to the adjacent anomalies.

Frank can go through this in a lot of detail, but we did a process. Both in the high-res Aeromag and in the drilling information, the analysis has gone on since in terms of age dating and geochemical analyses and so on to try to get the information that will give us the most bang for the buck in terms of dealing with uncertainties in the PVHA. So we'll see how that goes.

The question always comes up, "Where are you going to be?" We will have specific comments on the draft report because we have a couple of places where we would say there has been some conjecture about, in fact, numbers going down. We are not going to join in that conjecture at this point. We don't know where they will go relative to positions in the past.

I had a couple of other slides that just relate to the update, what we're doing, and the types of data that are being provided. With time and technology, the ability to get information together to a panel like this, to combine data sets, all on the same scale, to do simple types of combinations, GIS and so on, is far and away better than we had ten years ago and certainly ten years before that, the time these big elicitations were done.

So I think we have come well along the way in this particular area. I have been involved recently in some work over in Switzerland, a comparable type of study for nuclear plants over there. And I am just aghast at how much information can be represented and displayed and distributed to a panel in a short period of time. So this is really an area where there has been massive amounts of advance. And it keeps Frank busy and awake.

A couple of other things that also were part of these data sets are analog studies that have been done. Part of the issue of event definition, as I mentioned before, we had a very simple event definition in '96.

And the concept was we're going to need to look at more. We've got to spend more time getting information on things like the number of dikes and lengths of dikes and number of conduits and orientations and what does nature truly give us in these areas.

Those analogs have been developed and put together, put into publication-type form by Greg and his group at LANL. Here are some examples, get information that can be helpful.

Not all of them are accepted by the panel. These may not be appropriate analogs. All the panel themselves bring to bear is their own analogs. We saw some of that today.

I was pleased to see the argument back and forth on certain analogs. That's an area -- I've never seen earth scientists argue more about analogs than volcanologists do because they've all seen something, either in Iceland, Kenya, somewhere else, and it might apply here. So they give the story, and the story is great.

And then the discussion is by the other person, "Yes, but that doesn't apply. The volumes are different. The chemistry is different." And so they go on to the next discussion. So it's a wonderful process to watch.

Earthquake. There's a bit of that in earthquake where people have said, "Well, I chased out and looked at the North Anatolean right after it ruptured, and this is what happened."

But I think volcanologists want to be anchored in what they have seen. And that is a key part of this. And the more realistic, I think what

Chuck called realistic, or geologically based their models are, the better they feel about it. And it feels closer to empirical. But in this area where you have very few events, you have to draw an analog somewhere else.

Okay. One other thing, I want to show what we are doing in terms of event definition in the update. This is just a summary of that series of slides on the issues that we're addressing. For event definition, we're looking at detail of intrusive event geometry, both dikes; dike systems; multiple dikes; if we have multiple dikes, what is their spacing, their lengths; what is the relationship of the dike to the conduit. We are asking this question, is there influence of the repository opening on the probability of dike intersection or of conduit development?

The extrusive event geometry, we're getting into more detail in terms of the event centers, their number, where are they located, and so on. And this is it, the last slide.

I do want to point out the one thing that happened between '96 and the update is we have now a future time period that could either be 10,000 years or one million years. And we're asking for assessments for both of those in the update.

That's it.

MEMBER HINZE: Thank you very much, Kevin. We appreciate that.

We do have a few moments for questions. And I am going to start with Allen Croff, if I might, please.

VICE CHAIRMAN CROFF: Thanks. I think I would first like to start -- I am going to focus on the '96 exercise since it's down in the record. One of our earlier speakers this morning noted the importance of assumptions made going in.

And in the '96 study, the report was relatively terse. But what I took away from it is that an assumption was made that events were random in time and occurred at a constant rate over the period assessed, whether that was a million years or whatever database you happened to be using. Is that what was done?

DR. COPPERSMITH: Well, number one, it wasn't an assumption. We asked them for -- this is part of the temporal modeling. We asked them what model they wanted to use for a temporal distribution of future events.

Almost all of them use a homogeneous Poisson assumption. There is one exception. That was a time-volume relationship that Rick Carlson used to take into account the decrease in volume over time and, actually, the rate of decrease in the cumulative volume over time and the rate of decrease in the volume per event over time. With those decreases in different rates, you end up with a more --

VICE CHAIRMAN CROFF: I was talking more about frequency, not --

DR. COPPERSMITH: These are a frequency. These --

VICE CHAIRMAN CROFF: Oh, I'm sorry. I thought you said --

DR. COPPERSMITH: These are all temporal. I think if you broke it out, I would say, by and large, the homogeneous Poisson distribution was strongly used by all experts.

