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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE

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WEDNESDAY

SEPTEMBER 22ND, 2004

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

Advisory Committee Members Present:

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

Others Present:

KEITH ECKERMAN Oak Ridge National Laboratory
FRED HARPER Sandia National Laboratories
DAVID JOHNSON ABS Consulting
BRUCE CROWE Los Alamos National Laboratory
DR. BILL MELSON Smithsonian National Institute
MICHAEL LEE ACNW
JOHN LARKINS ACNW
JAMES CLARKE ACNW
WILLIAM HINZE ACNW

Others Present:

BRUCE MARSH	ACNW
BOB BRUDNITZ	LLNL -- on detail to DOE
LYNN ANSPAUGH	University of Utah
B. JOHN GARRICK	NWTRB
GEORGE HORNBERGER	NWTRB

A-G-E-N-D-A

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

8:05 a.m.

OPENING REMARKS

CHAIRMAN RYAN: Good morning. The meeting will now come to order. This is the first day of the 153rd meeting of the Advisory Committee on Nuclear Waste.

I am Michael Ryan, Chairman of the ACNW. The other members of the Committee present are Ruth Weiner and Allen Croff. Also present are ACNW consultants William Hinze and Bruce Marsh.

James Clark, another ACNW consultant will be joining us later in the meeting. He was unavoidably called away. During the next two days the Committee will conduct a working group meeting to review and discuss issues related to the evaluation of igneous activity and its consequences at a potential geologic repository Yucca Mountain, Nevada.

The Committee will gather information, analyze relevant issues and facts, and formulate proposed positions and actions as appropriate in the form of advice to the Commission.

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

Mr. Mike Lee is the designated Federal Official for these sessions. A transcript of this meeting is being kept. And the transcript will be made available as stated in the Federal register notice.

It is requested that speakers first identify themselves and speak with sufficient clarity and volume so that they can be readily heard.

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

As an administrative matter, if you haven't already done so, it is requested that you sign in at the table in the back. We also request that, if you have them, please confirm that your cell phones are turned off or alternatively have been rendered into silent ringing mode.

Lastly, for those of you who wish to do so, there are comment feedback sheets available at the sign-in desk. Items of interest, before starting the first session, I would like to cover some brief items of current interest.

On August 16^{th}, 2004 President Bush announced his intention to appoint ACNW members Dr. John Garrick and Dr. George Hornberger to the Nuclear Waste Technical Review Board.

Dr. Garrick was designated as the Board's new Chairman. We regret their resignations from the Committee and wish them well in this new endeavor. Congratulations to you both in every success.

The Committee and I, as the previous Committee Vice-Chair, have assumed the Chairmanship of the ACNW. Volumes one and two of the Nureg 1710 series on the history of water development in the Amargosa desert were recently approved for publication by the ACNW's Executive Director.

These Nuregs were co-authored by Mike Lee and Neil Coleman of the ACNW technical staff and Tom Nicholson of the NRC's Office of Nuclear Regulatory Research.

In addition to service to this Committee, the ACNW has encouraged the support of the Staff's efforts to publish technical reports and papers -- the Agency's overall mission.

Lastly, Mr. Marvin Sikes, a Senior Staff

Engineer with the Advisory Committee on Reactor Safety, the ACNW sister Committee, has been selected to fill a branch D position in NRC's region one division of reactor safety.

He will depart for his new position in mid-November, and the Committee wishes him well. The ACNW has been tracking developments related to the modeling of a disruptive igneous event at Yucca Mountain for several years.

Earlier Committee views on the pertinent issues can be found in five letter reports. Copies of these letter reports can be found in the Committee's internet web, as well as in Nureg 1423, the compilation series for ACNW letters.

Most recently, in June 2002, the ACNW conducted a workshop group meeting to learn more about the issues which resulted in the letter report for the Commission dated August 1st, 2002.

WORKING GROUP PURPOSES

The overall focus of the working group meeting is to better understand what knowledge base is available for decision making, areas of specific ACNW interest, including understanding the realism of existing approaches and calculations and identifying areas in those approaches and calculations that may require additional work.

Consistent with the published agenda, three technical sessions, consisting of about 15 presentations are planned over two days to focus on the treatment of probability, consequence, and dose in igneous activity performance assessment analysis.

To help the Committee explore the issues and interrogate the invited speakers, and maybe just have a conversation with the invited speakers, rather than interrogate, a panel of invited experts has been assembled.

They include Dr. Robert Budnitz from the Lawrence Livermore National Laboratory, Dr. Dave Johnson from ABS consulting of Irvine, California, Dr. William Hinze, Professor of geology and geophysics at Perdue University, and Dr. Bruce Marsh, professor of igneous petrology at Johns Hopkins University, and finally Dr. William Melson, Senior Scientist of volcanology at the Smithsonian Institute in Washington, D.C.

Welcome all, thank you very much for your time and participation in this working group meeting. At the conclusion of tomorrow's meeting, Dr. Johnson will provide summary remarks concerning the issues discussed in the context of the application of the risk triplet, the risk triplet being three questions.

What can go wrong? How likely is it? And what are the consequences? So, we will be thinking along those lines. The first session planned today is on probability.

Areas of specific ACNW interest here include understanding the types and kinds of geologic information needed for generating probability estimates, the uncertainty in that information, and identifying which analytical approaches yield defendable estimates.

And, to address these issues, three presentations have been scheduled for the first session. The first presentation will be by Dr. John Trapp of the NRC staff, and will feature a discussion of the geologic features of the Yucca Mountain region considered to be important in the estimation of igneous event probabilities.

Dr. Bruce Crowe, of Los Alamos National Laboratory, former principal investigator of igneous activity in DOE's Yucca Mountain programs, and a subject matter expert in the 1996 probabilistic volcanic hazards analysis, will share his perspectives on the type of geologic information that is important to decision making at the time the expert elicitation is conducted.

Other perspectives on the interpretation of the local geologic record, and how it affects probability estimates will be made in a presentation by Mr. Neil Coleman of the ACNW staff.

He will present a paper that he co-authored with Dr. Lee Abrams of NRC's Office of Research and Bruce Marsh that was recently submitted to geophysical research letters.

This paper relies on statistical methods to evaluate the probability of the issue. I'll talk about the second session when we begin that session. So, without further ado, let me turn to our first speaker, Dr. John Trapp.

NRC PERSPECTIVE ON VOLCANISM MODELING ISSUES

MR. TRAPP: Okay, Good morning. Like I was saying, a few comments. The actual discussion on probability comments will be given by Dr. Britt Hill.

I'm going to be presenting just a brief overview of-- our program, talking about really the main assumptions. That was the second one we were talking about.

And then, in addition, talking about what we feel like the risk significant items that we need to understand. That's basically coming out of -- I partly should first off say some things that we will not be talking about.

We will not be discussing any of the work that is presently in progress. Everything that we are talking about today from the NRC perspective will material that is readily available to the public.

In addition, we will not be making comments about DOE's licensing case. A, we really don't know it, and B, it's inappropriate at this time to discuss this type of things by the NRC staff.

Next slide please. So, what am I going to be doing? I'm basically going to, like I said, be providing a basic assumption, the NRC's and the RPA -- evaluating these.

Based on results that we have -- are not specific. Next slide please. For those of you who have not been to the area of Yucca Mountain, this was just kind of a slide overview.

The center of the slide is Yucca Mountain. If you take a look off to the west, you will see Bare Mountain. And, in between Yucca Mountain and Bare Mountain, there are a series of electrons down there.

As you come to the southeast, in the Crater Flat area, what you don't see is a series of other basalts, which are basically 3.7, approximately, a million years old.

Farther down, at the very tip of the mountain, you will see the youngest igneous feature, which is present in the area, Lathrop Wells. The Amargosa Desert area is an area which has quite a few varied igneous features and quite a few anomalies, which may or may not be igneous features.

This is an area where DOE has run a recent aeromagnetic electromagnetic survey, the results of which are just starting to become available. Yesterday the preliminary results from DOE -- this hopefully will shed a lot of light on information about the distribution of igneous bodies in the area, and help us work to determine the probability.

Jackass Flats, which is on the west side, or the east side of Yucca Mountain, has feature covered mountains -- the Fortymile Wash basin, which is going to be quite important in the whole discussion.

Let's take a look at these. That's of the wells that was drilled by -- there was a basalt of, I believe, nine and a half, a million years, approximately from that well.

More important, for the sake of some of the discussions that will be going on negative today, if you notice, the drainage coming out of Fortymile Wash, you will see just along the highway, running east and west through that area 23 feet that the characteristic of the drainage system changes tremendously.

You're going from a marginal system into a depositional system. This also happens to be approximately in the area where the reasonably maximally exposed individual, the person that we have to use to characterize doses to the public too high.

Next slide please. So, what are some of the basic assumptions? Well, if you took a look at that slide, you will see that a small volume of basaltic cones have occurred in the general area of Yucca Mountain in the past.

And there is some potential that there will be future basaltic igneous events that could possible occur. We modeled it, the DOE has modeled it, the State has modeled.

So far, all the models -- and there is quite a bit of arguments back and forth on what the probability is -- but, all the probability models come out a value that's larger than regulatory requirements considered in our performance assessment.

There is large uncertainty with this, like

I said DOE has finished the aeromagnetic survey. They have been doing some drilling with some of the anomalies to determine which ones really are under the basalt.

They are going to be digging those basalts. All of this will hopefully produce the uncertainty on this probability. Next slide. If you take a look at the volcanoes that you've got in the aerial, you'll see that these all produce not only lava flows, but their results, the deposits, show periods of sustained eruption columns with buoyant tephra plumes.

If you take a look at the historically active analog, what you will see is these type of volcanoes have the capability of hitting buoyant plumes and transporting them 10 to 100 kilometers downwind.

If you take a look at some of the recent results that have been published in the literature, what you will find is, contrary to some of the earlier modeling and some of the assumptions, these basalts are actually quite wet.

They have got -- the best estimate would be something like about four percent water. One of many of the original modeling studies on these were done with much lower water percents.

And what we are talking about is, with this high water, you definitely have potential fragmenting and getting these connected dispersive plumes.

Next slide please. One of the questions that has been asked quite a few times in the past is, why didn't we put any other -- a risk likely to in this?

Well, one of the reasons is, there really isn't a good way to measure how big the volcano is. Here is one example. If you take a look along the top, you will see that, really what it is talking about is two factors.

How much tephra is produced in the ash? And how high do these columns get? It doesn't talk about the total volume of magma produced. It concerns some of the other type eruption sequences.

If you go on down, you will look at the volcanic explosive that makes number two. And, basically, this is -- of all the studies we have done, approximately a majority of the events -- they may sneak down to a one.

They may sneak up to a three. But, really, we're talking about a single class for all practical purposes, is ten to the minus seven cubic meters tephra and columns on the order of two to five, maybe seven, possibly even size ten, but I doubt that high.

Another important point is, if you go on down a line, you will see that these do not get into the stratospheric level. So, this is very important in talking about some of the potential health effects and other considerations for that.

Next slide. Another assumption that we have is that the waste package is intercepted by magma and be subject to very high thermal stress, and very large mechanic stress.

This is a likely caused failure of the canister. And, therefore, many radioactive waste is exposed to magma. We have given this problem to our waste package engineers and talked about the conditions that we got in this type of situation.

And, with the days to weeks that this package would be subject to these types of thermal stresses, mechanical stresses, the conclusion that we come to is that this package -- well, basically, can be breached.

Our assumptions in the -- what we have done, is assumed the waste package offers no protection whatsoever. This assumption has been used in previous DOE analysis.

It may change, etcetera, but this is the present assumption that we are using. Next slide. Okay. We've got the package breached. So what happens?

Well, we've got the waste sitting there. And this is now assumed to be available to put in the tephra column. We don't really model, like I said, lava flows for a very simple fact.

If you take a look at all the data that you've got, a lava flow by itself really doesn't pick up too much. We do not assume that this waste melts in the basalt, because, really, we do not have the type of material that would dissolve in magma.

What we're following is what you see in a normal eruption, the fragmentation of the wall rock, the fragmentation of the material. This gets broken down in small sizes and traded with the material, and put up, and then transformed back.

Next slide. Okay, you've got stuff up in the air. You've got a transporter downwind. It falls to the ground. Well, when it hits the ground, we're basically assuming that, yes, you can suspend the stuff into the air, from which people can breathe it and get a dose.

This turns out to be the igneous scenario, the main method by which the dose gets in the -- we're using a bunch of very simplified assumptions going all the way through this.

It really boils down to two primary factors. What mass loading factor do we use, and how long does the deposit last? Next slide. The next assumption we talked about, we took a look at where the site was.

Okay, and take a look at where the remedy is, and try to do what I talked about, modeling assumptions. The majority of the time, based on our knowledge of the winds and the altitude, the tephra column will not go directly to the RMEI.

It would sometimes. But, most of the times, it would be blown somewhere east and deposited at Jackass Flats. So, I'm going to get to the RMEI.

I'm going to get to the RMEI by two means. It can be brought down by strain erosion. And, if you took a look at the Fortymile Wash, what you will see at the Fortymile Wash, like I said, as you go right at the RMEI location, right before the erosional sequence, that position of sequence.

It can also be brought by wind erosion,

etcetera. This, we believe, is a very important factor which needs to be taken care of. And we're working on that with the Staff.

Next slide. So, what's important? Well, according to our code, what is the probability of an event. This is apparently straight-forward. The rest is directly proportional to the probability.

Dr. Britt Hill will be presenting information on that. Another significant thing, well, the waste package is intersected by volcanic events.

And we're talking about the risk being proportional to the amount of waste that can be exposed. So far, packages in a larger area, the large area was.

The volume of ash produced during an eruption was important. And this is actually the inversely proportional, because, what you end up with here is a delusional package.

Larger volume eruptions tend to dilute the amount the material that is there. Smaller volume eruptions encounter larger concentrations. With these two factors, especially number two that we will discuss to certain extend this afternoon when we get to that session.

Next slide. As I mentioned,

remobilization of the process is important, because this will keep the majority of the ash to the location. Dr. Don Hooper will be discussing that, I believe, in tomorrow's session.

He will talk about the modeling in this area. And, like I mentioned, inhalation is the major factor by which you get the dose to the humans.

So, we will have a discussion talking about this fact, this subject matter, and how it is handled. These are the important things that we see in the load.

They can all be discussed in more detail later.

CHAIRMAN RYAN: Thank you, Dr. Trapp. And thank you for competing with the music next door. Maybe we can get somebody to see about turning that down just a tweak.

Thank you. Are there any openings? I think John set the stage for the following presentations and their own opening for John. Or shall we reserve out thoughts for the more detailed presentation? Yes, Bill Hinze?

MR. HINZE: Well, let me ask you, John, you did an excellent job going through all of the assumptions at various stages.

CHAIRMAN RYAN: You have to flip the microphone on.

MR. HINZE: I would like to ask you about this. We all understand that there are uncertainties with modeling because you use various assumptions.

Some of these uncertainties remain. Others, we would like to -- and Britt will expand upon this. Which of these has the greatest chance in the next few years of decreasing the uncertainty with better models, with better data?

MR. TRAPP: I think we can reduce the uncertainty quite a bit by taking a look at the remobilization. I think that is an extremely important factor.

Again, you are correct, you have large uncertainties. And we're not going to get rid of them by -- coming out in the DOE program to reduce the uncertainties in the probability model.

Again, we will not eliminate them. But we will reduce them. There is work that is going on in the understanding of magma flow, some of which you've got some preliminary. And there is quite a bit more, which we cannot discuss at this time.

And, yes, I think there will be some reduction in uncertainty in that area, but not as much as we could probably expect over the areas of the remobilization period. Britt, would you want to comment on that?

MR. HILL: That was fine.

CHAIRMAN RYAN: Any other opening questions or comments?

MR. HINZE: If Britt doesn't have a comment, I would like to ask you about this dilution that you mentioned. And perhaps Don will expand upon this in his presentation.

I understand he's making a presentation on this re-distribution of distribution. Yes, you mentioned that you are really interested in having more tephra because that leads to dilution.

But, according to your slide six, as we have larger amounts of tephra, our column height also increases.

MR. TRAPP: Right.

MR. HINZE: And that means that -- to me -- you have greater dispersion. And so, does this necessarily mean that, as you go from violent to whatever, that you really are leading to dilution?

MR. TRAPP: If you could have those type of eruption, yes you would be getting a tremendous amount of more dilution. But, seeing no evidence that we would have eruptions or volcanic activity, it would be anything more than approximately -- two.

So, we're really talking about a very limited subset of that. You would not have something like a PDI 4 or like a Mount Saint Helens.

MR. HINZE: It just seems to me that, if you have more, you don't dilute because you're throwing it up higher and spreading it out more.

MR. TRAPP: That's true.

MR. HINZE: Okay.

CHAIRMAN RYAN: John, just a quick follow-up as just kind of a question for maybe some of the other presenters as well. We kind of end up at the end of the day with a question of what is in the air that's inhaled by the RMEI or some theorized person?

You've touched on a lot of very complex processes that get us to what is an irrespirable size range in the fraction for that exposure scenario.

That's very complicated. And Bill has touched on one aspect of that. So, to the extent you and the other speakers can talk a little bit about, you know, what part of the mobilization process in an event leads us to that endpoint of irrespirable particles. That would be real helpful.

MR. TRAPP: Part of what Britt will be

talking about will cover that. Don will definitely cover that. Keith Compton will go into effects.

CHAIRMAN RYAN: Thanks. To me, that's kind of the focal point. Because, at the end of the day, having uncertainty on that is really where you can kind of begin, you know, be satisfied or unsatisfied with the uncertainty question.

MR. TRAPP: Like I said, Don will be discussing that.

CHAIRMAN RYAN: Okay, great. Thanks.

MR. HILL: This is Brittain Hill at the CNWRA. I just wanted to clarify a little bit for Dr. Hinze in response to his comment. In our performance assessment and calculates, we allow the total volume of tephra to be ten to the sixth to ten to the eight cubic meters.

But, the column height is though of not only of the volume, but the rate that it would come out. So, we also vary the duration of the event between essentially one day to like a week.

It's about five days, is our approximate sort of mass blow. So, the column height, while it is partially a function of volume, is also a function of duration.

So, when we run a large number of

realizations in our performance assessment, we can have small volume events that happen over a very short period of time give us a high volume.

We can also have larger volume events that would happen over a long period of time, that would give us the lower volume. It's not quite as straightforward as simply larger volume, more distal dispersion.

And, also, the source is about to vary between one and ten waste packages per event. So, we are getting that full sample in the variability. And no one particular size is truly driving the risk analysis.

MR. HINZE: I think we'd all like to hear about that in more detail as the presentations are made. I guess one of my concerns is that this is a useful chart, but it is very simplistic. And that's, I think, what you are saying.

MR. HILL: Yes.

MR. HINZE: Yes, don't hang your hat on that.

MR. HILL: No, this figure was just meant to be an example of the full range of volumes that volcanoes can produce. And, relative to that full range, here is the area of interest for a particular hazard related to potential --

MR. HINZE: There are a lot of problems with Richter magnitude, but at least it's --

MR. HILL: Right.

CHAIRMAN RYAN: Any other questions? You've accomplished the goal of the first speaker, which is to get everybody's attention and stimulate their interest. So, off we go.