VICE CHAIRMAN CROFF: This morning we saw a couple of graphics that showed the cyclic nature of volcanism in the area. Was it a one or two million-year period, I think, something like that.

How does the assumption made in '96 reflect that cyclic nature?

DR. COPPERSMITH: It depends on when you then start your period that you will be using for your Poissonian model.

VICE CHAIRMAN CROFF: I understood in '96 that the periods were relatively long, I mean, millions of years.

DR. COPPERSMITH: Well, if you look at the periods of these, episodes, if you will, go back millions of years.

VICE CHAIRMAN CROFF: Right.

DR. COPPERSMITH: So from 11 million years working your way towards the present, there are periods of higher rate that will go on for one or two million years, separated by more quiescent periods for one or two million years, followed by other.

So as they started at the present -- and the future, 10,000 years is relatively short -- they would then gather events in the past and use an assumption of either Poissonian or time-volume change.

Typically the highest weight was given to the most recent events, the million years to the present. They would say, "Oh, okay. Within that time frame, the Poissonian assumption tends to work." Now, as we move back in time, we get into more of this episodic type of behavior.

Typically the farther back in time, either in the Pliocene or even in some cases the experts use Miocene events for temporal, they were given lower weight, primarily because of the issues related to the number of events and some of these issues of stationarity or the applicability of a Poissonian type of model.

VICE CHAIRMAN CROFF: Can I take you back to your slides on page 32? In the report and in discussions, there's been a lot of focus on the mean, --

DR. COPPERSMITH: Right.

VICE CHAIRMAN CROFF: -- almost exclusive focus on the mean as the metric, I guess. And we happen to be in a very sticky situation here where the mean is slightly greater than a cutoff value and the median is slightly less than a cutoff value.

DR. COPPERSMITH: Right.

VICE CHAIRMAN CROFF: Why the emphasis on the mean or -- in a sense why the emphasis on the mean? Let me just leave it at that.

DR. COPPERSMITH: Well, others who have studied the regulation more than I probably should respond to that. My feeling is a person who has been involved in these types of studies and decision analysis for a long time, the mean is by far a better risk measure. Ultimately the median is more stable in many of these problems. I could have Leon talk to this as well, Leon Reiter behind you.

The mean, though, incorporates and is often very sensitive to the uncertainty distribution. And I think in regulation, there's been a desire to incorporate not only just the central characteristic, the median, but some explicitly incorporate the uncertainty. And that's what the mean will do.

VICE CHAIRMAN CROFF: That was my memory. I thought I remembered a recent example, not on this subject area, completely different, before this Committee, where the finding was the opposite. They determined that the mean was too sensitive and, therefore, decided to use the median for --

DR. COPPERSMITH: That's a constant debate. Alan Cornell calls that the tyranny of the mean. We do have problems. In many cases, highly skewed distributions that are very sensitive to one or two extremely low probability of parts in the distribution.

VICE CHAIRMAN CROFF: Do you know of any conservative assumptions that were in PVHA-96?

DR. COPPERSMITH: Not explicitly. We tried hard not to have conservatisms or optimisms built into these models. The goal of the previous studies that have been done in seismic showed that, in fact, there is no value of sneaking in a conservatism here or there. And ultimately it is more problem than it's worth.

So I don't know if there are explicit conservatisms, none that we tried to put in deliberately, or optimisms.

VICE CHAIRMAN CROFF: Okay.

DR. COPPERSMITH: We tried to avoid that. The basic philosophy is to have a mean-centered approach.

VICE CHAIRMAN CROFF: How many do I get, Bill?

MEMBER HINZE: You get another one.

VICE CHAIRMAN CROFF: I get another one. I noted in one point in reading the report, it said something like "Some of the source zones." And I assume that relates to that map of regions and zones you had up there. It didn't contain mapped events. So the experts used other means to specify the rate of events.

DR. COPPERSMITH: Right.

VICE CHAIRMAN CROFF: What does that mean exactly? I mean, why would they feel compelled to find an event where there wasn't one?

DR. COPPERSMITH: Well, simply the record may not be long enough or complete enough. Some of the places that would be included, for example, that show no events might not have been well-studied enough to have found them over different time periods. So the typical types of what we call a background rate in that case come from larger regions where you know this region, there has been enough study to see that we have a background rate that provides a lot rate.

There's no reason to think that our local background is any different than that. The southern Basin Range, for example, it would be a reasonable background rate.

It would be more of a lead to say that, in fact, the absence of mapped events in this zone means an absolute zero in terms of a forward hazard assessment.

I don't think any of our experts felt, in fact, that this area in the local Yucca Mountain repository area, was devoid of any volcanic hazard. In other words, there were regions you could say it is zero.