NRC OVERVIEW OF IGNEOUS ACTIVITY AT THE YUCCA MOUNTAIN REGION

MR. HILL: Good morning. It's nice to see we have such a taste in laptop computers. That's the correct one. I'm Brittain Hill. I'm the principal investigator for igneous activity at the Center for Nuclear Waste Regulatory Analysis.

And, this first talk this morning, I would like to talk to you about some of the Staff's positions and tools that we have developed for assessing the effects of uncertainty on probability estimates for potential volcanic eruptions at the potential repository site at Yucca Mountain.

Next slide, please. After a brief introduction, it includes a little bit of regulatory basis. I would like to talk about some of the uncertainties that we have in very basic probability estimates and also make sure that we all have a common framework or common definition for the remainder of the talk.

I will then focus on some of our current views on the spatial and temporal uncertainties that affect probability models in the Yucca Mountain region and see how those uncertainties can affect the NRC probability estimate, and of course, wrap it up with the conclusions.

Next slide, please. I guess that's my soundtrack. That's fine. What we are going to call upon to evaluate the probability models and licensing, you have to keep in mind that these probability models -- performance assessment.

And so, requirements for review under 10 CFR 63.114 are going to apply. In particular, the models for probability need to include actual geological and engineering data, account for data variabilities and uncertainties, consider the effects of alternative conceptual models, evaluate events with likelihoods greater than one in ten thousand in ten thousand years, include events that significantly affect risk calculations, and also be supported by objective comparisons.

So, we have to keep that in mind when we start looking at the currently available information around Yucca Mountain and how that information affects the probability model.

And right now, some of the questions that we're asking are how many past events have there been in the Yucca Mountain region? What are these igneous event locations?

And what are the event agents. So, I don't want to call this a probability triplet, but there is some parallelism on number, age, and location of past igneous events.

And, to cut to the chase, our conclusion is that, from the available information, you can have multiple interpretations and large uncertainties from what we currently have available for assessing probability in the Yucca Mountain region.

Next slide, please. One of the basic uncertainties that we have to address, and to begin a definition for any presentation, is what makes up an igneous event?

And, taking a figure from the Department of -- the NRC's technical basis document 13 on igneous activity, to illustrate what the uncertainty is in finding an igneous event.

This figure is a geologic map showing the

4 million year old basalt outside Southeast Crater Flat. There's a series of numbers out there. Number one, two, and three mark locations that I think, as a general agreement, represent the volcanic center.

This is a place where we have a hole in the ground where molten rock came up and material was dispersed in the accessible environment. But we also have points four, five and six, that may represent vent locations.

There's just a little less certainty about whether these were large vents, small vents, or vents that could start the beginning phase of an eruption only.

So, how many vents were erupting at the same time? How many vents may have erupted in sequence, may have represented gaps in time to be counted as separate volcanic episodes?

There's multiple interpretations that you can place just on these six features. For the purposes of this talk, I'm going to keep the simplest definition possible.

An igneous event is a volcano that has a hole in the ground. And we're just going to count up holes in the ground or cinder cones and call those our igneous event with this presentation.

We also know, in igneous events, have to worry about the subsurface conditions. What's going on beneath the volcanoes as well? And one of the things we see out here in Crater Flat, is the subsurface features, which are called intrusions, extend for 50 years plus laterally away from our vents, and for some unknown distance longitudinally to the north and south in these vents as well.

So, in characterizing igneous events, we not only have to find out the surface expressions, but the sub-surface expression as well. And one other point, when you talk about igneous event, is relevant to this.

Do you notice how these lava flows have been folded and partially eroded through time? Now, if you continue the deposition process out here and bury these lavas between tens or even 100 meters worth of alluvium, how would you interpret igneous events from this disruptive feature if all you had to go on was a pattern of colors in the geomagnetic map?

Keep that in mind when we start looking at pattern analysis in the later part of this talk. We may not be seeing in the subsurface impact features. We have to consider the possibility that these features, like this one at Crater Flat, have been disrupted, faulted, eroded, and then buried.

Next slide, please. One other very fundamental uncertainty or assumption in probability models is, what's the extend of the igneous system that we're trying to model.

This figure is showing in red basalts that's younger than about 11 million years old. And all of these parts of basalt at one time or another have been used to bring various definitions of what makes up the Yucca Mountain igneous system.

These definitions have been based on associations in age, location, and chemistry. And, you can't quite see it, but, the potential repository site is right here on the boundary of the NTS.

Now, there's not correct definition of what makes up Yucca Mountain igneous system. The point that we have to make, though, is that a basis for selecting some subset of these basalt features needs to have a clear, consistent basis.

And that basis has to be used consistently throughout the probability estimate and any resulting consequence analysis based on that probability estimate.

Next slide. So, that being said, I'm going to say what we think the relevant Yucca Mountain igneous system is for the purposes of these.

What I'm showing in this figure is the regional gravity survey that's done by the U.S. Geologic Survey a few years ago. What it shows is, in the hot pink colors and orange colors, are areas of fairly dense crustal rock.

The cooler colors down in the greens, yellows, and blues, represent low density crustal rock. The reason we are using gravity, is this is a real good regional indicator of structure.

What we see is this long feature through here with the low density rock represents an extensional basin where the crust has been pulled apart and in field with low density alluvium and tuffaceous rock.

The other rocks in high density here and here haven't been as disruptive in recent time, and consist of older, more crisp rock, like around Bare Mountain.

For convenience, we're just going to refer to this feature as the Amargosa Trough structural basin. Now, a little bit on the west, by Bare Mountain by this gravity anomaly, and by the east, by what's commonly referred to as the gravity fault, and extending some unknown distance up towards the old caldera complexes into the mountain.

And, again, within the Amargosa Trough is what we think the basaltic features are that are relevant to our probability estimate. And, we're defining igneous events in the following analyses as individual volcanoes that occur within this Amargosa Trough.

Based on that definition, we have a starting point of 24 past events in the Yucca Mountain region t use in the following sensitivity analysis.

Okay, do not adjust the dials. This is actually what the data is supposed to look like, these wild colors. This is the 2000 or 1999 U.S. Geological Survey -- aeromagnetic survey -- for the entire Death Valley/Yucca Mountain region.

This is the old survey. It's not the new data that the Department of Energy collected this summer. These data represent the magnetic characteristics of the region and of the rocks that are buried and exposed at the surface in this region.

We've gone ahead and done a little filtering on these data to enhance the basalt features in the region. The important point here is, we have known features, known igneous events, and surface -- such as Red Cone, Black Cone, Lathrop Wells, that create obvious anomalies, when you know where to look.

The anomalies are just these patterns in the magnetic data that have characteristics representative of buried basalt or strongly magnetized rock.

But, the U.S. Geological Survey and ourselves have also identified other areas that are representing sub-surface, buried rock that may represent a very igneous event.

And these interpretations are shown on the figure on the right, graded by competence level. The red features are ones that we have high confidence in representing buried basalt.

The green features, for example, this -- from L, M, N, O and two -- we have moderate confidence that these anomalies represent buried basalt.

And, in blue, we have low confidence but can't eliminate the possibility that these anomalies could represent buried basalt. So, one of the primary uncertainties that we're having to evaluate right now is, given these anomalies, what if they represent buried basalt?

How would the addition of these buried potential features affect our probability estimate? Next slide, please. We have the aeromagnetic survey that shows us features that we can detect,

But, along with that data, we can also see that there are some features that we know exist. But we haven't found them yet. They don't create obvious magnetic anomalies.

So, we have to consider the potential to have additional features located in this region buried in the subsurface that the exploration techniques have been unable to detect.

One of the ways, and not the only way, you can do it -- but, one of the ways that you can try to get an estimate for potentially buried features is, look at the spatial density of the volcanic fields and compare it to other volcanic fields and say, well, there's a long list of low, and a long list of high.

How could you add additional events and change such spatial vents? What we see is, within this Amargosa Trough volcanic system, just with our 24 known events, we have a spatial density of one volcano every 29 square kilometers.

For comparison, when we look at other volcanic fields in the western great basin, like the Cima volcanic field in California, they have the density of one volcano every four square kilometers.

Lunar Crater up in Nevada has one volcano every six square kilometers. The older Pancake range volcanoes are about one volcano every eight square kilometers.

And finally, the Big Pine field in the valley of California has a lower density of about one volcano every 16 square kilometers. From the water well drilling out there, we're pretty sure there is some additional hidden events out in the Big Pine field as well.

So, we can see that the spatial density of volcanic features in the Amargosa Trough are very pretty low compared to other similar volcanic fields in the western Great Basin.

The exploration technique, the aeromagnetic technique that has been used, we have fairly high confidence that the survey has been able to technique buried igneous features in the southern half of the Amargosa Trough.

The reason for that is the basement in this area is magnetically very quiet. So, strongly magnetized rock like basalt, will really stand out on aeromagnetic surveys.

So, we're not concerned about undetected significant features at this stage in the Amargosa Trough in the southern part. But we have these two areas throughout Jackass Flat and Crater Flat where the magnetic basement is very noisy.

And that noise may be masking additional features in the subsurface. Right now -- we have a volcanic density of one volcano every 13 square kilometers.

Just for comparison, if you wanted to get that spatial density up to something comparable to Cima -- the most dense volcanic field in this analysis -- you're going to have 26 buried events in order to get that high of a stageable density.

That's just a major comparison -- not that we think you have to have any volcanoes out there. Also, at Jackass Flat, you've got one volcano every 160 square kilometers.

Now, it is entirely possible that that is the actual spatial density within Jackass Flat and that there are no buried, undetected features in Jackass Flat or Crater Flat.

But, right now we can't eliminate that hypothesis. And we have to factor in our uncertainty analysis the potential for undetected events, as well as the events that have been detected by current exploration techniques.

MR. HINZE: Mike, is it possible to ask a question?

MR. LEE: Sure, I guess so.

MR. HILL: Sure Bill.

MR. HINZE: The one volcano per 29 square kilometers seems to be key to this discussion. And, it seems to me that your region of the Amargosa Trough does not correspond with the complete region of the Amargosa Trough that you outline in the previous gravity slide.

Am I wrong, or right? Or what's wrong here?

MR. HILL: It's the extent of volcanic in the Amargosa Trough. Now, the Amargosa Trough, as a crustal structure, extends down all the way into Death Valley, and all the way up into the lunar crater area.

MR. HINZE: And it extends considerably south. So, if the Amargosa Trough is controlling this, shouldn't we be concerned with the number of volcanoes per square kilometer or the volcanoes per kilometer, considering the Amargosa Trough problem?

MR. HILL: No, I don't believe so, because the Trough is a structural control on ascending magma. Not everywhere in the mantle, though, we believe this for the production of basalt.

We have many areas that are extended and lack any appreciable volcanism, not only in the Amargosa Trough, but in other parts of the basin and range as well.

So, you have to have an intersection of the whole extended crust and further mantel in order to get volcanism.

MR. HINZE: Okay, so you are arbitrarily selecting the north and south --

MR. HILL: Not arbitrarily. I'm selecting the north and south boundary that, within the last billion years, defines the extent of volcanism within the Amargosa Trough.

Until you get down to Death Valley, many tens of kilometers to the south, you're not seeing more volcanism. In the same way, this is butting up against the Caldera Mountain -- a little south of Caldera Mountain.

But, it's the northern extent of Solitario dike complex. We're coming up very close to the Caldera mountain. And I think that's defining a tectonal magnetic regime that we're calling the Amargosa Trough.

CHAIRMAN RYAN: We have one follow-up question from Bruce Crowe.

MR. HILL: Yes, Bruce?

MR. CROWE: I'm Bruce Crowe of Los Alamos Lab. One question I have -- I messed around with doing cone densities as well. And what I tried to do, though, was divide them in age increments, because you really need to look at how densities have changed through time.

And, if you look at the forming, you know, record, versus say the -- you're going to get somewhat different cone densities, both in Crater Flat Amargosa Trough, and in Lunar and Cima. Have you tried doing that?

MR. HILL: To an extent. One of the problems is, while we have good dating in the Yucca Mountain region, these other analogs we have very loose dating.

So, I tried to give a representation of the -- and Pliocene fields. But I don't think any of these fields have Pliocene database that we can go into.

As you're well aware, we have some disagreements about the relevant of the Miocene. And I think that's a fair interpretation. And we believe the Miocene from 11 million years, and then -- to the third.

In other words, the past 11 million years is relevant to the probability estimate. Whereas, others are saying that only the past five million years of volcanic history is relevant in their probability estimate.

So, for this talk, I'm trying to be consistent with our published positions using the Miocene record, and in standard volcanism -- the Amargosa Trough.

Again, these are not the only potential analogs. They are the most analogous of the Western Great Basin. And they are the limits of the available data for age clustering.

Given the uncertainty in the potentially varied events where we don't know the ages of them, we're trying to do more refined approach at this stage, really just pushing it forward.

But, to get to, is that -- to emphasize the main points here for the spatial uncertainty. We may have no undetected events. But we can eliminate the potential for undetected events.

We have to come up with some way to quantify in a traceable methodology a way to say how many could there be in this area? And, by looking at a general sense of spatial density, we say that, given an uncertainty of one to ten present undetected events, seeing a reasonable starting point in a sensitivity analysis for evaluating whether this kind of an uncertainty in undetected events is significant or insignificant to the probability estimate.

And, just as a point of comparison, if we were to have ten additional events in the Amargosa Trough, we would increase the spatial density within this region from 29 -- or one volcano for 29 square kilometers, down to one volcano for 23 square kilometers.

So, it's not an absurd over-estimate of spatial densities for the Yucca Mountain region.

MR. CROWE: Just a follow-up, if I may. If I understand your record, you start with the premise what is there at one to ten -- present but undetected events.

I ask the question, what's the probability of it being one to ten undetected events?

MR. HILL: Based on the currently available data, we think that -- let me back up for a minute. When we had a meeting about a year ago with the U.S. Geological Survey, Department of Energy and others to evaluation the aeromagnetic data, we all agreed that there were a number of known surface features that were difficult to resolve in the aeromagnetic data.

So, they were giving a sort of anomaly patterns. They didn't know that basalt was at the surface. You'd have a difficult time convincing yourself that that anomaly represented surface basalt.

So that's the first point of why I think there may be contentions there. The second is, out at this location, at Jackass Flat, according to early warning wells, we encountered basalt at about 1,300 feet below the surface.

That basalt is in an area that has no obvious magnetic anomaly. And that depth that encountered basalt is likely deep enough to attenuate any magnetic character of a buried well.

So, we have known features that don't give us a clear anomaly in both the surface expression and in the sub-surface expression. So I believe it is reasonable to assume that there could be additional undetected features here, based on the limits of the current exploration technique to detect known igneous features in the region.

I cannot give you a probability estimate though. I think that's so speculative on top of a speculation, on top of a hypothesis, that we really can't gain much knowledge that way.

CHAIRMAN RYAN: And therein is my problem in that, you know, we are in a way scuba-diving around in open. We really don't know where the bubbles are and where uncertainties are at this point.

Perhaps the new aeromagnetic data helps us, you know, resolve some of that. But that, to me, is kind of a critical issue, because, without knowing where you are in the probabilities space of all those potentials, you can run into not really knowing how to interpret what the hypotheses are.

MR. HILL: We don't need a probability to evaluation the significance of alternative conceptual models.

CHAIRMAN RYAN: But you do need the probability to know which one is real.

MR. HILL: Conversely, you can start with a reasonable range of uncertainty, let's say one to ten undetected events. Let's analyze that in the models and see whether it is significant.

And it may a lot easier to gain a reviewable consensus that says, we think if there are undetected volcanoes, there's less than ten of them, or less than five of them in the region.

When we can all agree to that to develop a basis for it, rather than trying to come up with probability distribution function that is going to be -- by it.

CHAIRMAN RYAN: I appreciate the fact that you are trying to -- that's different than being risk informed. So, I just want to clarify these two different lines of thought.

MR. MARSH: I think this is one way to kind of justify adding events in. But, it might be actually a more illustrative calculation. You just started with the probability basis itself and just kept adding until we became alarmed.

In other words, we may have to add 5,000 to actually make it. So, we're basically wasting each other's time down at this level. And that also answers Mike's question a little bit, in that it puts uncertainty on this in terms of saying how much seriousness do we have to put into actually adding and comparing to these other fields up there that are basically very homogenous in age fields that we can interpret very simply and whether this field here, as Dr. Hill mentioned, is.

We're looking at stat data over time, and so forth. It might even -- to the chase. Just look at the numbers, add them in directly, justify them later, worry about it after.

MR. HILL: The talk is -- we're around the first corner, and we're cutting to the chase. I think I'll address your comment in a few slides here.

Let's go on. Okay, and this is just a very quick summary of our view of current spatial uncertainties in probability. The addition of the 11 anomalies that are well recognized by ourselves and the U.S. Geological Survey would increase --

CHAIRMAN RYAN: Shut the microphone off, I think we can hear you.

MR. HILL: Well, will that affect the recording.

COURT REPORTER: I have a back-up here.

MR. HILL: Okay. Can everybody hear me now, without the feedback? Excellent. Okay, we're looking at, with the addition of the magnetic anomalies that we have high to moderate confidence in that increases the spatial recurrence rates for about one volcano for 40 square kilometers, to one volcano for 29 square kilometers.

Again, a comparison with the volcanic fields, the point that we had made before about the limited resolution of known features, the accounting of basalt in 23E is the basis for suggesting that there could be additional undetected events.

And, finally, for spatial uncertainty, if we add one to ten additional events, the spatial recurrence rate would increase form 29 -- excuse me, one volcano for 29 and one volcano for 23 square kilometers, a very modest increase in spatial density.

So, let's move on from spatial, and go on to the temporal uncertainty. Now, again, there's no correct definition of an igneous event. And I know there are people in the audience that have alternative definitions of what constitutes an igneous event.

But, at the ousted I said, where is an igneous event definition that's each individual vent is an igneous event, a cinder cone event, very simple.

What we've done is plotted out the number of cinder cones, and cinder cone remnants that we have in the region against their ages. The points that are in gray are the ones that -- just to be honest -- are altitude interpretation that sometimes lump them all together as a single event.

But, again, to be consistent, these are the 24 individual events that we are using for the purposes of this talk. And these are the basic data that we have for when have past igneous events occurred in the Yucca Mountain region for the past 11 to 11.4 million years.

Next slide, please. So, our base case, if you take a longer 11 million year average, we have the 24 events for 11 million years, to give you a temporal recurrence rate of two volcanoes every million years.

But now, we somehow have to address what's the age of these magnetic anomalies? We have higher confidence in the buried basalt. We don't have any dates on these anomalies.

So, we have to look at alternative hypotheses on what these dates could be, based on our interpretations of past patterns of activity in the Yucca Mountain region.

So, let's just say in the first hypothesis that these anomalies represent basalt that have ages that are randomly distributed between two million years and 11 million years.

You don't think, by the way, that any of these anomalies are younger than two million years old. They are too far below the subsurface to be two million year old or younger basaltic features.

But, if we just say that they represent randomly aged events, we would add in up to 35 volcanoes, 11 million years, temporal recurrence rate goes from two volcanoes per million years, up to three volcanoes per million years, not a really large increase in temporal recurrence rate.