And I think that's why they would need the -- okay. Let's look over a bigger area, where were have more opportunity to find these widely scattered rare events and use that as a background rate.

VICE CHAIRMAN CROFF: Okay. I have used my time.

MEMBER HINZE: Dr. Ryan?

CHAIRMAN RYAN: Yes. Thanks. Just one in the median versus the mean. To me, I think it's important to try to figure out one does a better job at the central tendency.

DR. COPPERSMITH: Yes.

CHAIRMAN RYAN: And that's really the way you are trying to avoid some of those conservatisms or optimisms. You know, that's not an easy task. I think that is the important point, if you like one and don't like the other and you're trying to have a bias in the result. You do it numerically.

I think the central tendency idea is why one versus the other needs to be --

MEMBER HINZE: Mr. Coppersmith?

CHAIRMAN RYAN: If that is your explicit risk, risk goal, is central tendency. All right? If it is phrased that, in fact, we are looking We are looking for a risk goal, the probabilistic side of the risk in this case. That is, central tendency, then yes.

CHAIRMAN RYAN: And I'm just speaking kind of on differences, too.

DR. COPPERSMITH: Well, goal is a conservative estimate of something.

CHAIRMAN RYAN: Absolutely. And I'm just trying to probe. Is that your understanding, the difference between the mean and the median is that they're both potentially useful ways to express central tendency? And, of course, that's dependent on the data set you're manipulating.

John?

DR. TRAPP: Yes, just one basic thing. The mean in the rule is the performance measure by metric that you use --

CHAIRMAN RYAN: Fair enough. I'm not arguing that.

DR. TRAPP: -- for the reasons you've got going there. Yes, you want at look at rest.

CHAIRMAN RYAN: Right.

DR. TRAPP: But the mean is really the one that's --

CHAIRMAN RYAN: I'm with you that, but I just wanted to clarify for my own benefit what we were talking about when we were talked about median versus mean, just to be clear about it. Thanks. Thank you.

MEMBER HINZE: Dr. Clarke?

MEMBER CLARKE: Okay, Bill. I was

interested in this slide as well. And this discussion addressed my concerns.

MEMBER HINZE: Dr. Weiner?

MEMBER WEINER: I'll start out with a comment, a quote from Lee Merckhoffer on your outlier experts. He said, "Sometimes everybody is over here and one guy is way over there and he's the one who's right."

DR. COPPERSMITH: He's right.

MEMBER WEINER: But my question is this. Here you have a group of experts. And I heard Dr. Smith before talking about one point of view and Frank Perry talking about another point of view.

I have no personal knowledge of how these probabilities are arrived at. And I can't go back and look at all the evidence that goes into it. What is your recommendation to someone like me who sees these opposing views, recognizes that there is evidence for all of them, recognizes that all your ten experts are indeed experts, assumes that they're all honest and giving their honest perspectives? How do we make a judgment?

How does someone looking at all of this make a judgment about in this case the frequency or probability of a volcanic event that would affect the repository at Yucca Mountain?

DR. COPPERSMITH: Well, let me first make a distinction. These discussions, like Gene's discussion and Frank's discussion, are ones that we would call proponent views. They are advocating a particular point of view based on the line of evidence and information that they have.

The experts on our panel -- and there's only eight, not ten, maybe more.

MEMBER WEINER: Okay.

DR. COPPERSMITH: -- have a different function. They have a different job. They're allowed to put on the proponent hat when they want to and as long as they say they're going to.

Their job is to evaluate these hypotheses. They have to say, "Okay. I've heard Gene talk about this and his deep model and shallow model. I've heard Frank talk about this. Now I'm in the process of, let's say, developing my spatial model that I have to do for PVHA." These may or may not be important to PVHA. That's an assessment the experts need to incorporate.

And when they are developing their assessment of uncertainty, let's say, in conceptual models related to the location of future events or in this case, I think it really affects the temporal model more. They'll say, "Okay. I'm going to look at whether or not future distribution of events will follow a Poisson process or an episodic process. What do I know about this?"

Gene says he sees an episodic process in his data set. And I'll look at that and study that. I see other places, and I see evidence of an episodic nature. Even here locally I might. And I will construct the model that incorporates that.

The only way you can see whether or not, in fact, these experts on the panel have considered those alternatives is by reading their final documentation and run a search on the publications and the information that we're presented here.

We can demonstrate in our discussion in the report that we have provided that to the experts. We can document slides and other things presented to them and papers given to them, but it's the expert who has to document that, in fact, he considered it.

He might have said, "It's a bunch of hooey. I don't buy it," but they might have said, "Well, there are certain elements of it that I will include in my assessment." And I think that's the only way for you or anyone to independently look at this to see whether or not, in fact, those conflicting views were incorporated.