Next slide, please. We also have to consider that maybe these are related to a younger episode of volcanism, something that's no younger than five million years old and has nothing to do with the past five to 11 million years.

So, if we just look at the available data, we have 19 events in the past 5 million years of temporal recurrence of four volcanoes per million years.

Add in the 11 anomalies, and again, assume that they are randomly distributed ages between two million and five million years and you end up with a recurrence rate of six volcanoes per million years that you could use in a sensitivity analysis.

One of the things that you may have noticed in the basic data is that the past events are not uniformly distributed in time. They tend to form temporal clusters.

Some of these clusters aren't very intense, maybe three events in a couple of million years. But, some of these clusters a little bit more intense than that.

Next slide, please. And here are what we're seeing, is that we have this one temporal cluster of about four million years ago where we know by these definitions we have 13 volcanoes in about point six million years.

That would give us a recurrence rate in that period of time on an order of 20 volcanoes per million years. Obviously that recurrence rate didn't occur for a long period of time.

But for a geologically short interval of time, roughly a half million years, there was an elevated volcanic occurrence rate in that interval. So, these anomalies also could represent part of that pulse of past activity, four million years.

If they were related to that period of activity, we would see the recurrence rate for a small interval -- say half million years in time -- come up to a rate of about 40 volcanoes per million years for a short duration.

Next slide, please. So, there's three altitude hypotheses you can use to evaluate the temporal uncertainty represented by these magnetic anomalies.

Now, depending on the time interval used, these hypotheses of the age uncertainties, you have about one and a half of the factor two increases in temporal recurrence rate.

So, we have to consider the possibility that our temporal recurrence may have range from two or three volcanoes per million years if you make a long-term, uniform recurrence rate assumption.

Also, clusters of activity -- that they have been as high as on the order of 40 volcanoes per million years. Now, with that, we can use analog volcanic fields to gain a sense of perspective for what those recurrence rates mean to volcanic fields in the western U.S..

And you can see, for Quaternary fields, and again, I'm restricted to the last two million years of data, because those are the only intervals that have good dating in these analog volcanic fields.

But, with the available information in Cima, your recurrence rates are 26 volcanoes per million years per a period of a billion, billion and a half years.

That would be 22 volcanoes per million years. And, up at Lunar Crater, it can get as high as 50 volcanoes per million years. So, you can see the upper bound on the range of recurrence rates that we would consider in sensitivity analysis for Yucca Mountain.

That upper bound doesn't exceed known

recurrence rates in the Western Great Basin. And, the lower bound also would be representative of a much longer lived volcanic field at the time.

The question really that we have to answer is what is the appropriate recurrence rate for the next 10,000, 100,000 or million years, not what is the absolute recurrence rate to some arbitrary period of time in the geologic past.

You have to forecast the future. And we believe that we have to evaluate multiple hypotheses in that evaluation of probability and not focus on a single interval of time in the past.

Next slide, please. How we are doing the sensitivity analyses. This is a familiar figure for many people. This is the published NRC probability model that uses clustered event locations and uniform temporal recurrence rates to calculate the probability estimates.

What we're seeing in this figure is the spatial recurrence rate based on the clustering algorithms that we used, normalized to the gravity outline of the Amargosa Trough.

And, again, for our probability estimate, we believe that the controlling structure that localizes magma in the region is that crustal extension zone in the Amargosa Trough.

So, in our models it would make no sense to say that there's a probability of volcanism in Bare Mountain, even though the statistical clustering might say that there is some likelihood in that area.

We believe we should pool that geologic information and normalize that controlling geologic structure, the Amargosa Trough, rather than allow for volcanism to occur -- to the incredible places.

We agree that the structural weighting that we use is subjective. But, it does account for the available data and does provide a transparent basis for review of that analysis.

The other good thing about the models we're using is we can accommodate the spatial and most of the temporal uncertainties that we're seeing in the currently available information.

We can evaluate the significance of those uncertainties using the probability analysis. Next slide, please. What we're using to evaluate the uncertainty is a tool called PVHA_YM, which is a series of JAVA applets that -- on anybody's web browser.

This is readily available from the Nuclear Regulatory Commission on all basic data sets. This is a screen snapshot. We put in the area of extent assumptions about recurrence rate and clustering functions.

The figures that I'm going to show on the next couple of slides come from screen snapshots from PVHA_YM. Next slide, please. Here is our basic example for the purposes of this talk.

Again, I'm just trying to give you a sense about how we can go about uncertainty analysis based on the current uncertainties in the age, location, and number of features in the Yucca Mountain region.

This isn't mean to be our position on what probability is or is not at Yucca Mountain. So, for this base example, I'm taking the 24 events that we previously defined, given a long-term average occurrence rate of two volcanoes per million years, and a simple Epanechnikov kernel that uses gravity weighting at a 90^{th} percentile.

So, we're re-normalizing gravity by 90 percent, allowing a little bit of slop around the margins of the gravity anomalies and a simple clustering algorithm is the plain English way of looking at that.

So you can see, by those basic assumptions, we have come up with a probability of a cone or a volcanic event intersecting the current compository footprint of essentially one times ten to the minus eight per year.

So, this is our starting point for this talk. Next slide, please. What do we do if we add in the 11 high to medium confidence magnetic anomalies, which are shown as additional black dots?

And so, I can explain that the black dots represent vent locations in the Yucca Mountain region. So, we add in the 11 high to medium confidence magnetic anomalies.

And let's just look at the mid-point of the uncertainty, when we are going between two and 40 volcanoes per million years. For illustration purposes, let's say the recurrence rate with those anomalies is not 20 volcanoes per million years.

You can see our base probability would increase from ten to the minus eighth, to one times ten to the minus seventh per year for those assumptions.

We can also use PVHA_YM to calculate the probability of a subsurface intrusion intersecting the potential repository. Given these assumptions for guidelines that vary between one and ten kilometers long, that probability of subsurface intersection would be on the order of seven times ten to the minus seventh per year.

If dikes were shorter, it would be down around four times ten to the minus seventh per year, given these 11 magnetic anomalies represented here.

Next slide, please. Now, we have some questions about present undetected volcanoes and how significant could that be. What I've done in this example -- and believe me, there are many examples you can run with this -- I have added five randomly located volcanoes in Jackass Flat.

Hit the spacebar please. There should be a pop-up. There we go. Five anomalies in Jackass Flat. This is randomly located to try to look at sensitivity for undetected events east of the potential repository site.

And you can see that, if we have the same recurrence rate -- 20 volcanoes per million years -- our probability only increases from one times ten to the minus seventh, to two times ten to the minus seventh by adding these five locations into the dataset.

And, again, a similar increase would occur by saying that these are -- we also would have igneous dikes and subsurface diversions. We go from seven times ten to the minus seventh, to eight times ten to the minus seventh.

So, here is one of many possible examples that show that adding five events that have been undetected -- adding those undetected events into the probability dataset, it doesn't have a very large effect on the probability estimate.

And, again, if you want to do some additional analyses, you can use your own locations, own number events, and see how these models are sensitive or insensitive to the addition of potentially undetected events.

Next slide, please. So, what did we learn from all of this? First, kind of interestingly, the addition of the anomalies into the dataset doesn't really change our spatial recurrence patterns very much.

In other words, the anomaly locations are following the known event locations, and not having a profound re-alignment of our spatial patterns in the Yucca Mountain region.

More volcanoes are located toward the existing locust of activity than they are distributed in areas away from that known locust around Southwestern Crater Flat.

We also see, by running a number of these simulations, that clusters of more than five undetected volcanoes appear to be meeting with a change our spatial recurrence rates have is significant.

We already thought patterns that are pretty well established by the existing data, including the magnetic anomalies. So, to perturb those patterns in a way that would grossly affect the probability of potential repository site, you have to create a pretty intense cluster of undetected events on the east side of the potential repository site.

That cluster would have to have more than about five volcanoes located within a couple of kilometers of one another in order to create that spatial recurrence based on our models.

Also, we are seeing that the uncertainties in the temporal recurrence rate for short periods of time -- and by short, I mean 10,000 to 100,000 year periods -- those variations are not really captured by the existing uncertainties that we have in long-term recurrence rates.

In other words, the million year average, the variations that we see in the million year averages really aren't capturing the potential variations for shorter intervals of time when we could have higher recurrence rates than the long-term average.

Finally, the cluster of past events gives short-term recurrence rates that are comparable to other Western Great Basin volcanic fields. Again, those recurrence rates don't exist continuously through time.

But we're not looking for 11 million years in the future. We're looking for some shorter interval of time in the future, time to forecast what's the likelihood in that future time of volcanic eruption.

And, finally, evaluate the large uncertainty anomaly ages and anomaly locations by testing alternative conceptual models and looking at the sensitivity of those models to the resulting probability estimate.

So, to wrap it up, next slide, please. In looking at the current uncertainties in the number, age, and location of past events in the Yucca Mountain region, we have concluded that our conceptual basis for the probability estimate has not been affected by those uncertainties.

We're not seeing anomalies outside of

areas that previously we had defined as the structural basis for probability or clustering effects that we can't account for in the current probability model.

We can evaluate the effects of the existing spatial and temporal uncertainties on the NRC probability estimate. And we had questions before about what are reducible uncertainties.

One of the key areas for reducible uncertainty is the potential for undetected events, I believe is a very reducible uncertainty. And I'm optimistic that the new data that are being collected by the Department and the high resolution magnetic survey will help to resolve that uncertainty more than the current data can do.

Our best estimate of the effect of these current uncertainties it can get a factor ten increase in the NRC probability estimate relative to these base models.

That kind of a factor on the probability estimate gives us a high significance to performance calculations. So, we are going to need to have a good basis to review those uncertainties and a traceable basis to document those uncertainties during our potential license application review.

Finally, we also can conclude from doing this work, that the potential effects of current uncertainties on the number, age, and location of past events really can affect some of the assumptions in the conceptual basis used in many probability models, the key interpretations of past spatial and temporal patterns.

And finally, these uncertainties can also directly affect parameter ranges used in any probability model for the Yucca Mountain region. Thank you for your attention.

CHAIRMAN RYAN: Thank you. I guess we'll start with any questions from the members. Allen?

MEMBER CROFF: In going back into this, I look at your slide 15, which shows, I think, your basic probability contours. I think the high being to use all the exponents above 18 or 20.

And the Yucca Mountain site being -- I'll call it eight roughly. But then, when I go back and look at the diagram say, on page seven, which shows the magnetic anomalies, it shows, to me, sort of a clustering of these anomalies in certain areas.

And in other areas, such as the bedrock, where the Yucca Mountain site are, and other areas of bedrock, essentially zero recurrences over all time.

Whereas, the probability model you end up with has about a factor two probability difference. And that intuitively doesn't seem right to me. Am I missing something.

MR. HILL: There are a few points that I can clarify for you. First, there is an event located about 200 meters from the northwestern edge of the repository site.

That's our roughly ten million year old basaltic canyon dike and eroded vent complex. It's a very small feature, but a very significant feature.

So, given these past events, like the models have consistently said, the highest likelihood for the next event would be in that southern part of Crater Flat, not in that potential repository site.

But, through time, there has been an event coming very close to that location. And, that would scale as about the order of magnitude reduction in recurrence rate given the number of events that we have -- 20 events, 30 events, one out of 30, as opposed to the two orders of magnitude or continues.

Second, the probability map isn't really a probability map. The contour lines are spatial recurrence rate. And then you have to multiply spatial recurrence rate by the chemical recurrence rate by the area of intersection, which is about five square kilometers for the current.

So, to translate in figure 15, those contour lines in the probability, you have to define probability in what area. We're using, of course, the five square -- in this case, the seven square kilometer repository footprint.

So, to calculate the probability, you have to average some spatial recurrence over that interval times the temporal recurrence, times the area.

So, these contours are volcanoes per square kilometer using that specific kernel function.

MEMBER CROFF: Okay, so what is approximately the difference in the probability of a volcanic event in your base case, between the peak in the middle of the valley, and the Yucca Mountain site?

MR. HILL: It would be about -- if we were saying ten to the minus seventh at the potential repository site, it would be approximately ten to the minus sixth at the center of the locust of activity in Crater Flat.

And it would be about ten to the minus eight when you get to the edge of the Amargosa Trough out there just at the western edge of Jackass Flat.

MEMBER CROFF: Okay, thank you.

CHAIRMAN RYAN: And, again, that's average per year?

MR. HILL: Yes, probability per year.

CHAIRMAN RYAN: If I could follow-up just quickly, you talked about the spatial aspects. I'm real interested in the temporal aspects. When I look at the temporal distributions as a math problem in trying to predict, you know, recurrence or a look at recurrence interval, can you think of any strategies to address?

The aeromagnetic survey updates will do the spatial work. But, how do you attack the uncertainties in the temporal distribution?

MR. HILL: Well, again, it's do we evaluate this as a homogenous or non-homogenous process? And, in the absence of data, you just have -- you hypothesize.

So, we can take a rigorous statistical approach to evaluate what is unconstrainable in terms of the age uncertainty. What we need are the data, which would be the proposed drilling program that will look at some of these anomalies, drill down and sample whatever is causing those anomalies.

It may be a welded tuff that's been faulted. It may be basalt. If it's basalt, we need to get those data. I think that's a very straightforward process.

CHAIRMAN RYAN: So really, the drilling is how you get at the age distribution and prove your temporal --

MR. HILL: Right. And, if we have that age information, we can factor that into the uncertainty estimate. Again, this is not our view of how things will be.

But, it's an attempt to present to the committee how we can evaluate the currently available uncertainties with currently available information.

And then, of course, as new information comes in, you can use these methods to evaluate that new information for the licensing process.

CHAIRMAN RYAN: That's coming through well. And I appreciate you clarifying that again. Ruth, a question?

MEMBER WEINER: Is there a microphone?

CHAIRMAN RYAN: Oh, sorry.

MEMBER WEINER: I think John's -- first of all, I'd like to congratulate you on making the PVHA model available. I did play with that, and it works very nicely.

And I think you all aught to be commended for that.

MR. HILL: Our consultants Laura Connor and Chuck Connor were the real --

MEMBER WEINER: Well, convey to them my congratulations.

MR. HILL: I will.

MEMBER WEINER: I have what's probably a very simplistic question about the spatial density. And that is, you outlined very carefully the area that you were looking at for the Crater Flat, Jackass Flat volcanoes.

How does that area compare with the comparisons where y have volcanic fields that have a higher density of events, higher spatial density of events?

MR. HILL: I think we're looking at fairly comparable. I would want to check on that. But, we're not comparing huge fields or microscopic fields compared to the area that we're dealing with for Crater Flat, Jackass Flat.

The entire basin, the Amargosa Trough, that contains the volcano is bigger than the Quaternary part of a number of these fields. But I think it is comparable to area for Lunar Crater field, which is a bit more extensive. Is that addressing --

MEMBER WEINER: It does address it. The

thing that is of concern that I picked up on is, if you define the area differently, how differently do you need to define the area to make a significant difference in the spatial density?

MR. HILL: On all of these definitions, the area is defined by the extent of mass -- not connected on the margin, but pretty close to the margin of that, and also accommodating the very obvious structure.

Like Bare Mountain, we wouldn't include that potential area. And the same thing in a place like Lunar Crater. You're not going to expand the area out into the alluvial basins just to get a bigger area.

You define it right around where the mapped volcanoes are. And so, in the scale on order of magnitude, these are comparable. In detail there is going to be some variation. But we're not taking a comparison with a huge volcanic field to come up with spatial densities.

MEMBER WEINER: Okay, thanks.

CHAIRMAN RYAN: Let me open it up for questions from our panelists and participants and consultants. Bruce?

MR. MARSH: Yes, it's a very interesting

presentation. One of the things I've always been amazed over in the Western United States and in volcanic terrains themselves is that, if you actually look at the solid rock areas, where we know the geology the best, you don't see much signs of volcanism compared to what we see in valleys, for example.

Our discussion today, for example, is all mostly concerned about things that perhaps we don't know what's going -- buried in these valleys. And, it would be interesting, I think, in some ways, to adopt a different view, in other words, build a probability model that didn't use anything in the valleys, but only used solid rock data information.

The repository, for example, the mountain ranges are all solid rock. We know the geology there well. We can see what happened there. And, if we built up, for some reason, for example, there aren't a lot of cinder cones up in the mountains on the solid rock areas where you see the geology very well.

It would be very interesting, as an alternative to build a probability model using only the areas of solid rock in the mountain and say, okay, we know we can see the dike, maybe a cinder cone, and build up a model like that, and then use that for the whole region.

In effect, now we are doing the reverse. We are actually taking all the stuff in the valleys, the alluvial fill and things we don't know, and we're putting a model forward that we're pervasively using in the regions where we have the best geologic control.

And it's odd that, in many ways, you know, volcanoes just don't seem to appear ever in some areas, regardless of what's going on nearby. And, so, have you thought of this in trying to build a model like this?

MR. HILL: We thought about this a lot. And, while maybe true in some areas, we see in other areas the fact that volcanoes do erupt, which are characterizing as solid rock.

It depends very much on what are the controlling structures in the region, and what are the areas of local extension, versus local compression, to put it very simply.

In places like the Big Pine field, you see them coming up the range of the Sierra. Some of them are in the valley, and some are buried in the valley.

But other volcanoes come up and are essentially sitting there in the foothills of the

Sierra Nevada. In the Yucca Mountain region, we see not only Solitario Canyon Dike, but also up around Thirsty Mountain we see the hidden cone sitting all on a bedrock bottom.

You know, there are plenty of alluvial basins sitting around there. The reason it's all that high is structural control, not anything to do with whether the bedrock is above surface or below the surface of baseline alluvial.

One of the reasons -- well, I'll back up for a minute. The existing pattern of volcanism already reflects that control. We have no basis to say that Yucca Mountain is somehow a zone that magma physically cannot get into.

The current patterns show that, while it's less likely for it to go there, it still can go there.

MR. MARSH: Well --

MR. HILL: The greatest likelihood is down where we are seeing the most volcanoes. But, at a process level, the controlling structure is not whether a couple hundred meters of bedrock sticks up above the alluvial or is below the subsurface.

It depends on those structural elements that are important for mobilizing the magma and allowing breakout at certain points.

MR. MARSH: Well, I mean, that sounds interesting. But, in fact, it is the numbers and the effects in the model that we really need to put in.

For example, we know that the regional stress fields direct the localization or the local dispersal of magma. So, when a cinder cone is erupting, for example, these -- what we see -- reinforced yesterday, for example, is that there's an extreme north-south predilection for the magma being dispersed.

So, one of the things that missing, I find in this probability model, is the detailed local characteristics of the structure that you're mentioning.

Structural integrity is expressed on a local basis, let's say on an area that involves, let's say, you know, 10,000 square kilometers, 5,000 square kilometer area.

That detail, that granularity in the model where you need to put those details in this regional stress field and how that influences it, is extremely important.

Instead of having a very dispersed line sampling kernel like this, it spreads as an umbrella over the whole area. It doesn't have any granularity in it for the integrity.

For example, in a big earthquake we know we built buildings on areas that are alluvial areas that may undergo -- basically quicksand. How do you stabilize a building?

You build a big sub-structure on it. You put basically a boat in the earth's crust there. And this building will sit there and sway back and forth and be perfectly fine.