MEMBER WEINER: Well, if I were to do that and say, "Yes, these experts have looked at the entire field," do I then reach the conclusion or is it a logical conclusion, then, to say that the combined consensus, mean, if you will, if the PVHA is a better indication of these probabilities than these individual things that I have heard of? In other words, you would give more credence to this?

And then I look at the lower part of your graph.

DR. COPPERSMITH: Right.

MEMBER WEINER: And I see that in at least one case, two of your experts differ by more than an order of magnitude.

DR. COPPERSMITH: Yes.

MEMBER WEINER: And how do I incorporate that into a decision or do I just look at the mean or the 50th percentile or whatever?

DR. COPPERSMITH: This is reflecting -- and you will see where we are with the update, but I think it will be comparable. This reflects these experts' assessments of the state of knowledge when this was developed, the alternatives, the credibility of the alternative conceptual models.

The arguments that's made in SSHAC -- and there is good reading on this issue of consensus -- says that we can start with multiple experts who agree on the same value of a parameter. That's ultimate consensus.

We can get experts who will agree that a probability distribution, the same probability distribution, applies to that parameter. That's a different level of consensus. We can get a group that develops alternative of uncertainty distributions that says, "As a whole, this represents the community."

That's the level we get. That's the best we can do in these fields. We simply will not get -- until they solve some of these uncertainties in this particular field, we will not get to where people agree to a single parameter value or uncertainty distribution. We will have to live with a composite of multiple experts. That's sort of the conclusion in SSHAC.

MEMBER WEINER: Thanks.

MEMBER HINZE: Thank you very much, Ruth.

With that, we will close the discussions. And I will pass it back to you, Dr. Ryan, for adjournment and reconvening at 1:30.

CHAIRMAN RYAN: Thank you.

And we will reconvene at 1:30. And thank you all for a very interesting morning. I hope the next day and a half will be just as interesting. Thank you.

(Whereupon, a luncheon recess was taken at 12:42 p.m.)

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

(1:36 p.m.)

VICE CHAIRMAN CROFF: At this point we're reconvened in the afternoon session. And, without any further ado, I'm going to turn it back over to Dr. Hinze for the afternoon portion.

MEMBER HINZE: Thank you very much, Allen.

This afternoon we will be hearing comments on the white paper from various stakeholders: the NRC, the Department of Energy, Electrical Power Research Institute, and the Clark County will have an opportunity for making their thoughts available to the Committee regarding the white paper.

Without any further discussion, I will ask the NRC to begin their discussion. And I would like to introduce Jack Davis, Deputy Director that's associated with us. Is this NMSS or --

MR. DAVIS: Yes.

MEMBER HINZE: The NMSS.

MR. DAVIS: Deputy Director of the Technical Review Directorate for the High-Level Waste Program at the NRC.

MEMBER HINZE: Jack, we're pleased to have you here. And we're interested in hearing what you have to say.

NRC PERSPECTIVE ON IGNEOUS ACTIVITY ISSUES:
OVERVIEW OF THE LICENSING PROCESS,
DEVELOPMENT OF NRC REVIEW CAPABILITIES, AND
PROBABILITY OF IGNEOUS ACTIVITY

MR. DAVIS: Okay. I thought that what I would do -- this presentation is actually in two parts. I'll give the first presentation. And basically it's on roles and responsibilities of the various entities, the licensee, the regulator, the advisory groups, and so on, so that all of the stakeholders understand how all these things play out; also what we would expect in the license application with regard to igneous activity.

And then the second half will be given by John Trapp, my senior geologist. And he will go into a lot of more detail on what we have done over the past few years in developing our review capability in the igneous area.

I'm sure that the folks here understand that the Waste Policy Act of 1982 established DOE to build a permanent repository for high-level waste. We promulgated our regulations in 10 CFR 63. And as part of those regulations, DOE is required to conduct a program of site characterization. Primarily this is to look at the geological conditions, look at a range of parameters that are appropriate to the repository.

The regulations also require that DOE meet certain post-closure performance objectives that limit the amount of radiological release to the public and also to the accessible environment. And then, of course, they would have to prepare and defend their license application.

I think it's important to realize that over the time periods that we're talking about and the uncertainties that we're talking about is not deterministic, as we all know.

So they have to make a reasonable determination of safety over the compliance period. And we will certainly evaluate that and challenge them on certain areas if we don't feel that there is sufficient data. Obviously if we license the facility, then DOE would operate, construct and operate, the repository.

With regard to NRC staff, our congressionally mandated role here is that we have to review this and license it. And as part of that, we had to develop our own technical understanding of these various areas, like igneous activity and then develop a review process to do the license.

We have held a number of prelicensing

interactions with DOE on igneous activity. We have even challenged them on numerous times in the past in some of their early models, on some of their early data. And to help us understand that better, we also conducted our own research in the igneous area and developed certain models.

I think it's important, though, to point out that just because we developed certain models, certain tools that help us review doesn't mean that we have actually come to any conclusion on igneous activity. It's just helped us further to be able to look at their data, be able to challenge them on certain of the areas that they have put forward. The actual review, the official review, won't occur until DOE actually submits an application to the NRC.

The only thing I wanted to point out here for those interested stakeholders is how these various advisory groups, like the ACNW, the ASLB, factor into the licensing decision that is going to occur.

Certainly the ACNW reports to and advises the Commission on all matters related to nuclear waste management, but it's important to note that they are independent of the NRC staff and the review that we have to do with regard to the repository. And so they will advise the Commission, but they don't actually render any decision regarding the licensing of Yucca Mountain.

Likewise, the Atomic Safety Licensing Board will hear from the public, from other interested stakeholders any contested issues. They will render a decision on those contested issues. But, again, those decisions are provided to the Commission, which ultimately makes a determination on whether the repository can operate safely.

Going over to what we would expect to see in a license application with regard to igneous activity, we're going to expect it to have a transparent and traceable technical basis and then also a quantitative performance assessment of how the repository will perform over the compliance period.

Certainly certain events can be excluded if they're considered very unlikely. We do require that the events be assessed if they have at least one chance in 10,000 of occurring over 10,000 years. And then DOE would have to evaluate for uncertainty the variability in the data of the certain events that they were looking at and, of course, looked at the risk significance.

I don't have to tell this group here that the models are complex. The data is limited, as we heard this morning. And so it's not, again, deterministic. It's something that DOE has to put together. They have to have some kind of basis. We would look at that basis.

Yes, we have developed tools. Yes, we have done some research in the igneous area but, again, only to inform ourselves so that we can ask the right questions.

The regulations also require an alternative conceptual model to be considered by DOE. Tim McCartin is going to talk to you a lot more about this tomorrow. The only thing I wanted to say here was that obviously, as you hear in the various views on igneous activity, these things can be factored in to a conceptual model that is different than maybe the one that the NRC has looked at in its own models and analysis. That would be expected.

And we would review that for appropriateness of the data that is being used to provide that conceptual model and, of course, demonstrate model support, as I just discussed.

The regulations, however, don't require DOE to predict an igneous event. What we're asking DOE to do is to forecast a range of outcomes. Obviously there are uncertainties involved. And they would have to consider those uncertainties. And we would look at how those uncertainties are factored into the actual analysis.

We have heard again that there have been many different views here. That's actually good, and it is to be expected. And we will look at what is provided by DOE when the application comes in. And from there, we will assess whether we think there is sufficient data that DOE can provide a reasonable expectation of compliance for the repository.

Again, I just wanted to drive home the last bullet there, the fact that we don't have a position on igneous activity. We use the data to help inform us, to ask the right questions.

With that, I am going to turn it over to John, who is going to take you into a lot more detail on some of the activities that we have done to develop our capabilities for review in the igneous area.

MEMBER HINZE: Fine. Please.

DR. TRAPP: I want to go very briefly into risk significance. And the point I would like to make here is in general NRC and DOE have a kind of similar view on this. We all agree that it's a low-probability event. It has the possibility of being very high consequence. And we feel it's got high risk significance.

If you go through things like the risk insight baseline report, you will see that there is estimated risk significance for all the various processes. Probability, for example, is considered of high risk significance. Britt tomorrow will be talking quite a bit more about the risk significance of the various consequence subissues.

Using this risk significance and all of that, we have gone through a KTI process and basically used this to figure out the questions which we thought needed to be answered to get DOE to help produce a successful license application. By "successful," I don't mean it does or doesn't or thumbs up or thumbs down. I mean, will they have enough information to be able to provide a license application?

I am going to take a brief walk down memory lane on a few of these things, probability, airborne transport, and magma drift interaction, just to show how things have kind of fallen together through the years. Then I will go into a little bit on probability and where we sit on that.

If you take a look on the NRC staff review capabilities, probability was one of the first ones. This was basically because if you kept a look at old

10 CFR 60, probability was really much more important than the consequence, the way the whole thing was set together.

If we took a look at where we were sitting in the early 1990s and a lot of this before that, Bruce Nyevski was the author of many of these things. There is some question on the traceability of the data.