If you don't know anything about that granularity and detail of structure, you would predict that everything would just collapse into the earth when, in fact, it actually has this integrity built into it to make it survive.

I'm worried that we're looking at detailed numbers. And these numbers are so uniformly spread as kind of a wide umbrella here that we're missing very important granularity in this.

And, as you're mentioning, there are areas where we cinder cone things spread up on sides of -- in the Sierra's, for example. We see it in Antarctica.

We see other places. But, we don't see it here. And that's something that's special to this area. And I'd like to see that somehow evaluated or built into the model, because we do have a lot of variations that's due to the stress fields, for example.

But, that is a particular characteristic here that doesn't seem to be in the model.

MR. HILL: Do you think that we have the science that would allow us to make that sort of deterministic approach that certain areas are structurally or mechanically facilitating?

MR. MARSH: Absolutely.

MR. HILL: What do you think those would be.

MR. MARSH: I mean, we worry about it all the time. We can see things even using the models that -- developed. For example, for years and years, looking at stress fields around volcanoes and knowing where the dispersal is going to be.

MR. HILL: But you're talking about around a volcano, you know, gross perturbations in the local and regional stress field. Here, at Yucca Mountain, we're talking about first characterizing the stress field in the alluvial subsurface, which would be a very challenging thing to do with the available information.

Second, the pattern -- as best we can tell

-- really doesn't change from in the alluvial part of the basin, out into Yucca Mountain. Those variations are continual.

We're trying to, in the first order, represent a continual variation in deviatory stress. So, what I'm really getting at is, in some areas I agree, that there are profound changes in the local stress field that could be used.

In this particular condition, though, we're not dealing with huge or large variations of deviatory stress. They are very subtle.

MR. MARSH: Well, let me get down to actual some detail here. For example, this area is heavily fractured in the north-south direction. So, if a magma is coming out, since there are so much availability to run in north-south direction, the probability, for example, if we were to look at the propagation of dikes, the probability is very large that it would go in a north-south direction, rather than in east-west Director, for example.

MR. HILL: Yes.

MR. MARSH: So, that should be built in in great detail. In other words, these cones, for example, in terms of setting off a dike that would be off east-west in any of these would be a very low probability event relative to a north-south event.

MR. HILL: When we talk about the dikes in a probability estimate, we have a variation from zero to 20 degrees that reflects both the regional structure as well as the deviatory stress in the region.

We're not considering the probability of east-west dike, because it just wouldn't occur in this.

MR. MARSH: Right. I know. But those ripples do no appear here. For example, if we have an eruption at one of these centers, when we look at in detail with exactly the same space, we should be able to predict in great detail, in terms of the volumes involved.

And that would also give us some limit on the dikes, but also where the dikes are going to go. We should then have a much different basic umbrella probabilities than we see here.

MR. HILL: Well, this is the spatial recurrence pattern --

MR. MARSH: Right.

MR. HILL: -- for the volcanic event. It's treated as a point source, not a line source. So this model was not intended to try to represent the distribution function of a linear event.

It's a point. That's the whole thing with this probability. I can't really extemporize on how you go about making a linear event probability model in that sense and then look at the variations in three dimensions of the regional stress field and pull that in to a normal function.

But, my personal review is that we don't have the data or ability to resolve this on scale or kilometer at the Yucca Mountain region to say that, if we move over one kilometer, that we can grossly characterize this as favorable for magnetism or unfavorable for magnetism.

All we're seeing is what's right there at the surface. And the magma isn't coming up from a very shallow inter-volcanic magma field. It's coming up from depth that is controlled by the regional structure as well as the local structures in the near surface.

CHAIRMAN RYAN: John, you had a comment?

MR. TRAPP: Yes, this is John Trapp. Is this on. This is John Trapp of the NRC. I would just like to make a couple comments. Number one, it is not the NRC's job in licensing to provide the probability model.

It is our job to evaluate the probability models that are presented by the Department of Energy. Second point, if you want a north-south model, there already is one.

Smith, from the State of Nevada, had published models in which they have basically taken a north-south structure and used this and compared the results of their models with the models in another direction.

You will find a tremendous -- up to a couple orders of magnitude in the results of the probability. Third, if you want to go into the detail that you are talking to, again, this would be something that should be directed at the Department of Energy as far as characterization studies that should be taken.

There are a lot of things that would be nice to put in a deterministic patter. But we just don't have them.

CHAIRMAN RYAN: Other questions. John Garrick had a question.

MR. GARRICK: I wanted to talk a little bit about the probability calculation itself. One of the great difficulties is getting our arms around the issues of uncertainty and the issues of igneous event scenarios and thresholds of concern.

On the uncertainty issue, Rick, you articulated very well a number of scenarios that resulted in different volcanic frequencies. As one way of getting some additional insight into the uncertainties involved, have you, in getting to your bottom line probability numbers, have you embedded those frequencies in probability distributions to reveal how the uncertainty varies with respect to the different scenarios that you presented?

In other words, it lends itself very nicely to doing that. And, you developed some probability frequency curves that would really give some illumination and insight as to the uncertainties for the different categories of events that you described.

MR. HILL: Right. I agree we could. We have not done that. We could do that to look at a distribution given these parameter ranges, parameter uncertainties, what would be the resulting effect.

Essentially, the same as I presented a very deterministic sense here, you can do a more stochastic analysis that would give the full range. And it could then be integrated into another distribution. We have not done that.

MR. GARRICK: I just see it as another opportunity to get additional insight into where the uncertainties are as a function of the igneous event scenarios that you should be worrying about.

MR. HILL: I want to make sure I understand. When you talk about the igneous event scenarios, are you talking about a different consequence scenarios or evaluating different probabilities for different --

MR. GARRICK: Well, yes. I have trouble separating the probability calculation from the consequence calculation. And, when I think scenario, I think from initial condition to the consequence.

And, with the underlying assumptions associated with the consequences because there's some certainty associated with them as being part of the makeup of the probability.

So, my -- is different than the way it has been presented.

MR. HILL: Right. But, in a very simple sense, what we're looking at is the igneous event is the initiating event in the event sequence.

MR. GARRICK: Yes.

MR. HILL: And then we have two branches in the event, one for volcanic disruption, one for intrusive disruption. But the probability of both of those branches still comes back to a singular issue.

We don't have a discrete probability for those the way we are treating the performance assessment. And, within the sub-branches of volcanic or intrusive, we don't have the data to begin to say that we have a probability distribution for this class of initiating event gives us this sub-class on a volcanic event consequence.

MR. GARRICK: But I suspect you have some sort of evidence that would allow a certain level of discrimination between your supporting evidence for these different frequencies.

And that might turn out to be very important to characterizing the overall uncertainties of the probability. That's just a thought.

MR. HILL: Yes. It's certainly something that we've thought about from day one of the program, because this does appear different from how you would do a seismic hazard analysis where you have a large variation in the magnitude of the initiating event.

A large volume data to characterize the frequency -- have this large range of initiating events. And a hazard that is directly related to the magnitude of the initiating event.

But this comes back to the point that John Trapp was talking about earlier. We're not looking at a range of initiating events like you're doing in seismic where you go off the magnitude seven and half down to maybe magnitude three.

Our hazard and earthquake space would pretty much be about a magnitude four. So, we're not sampling that entire magnitude range using this analogy.

Our initiating event is restricted to a kind of earthquake analog that would be only about a magnitude of four. So, we don't have to consider large changes in the hazard because the initiating event has a small range in consequential hazard, unlike the earthquake scenario.

But, it's again something that we continue to look at. We hold in a lot of the variability within that narrow initiating event. We still have variations in eruption size, eruption duration, etcetera that reflect a lot of the uncertainty in the event.

But, we're not using a strict probability linkage between the larger volume range having one probability to the smaller volume range having another probability, for example. The data just don't support that.

MR. GARRICK: Thank you.

CHAIRMAN RYAN: We'll go to George Hornberger and Bill Hinze.

MR. HORNBERGER: I'd like to perhaps approach Bruce Marsh's first question from a slightly different approach angle. So, Rick, I think if I can loosely summarize here, you said that you're base case temporal recurrence rate at Yucca Mountain would be something like ten to the minus seventh or eighth.

And then if you add in the potentially hidden features, it goes up to ten to the minus seventh. And then your sensitivity study said it could increase another order of magnitude.

Okay, now, my question is, with any of those estimates of temporal recurrence, and now if we restrict our knowledge to the hard-rock geologic features where we have the best information, are any or all of those temporal recurrence rates consistent with the observed features within the hard-rock portion of the area that you're considering?

MR. HILL: By hard-rock you mean the surface exposures at Yucca Mountain? I'm not quire sure what you mean?

MR. HORNBERGER: Again, Bruce was

suggesting -- his question, why not build a model just based on features not in the valley. Now, my question is, turn it around. You've estimated frequencies at Yucca Mountain.

And, presumably, we can take Yucca Mountain to be not in the valley. And so, we have observation throughout the region of dikes, like you said, the Solitario Canyon being the closest to Yucca Mountain.

We can count up the number of observations we have that are not in the valley. Do the number of observations we have over 11 million years, are they consistent with your estimate that is ten to the minus seven or with your estimate that is ten to the minus eight, or your estimate that's ten to the minus six?

MR. HILL: I think, if I understand -- first, I would just want to go on the record as saying that I don't believe that there is a controlling difference between a couple hundred meters of bedrock versus bedrock being a couple hundred meters below alluvium that changes -- head just isn't significant in the sense of magma.

But, to answer your question very directly, if I was to outline the bedrock exposure of Yucca Mountain and say, how many events have occurred there in the past 11 million years?

Again, I want absolute certainty of if you include Lathrop Wells in that dataset or not. But, let's ignore Lathrop Wells. We just have one in the past 11 million years.