Some of the models suggested that you might be able to screen igneous activity out of consideration. And if you took a look at things like our site characterization and study plan comments, they really focused on the need for DOE to consider alternative models and a broader range of site data. One of the places that we talked quite a bit about was again in the geophysics, which was brought up by Chuck Connor.

We also noted there was a range of interpretations possible and in available models we didn't feel adequately incorporated geologic data.

We needed an independent understanding to be able to evaluate this. So basically when I'm talking about "staff" here, it's NRC and CNWRA. And Chuck has mentioned there were the Connor and Hill papers all the way through that really got this thing going. The Hill and Stamatikos is basically the last one that we put on discussing this whole thing. So a lot of this, a large portion of this work, was done through the center.

Again, we developed these models and felt they were traceable. They helped us get the key technical issues or ask the questions of DOE that we felt needed to be asked. They provided tools for evaluating this new information; for instance, the information on the aeromagnetic data, and take a look at alternative conceptual models. And also we could test this against alternate analog fields.

In the change from 10 CFR 60 or when it was remanded as far as the site goes, we went to 63. And there was a change at that time going from release into accessible environment, which basically meant all you had to do was get the waste up to the ground surface.

If you took a look at the way this whole thing appeared to be going from what you saw in the mid '60s from the review counsel, et cetera, they were talking about dose at the site boundary, et cetera. And it appeared to be something on the order of 20 kilometers.

There wasn't any acceptable model at that time to talk about airborne transport from basaltic volcanoes and definitely nothing to talk about transport of waste in this ash. So we needed an independent approach.

Basically we took a look at a whole series of different models that were available; Pop, et cetera, is one, Suzuki model. From that, we developed what is known as the ash plume model, which to us appeared to be the best way to take a look at this thing. At least we felt comfortable using it.

We're able to test this model against alternate fields, analog fields, such as Serra Negra. And then we incorporated those model into our TPA model.

Has it improved our technical understanding of the field and, again, allowed us to ask questions of DOE that we felt needed to be answered for them to get to the license application?

We're still working on this model. It's being updated to accept the full wind field. That should be hopefully done fairly soon.

One of the areas that has been discussed quite vigorously is the area of magma drift interaction because, again, we expected that there would be a change from the straight release standard to the dose standard. And there really was nothing in the literature that could allow you an independent way to evaluate the complexity of some of these possible interactions.

We were also concerned that at that time DOE was not really addressing this issue. Anyway, we developed the models -- these are some of the Woods models, all these others -- to evaluate the risk significance concerns with the program, get our review capacity up and get a technical understanding.

And these models do provide a technical understanding that we can take a look at the different things that have been done; for instance, Greg Valentine, all the Gaffney work, this type of thing that helps us go through.

If you take a look at where we sit right now in probability, well, we have a few technical issues, the two ones on probability, 1.01, which I'll go into in some different parts, but 1.02 is really a reaction to the whole deal with the airborne Aeromag anomalies.

Basically we took a look at what DOE had done or is proposing to do. And if you took a look at our letter in 2004, what we basically said was the complication of all these planned activities may contribute to a reasonable basis to constrain the uncertainties, at least as far as we constrain the concerns.

So we've got to transparently think, a transparent technical approach to PVHA, PVHA new model. And we have got the tools we need to evaluate.

Airborne transport. Well, we use this again, but we're taking a look on the airborne transport with things like wind speed, how much ash is out there, how this gets in effect, questions on how you actually incorporate waste in there and how it is used to get the correct aerodynamic properties, densities, and this type of thing, tested this thing against a volcanic field, the Serra Negra deposits, and were able to show that you could improve this model.

I'll point out here that, again, DOE is updating the relevant AMRs. We hope to get these sometimes in this 2007 period. And this if you take a look is one of the reasons -- it goes all the way through here -- why we cannot say we've got a position.

We haven't got a position because DOE hasn't told us what they are doing, results of the models. Until we can take a look at this and get to a licensing process, the position is not there. We have got a transparent technical approach to evaluate this stuff. And we're developing the tools.

The same thing goes through when we take a look at the magma repository interactions. Basically it allowed us to take a look at the complexity of the interactions between the waste package and the waste form. The KTA IA 2.19 is basically how magma interacts with the waste package 220. It's how it interacts with the waste form. And 218 is how it interacts with the repository itself. Tomorrow you will be hearing an awful lot more about this. So I'm not sure I need to go into any more detail at the present time.

Again, DOE is updating their AMRs, specifically dike-drift interaction, the magma dynamics are the ones that come to mind. Dike-drift interaction is, what, 450 pages of very detailed complicated analysis. Dynamics may not be as long, but it gets into some very good modeling. Again, we've got a transparent capability to evaluate waste things.

So where do we sit on probability? Well, based on the available information, probability values can range from 10^7 and 10^8 per year. This is basically the mean value, where we think the mean can range from, the possibility of an increase up to an order of magnitude due to past uncertainties. That's where we are sitting right now.

This may change because we haven't gotten all the new data from DOE analyzed. We haven't put it in here. But that's where we think we are.

We have stated that the ongoing work by DOE will help constrain the uncertainties. We're still going through the results of the geophysics for drilling, laboratory work.

And we are using a single point probability estimate for several reasons. One, we're using this to take a look at the different conceptual models. And what we're using it for is a point estimator of a point estimate. What we're doing is evaluating the mean. And we're using a point estimator to evaluate the mean and the change of these models and how it affects the whole curve.

Yes, we've got to take a look at the uncertainty, but we have been using this as our quick way of doing things. Among others, one of the reasons is ease of computation. We can run through these models much, much faster and get answers quicker by using this, rather than doing the thousands of runs you need to be able to get the full-blown probability.

And it's also because we do not do the compliance demonstration. DOE has got to demonstrate the plant. What we have got to do is evaluate whether they have done it. And, again, the mean is the performance measure that we're judging this against.

I was interested to hear that several other people had problems with event definition. This is something that I have had problems with quite a way through. And I don't feel that the report really accurately portrays our concerns.

As Chuck pointed out, you can define these things many ways, but when you are going through the calculation, you have got to be consistent all the way through the calculation. If you start changing the way you're defining means or defining the event and don't use it consistently, you get results that are totally meaningless.

One of the problems I personally saw when you took a look at the original PVHA is stuff like event length, dike length was elicited totally separate from the number of events.

You really can't do that because if you take a look at something like Crater Flat, if it's four events, it may be four very small events. If you're talking about as one event, you can't have it at three kilometers, which is the basic average length of the dikes that come out from the PVHA.

Anyway, when you do this, these events are mutually exclusive and represent alternate conceptual models. And that's what we're going to be evaluating.

In summary, we've got to review the DOE application and see if there is reasonable expectation that they have demonstrated the performance objectives we have met. We have taken these independent evaluations so we can better be prepared to ask the questions.

Prelicensing investigations have provided us with the information we need to get to the point that we can effectively conduct a licensing review of those risk-significant issues.

And DOE, as far as we can tell, is updating all the reference documents and conducting expert elicitation, which will support this. And at that time we will review their products as they become available. And the actual positions that we will be making will be put in the SER.

I think that's the last one except for the required disclaimer.

MEMBER HINZE: Thank you very much, John

and Jack.

With that, I'll turn to the Committee for questions to the NRC, both Jack and John. Dr. Clarke?

MEMBER CLARKE: No questions. Thank you.

MEMBER HINZE: Allen?

VICE CHAIRMAN CROFF: Thanks. I wanted to clarify a point based on something that John said this morning. This is about the median versus mean business.

I understand about the need to use the median when calculating the dose, the mean dose, to the REMI, which is required in the rule. What were your thoughts on mean versus median concerning calculation of the probability or, maybe more specifically, the probability used to compare to the 10^{-8} cutoff?

DR. TRAPP: Basically, again, that's a mean value.

VICE CHAIRMAN CROFF: Because?

DR. TRAPP: Because you're dealing with a rule that is based on reasonable expectation, not reasonable assurance. Basically you're required -- or it's just written into the rule that you will be using the mean. Therefore, we are following this through. That's --

VICE CHAIRMAN CROFF: By "rule," do you mean part 63?

DR. TRAPP: Yes, part 63 in the EPA standard.

VICE CHAIRMAN CROFF: Okay. Thanks.

MEMBER HINZE: Dr. Ryan?

CHAIRMAN RYAN: Let me first apologize for being a few minutes late. We're wrestling with the weather decision. So we have to do that.

MR. DAVIS: Right. We are the only agency still open, right?

CHAIRMAN RYAN: Well, it could be true tomorrow. I don't know. We're working on that. Thanks.

John, I was interested in your pointing out to us that in mid '07 we're going to be getting some information. I had one conversation a few weeks ago with Carol Hanlon. I guess it's going to be the updated and relevant AMRs relative to the airborne transport.

We're hopefully going to schedule, through my conversation with Carol Hanlon I've got some hope that we'll schedule, some briefings on, you know, risk-significant topics. Hopefully this will be some.

So maybe we can agree we'll just keep each other up to date on schedule, whether it's, you know, your meetings with them, with DOE, or our presentations here, and hopefully get the benefit of both of those.

Do you have any other details besides this one set of AMRs on this topic or --

MR. DAVIS: There's a total of six AMRs. Right, Eric? Eric can actually answer this much better than I --

CHAIRMAN RYAN: Okay.

MR. DAVIS: -- because I asked him when they're going to come in. I can't really tell you.

DR. SMISTAD: There's a number of AMRs that will be coming in later in the fiscal year. Dike drift is one of them, the ash plume AMR, magma dynamics coupled in there. So there's a suite of them coming in towards the end of the F.Y.

CHAIRMAN RYAN: It will be real helpful if we stay in contact on the schedule as they come out. I see some heads nodding "Yes." That would be great.

MR. DAVIS: And, plus, someplace in the pipeline I've got another paper that came in from -- Andy Woods is the main author, which hasn't gone through a review yet. That will soon be available to people also.

CHAIRMAN RYAN: Okay. Thanks. That's all.

MEMBER HINZE: Dr. Weiner?

MEMBER WEINER: You are using a point estimate for probability of an event occurring. And DOE is presumably going to present a range of uncertainties. Are you simply going to look at their mean? How are you going to --

MR. DAVIS: No. We are going to look at the total range of uncertainty. We are going to look at the various bases for the uncertainty, why the uncertainty is there.

But what we are doing is a quick calculation to find out what effects do these changes have on the measure of compliance.

MEMBER WEINER: In other words, you are going to look at the range --

MR. DAVIS: Oh, yes.

MEMBER WEINER: -- that was presented to you. You're not going to simply compare it to your point estimate?

MR. DAVIS: No.

MEMBER WEINER: Okay. That was a --

MR. DAVIS: Well, remember, they are the ones who have got to demonstrate compliance. They're going to have to go through. They're going to have to run the thousands and thousands and thousands of simulations to get this total curve.

We're going to be taking a look at that. We're going to find out what's the effect of the various parameters, how significant they are. That's much easier to compare it to a points estimator to determine the significance, to take a look at this whole series and try to determine why this one curve changed when you're looking at so many different variables.

MEMBER WEINER: But maybe you can enlighten me. Wouldn't you have to do a lot of calculations to compare, to look at their answers, to investigate whether their answers are meeting the standard, whether they're in compliance? Wouldn't you have to do that anyway?

MR. DAVIS: Yes. But I can't do it anywhere near as efficiently and effectively if I run the whole thing because in order to get enough samples to show any change, basically they're running this thing thousands and thousands of times.

And that's really -- it's an efficiency method to take a look at this. We don't say that we're not going to look at all the rest, but we're doing this as a post-processor that just allows us an efficient way of going through this.

DR. TRAPP: I think it's important to realize we're in pre-licensing space. And that's why we're looking at some of these.

MEMBER WEINER: I've been told that we're --

CHAIRMAN RYAN: I hate to interrupt, but this is an intermission and not a finale, hopefully, for this group. The government shut down at 2:00 o'clock. So I'm told we have to let everybody go today, I'm sorry to say. However, I guess we're going to spend maybe a few minutes with Lawrence and maybe a couple of other folks to help figure out what we're going to do.

I think if the government is closed tomorrow, we will move tomorrow's meeting. Unless I get something hitting me in the back of the head, we'll move tomorrow's meeting to Thursday and deal with the agenda at another time. I'm sorry to say this.

MEMBER HINZE: I think we can compress some things as well.

CHAIRMAN RYAN: And we'll work with the presenters and staff to do that, but the game plan right now is that if the government is open tomorrow, business as usual. And we'll make any switches and accommodations for people's travel plans and needs as we have to.

If the government is closed tomorrow, we will reconvene Thursday morning. And we will adjust the schedule Thursday.

MEMBER HINZE: May I ask a question for the non-government types? How do we find out whether it's open tomorrow or not?

CHAIRMAN RYAN: Great question. Frank?

MR. GILLESPIE: Listen to the radio. You could probably just call in the NRC's central number, which is (301) 415 --

CHAIRMAN RYAN: Can somebody post something on the Web? Will that happen or does that happen on the NRC Web?

MR. GILLESPIE: There's a banner on opm.gov at the top of the page where you can --

CHAIRMAN RYAN: And on opm.gov. Okay. Opm.gov. And the banner will be there open or closed. Thank you very much for that information. For our guests, particularly our out-of-town guests, I apologize for the inconvenience.

Actually, the roads and sidewalks are now pretty slippery. So I think that's probably what's influencing folks' decisions. It's getting cold.

I really apologize to everybody who has come far and wide to do this, but we're at the mercy of the weather. And I really appreciate your patience and understanding. And we'll rerack either tomorrow morning at the appointed hour of 8:30 or Thursday morning at the appointed hour of 8:30. Okay?

Thank you all very much. I appreciate your patience.

(Whereupon, the foregoing matter was concluded at 2:06 p.m.)