That would be the Solitario Canyon Dike. If you believe that they are discrete probability issues, which I do not believe, between Yucca Mountain and the adjacent part of Crater Flat and Jackass Flat valleys.

MR. HORNBERGER: I just want to restrict it to just the footprint of Yucca Mountain because we have this whole area.

MR. HILL: Okay.

MR. HORNBERGER: And you have bedrock exposure across the whole area. So, don't restrict to Yucca Mountain. How many events do we count in your database that are in the bedrock exposure.

MR. MARSH: Let me interject one thing here. I think you do believe that exactly, Britt, because you put a emphasis on the Amargosa Trough region.

The way that's drawn and the basis of that is extremely important. You're using that as a guide to bring magma. If we actually exclude the mountain ranges to the east, including Yucca Mountain, of that -- in other words, we did detailed gravity, and maybe did the isostatic correction a little differently, the Amargosa Trough would be defined in such detail that the regions that we're talking about are outside of it.

So, you actually do believe this, without realizing it, because the Amargosa Trough you're saying is the preferred area to go. And things that are happening that -- that's a heat transfer zone.

So you're actually believing it without realizing it.

MR. HILL: No, I don't believe we do. We have seen many other people's interpretation, including many of the U.S. Geological Survey. We have looked high and low to find what it is.

Is there a change in structural domain between the dirt in Crater Flat and the Rock in Yucca Mountain. So, we don't see a crustal structure there.

This is just part of a continuous extensional basin that's in -- with maximum extension on the west and -- extension on the east. And Yucca Mountain is part of that continuum of extension.

So, I'm not going to agree that somehow there is a large or significant or controlling difference between the structural domain at Yucca Mountain versus what occurs to the west, except that this is an extensional basin, and you have a base level alluvium that is covering part of that basin.

But, in terms of what controls the ascent of magma, it's not the upper couple hundred meters of dirt. It's that large scale structure.

CHAIRMAN RYAN: Bill, you had a question.

MR. HINZE: Well, --

MR. HILL: Let me go back to George. Within this structural basin, we have 24 events. One of those events has been within a couple of hundred meters of Yucca Mountain, on that bedrock exposure. So, one out of 24 in 11 million years.

CHAIRMAN RYAN: Thank you. Bill?

MR. HINZE: Why don't we have this 15th illustration? If we set ourselves back 80,000 years ago, I assume that this spatial temporal clustering is impacted by the presence of Lathrop Wells, which you pointed out there.

But, I suspect much greater. Have you tried it out?

MR. HILL: Yes, we have.

MR. HINZE: And, how was the probability changed -- the contour changed between Yucca Mountain and Lathrop Wells? In other words, I read eight times ten to the minus eight at the repository and eight times ten to the minus eight at Lathrop Wells.

If I was back there 80,000 years ago, I would expect that to be eight times ten to the minus eight.

MR. HILL: Well, certainly this is something that anybody can do using the PVHA tool -- go in, edit the volcano dataset, go out, take Lathrop Wells out of the dataset, run the model, use your preferred assumptions and see.

This is a general guide. The addition or subtraction of one event doesn't change the spatial patterns significantly. So, you would see that you'd have the same basic spatial pattern about, like you were saying, eight per square kilometer or -- I forget the exact unit -- volcano per eight square kilometer.

I think that was the spatial recurrence of that particular point per square kilometer. And that would be about the same recurrence rate -- eight of ten -- that you would have before Lathrop Wells existed.

So, in the end result, it is really comparable to Lathrop Wells. It is important, Lathrop Wells did form in the most intense part of the field.

It was out there out around the eight to ten, not 18 or 15 in terms of simple recurrence, similar to what we see at the potential repository site.

CHAIRMAN RYAN: We're getting close to the end of the session, so let's --

MR. HINZE: Another brief question, or I want to make sure that we're together on nomenclature here. And I'm stepping into Bruce's space here.

The PVHA expert elicitation had this hidden event factor of 1.1 to 1.5, something like that. These are your undetected events? That's a question.

MR. HILL: I'm afraid we're getting into an area that I really can't speak to in this meeting in commenting on the Department of Energy's --

MR. HINZE: Is your definition of undetected event and hidden event factor in the PVHA the same thing?

MR. HILL: No, they are not. What we mean by undetected events is events that slight characterization has not detected in terms of volcanic features, not dike eruptions or dikes that haven't gotten to the surface.

MR. HINZE: Okay, so it's not fair to compare the hidden event factor effect upon the PVHA?

MR. HILL: Not always. Sometimes it is, sometimes it isn't. It depends on whose definition.

MR. MARSH: I just have something brief.

CHAIRMAN RYAN: Yes.

MR. HILL: Yes please.

MR. MARSH: Getting back to the issue, your reason on how you count events and what is event. I think that's a salient issue to be worried about.

And, one of the things is that, an event, for example, if you look at those flows out there at Lathrop Wells, for example, and you've been around volcanoes that are erupting, you can see that these things have these big -- you know, there are small lobes and there are tractor tread type things.

And they're kind of pushing towels ahead of them. And they're moving along maybe at meters per days some places, maybe meters per hour other places.

But if you live there -- let's say you have a little hut nearby -- you'd be worried about hour-to-hour. An event would be a boulder falling off and rolling over your house.

That would be an event. So you would call that an event. But, if you're actually concerned about -- you live, you know, five miles away, and you're concerned about a dike coming out and hitting your house or hitting your farm, that's a different kind of thing to think about for an event.

For example, because when, as you know, centers establish themselves, most of the destruction, most of the dispersal of the dike's warmth is early.

It concentrates down to something more. So, maybe we actually should think about counting events in several different ways. For example, the outpouring itself would be one event.

We would think about each one of these things as a just event, no matter how many small effusive cones it had near by. That would be one extreme.

And that would be for, let's say, a disruptive event -- up through Yucca Mountain. On the other hand, we could have another one that sent out dikes.

And we worry about then the radial component of sampling kernel I was talking about before in the stress field. And that would be a different kind of event we'd talk about.

So, these are different ways to calculate, instead of lumping them all in and saying, you know, there are going to be hidden things that are -- you know, 24, 40, whatever.

We would actually classify these kind of in a category that John was mentioning earlier, in a hierarchical structure and based somewhat on outcome in their potential destructiveness in terms of what their potential capabilities are.

So, have you thought about this or tried to build this up.

MR. HILL: I'm not sure I really understand the comment, the event. First, we don't have a minimum threshold event below which igneous activity would not create a potential hazard if it intersected.

Second, these are all for direct disruption. In other words, the dike or the volcano would have to penetrate the footprint of the repository.

And that's the only conditional probability to worry about because that's really the only hazard. For the volcanic event, we're not worried about the lava flow, because encapsulation of a lava flow is not going to create a potential hazard at a location 20 kilometers down range.

It's only that part of the eruption that produces the dispersed tephra that truly caused the hazard for the RMEI who isn't living at the volcano. We're not worried about rolling rocks on the RMEI.

We're worrying about penetration of potential repository site. So, I think a number of these assumptions are already built into the basic probability model.

The thing with event that's, I think, a bit more important to you is you can also define events as like the Crater Flat center. That could be little cones, Black Cone, Red Cone, and Northern Cone as a single event, depending on how long you want the event to last, the same way Sunset Crater has multiple vents and discrete hiatuses and activity.

The reason I chose this particular definition is not because it is the correct definition, but it is the simplest definition. Here is a cinder cone, here is an event.

Here is an anomaly, here is an event. I don't have to make assumptions about the nearest neighbor is a part of that event in defining distribution of event sizes or event areas.

Because, once you start say an event is a series of points, then the point has an area term that has to be tracked as well. Here, because the point --

the vent -- is small relative to the area of interest, the footprint -- five square kilometer -- it is treated as a simple point-source and not worry about the area itself.

MR. MARSH: My point is that this is a pretty serious issue in that getting at one question earlier -- people were saying what's your uncertainty in your definition of these?

This is a way to get at those things. And it's worth, I think, taking the time to actually look at them.

CHAIRMAN RYAN: That's probably --

MR. HILL: -- ponder a paper about the sensitivity of the probability estimate in event definitions.

CHAIRMAN RYAN: Let me ask that we continue the discussion after we take a break and hear from the other speakers. I'm sure we'll more into the details of this as the next two days go on.

Britt, thank you for a wonderful presentation and answering all the questions. I think the dialogue is wonderful. So, thank you very much.

We'll take a break now. It's ten o'clock. We'll reconvene sharply at 10:15.

(Whereupon, the above-entitled matter went off the record at 9:59 a.m. and went back on the record at 10:20 a.m.)

CHAIRMAN RYAN: We had one request for a brief question from Mike Sharpton, the University of Buffalo. We'll go ahead and catch that question now.

PARTICIPANT: Okay, thanks a lot for allowing this. This question is for Britt. In your reply to some of the questions from the panel, you use this as an example of a volcanic event in bedrock as the Solitario Canyon dike.

And that's ten million years old. Now, in the analysis of the PVHA panel and the probabilities that we've been using, we only considered volcanism from four million years to the present.

What is the reasoning for using these older rocks, because the tectonic regime was probably different at ten million years from one million years.

MR. HILL: The simplest answer is that we don't believe the tectonic regime was that much different ten million years ago as to a comparable current tectonic regime.

One of the papers by some -- reports on paleomagnetic direction data for this data. It shows that most of the extended and rotation that accompany the end stages of -- have been accomplished by the time and in the place of a basalt, for example -- Canyon -- in the southern part of Crater Flat.

So, the tectonic regime had been set by that, which is comparable to the tectonic regime that we see in the present. It doesn't mean it's identical.

But, it's near an episode of tectonic. Second, the petrogenesis of those lavas -- fit the Dikes are preserved, Solitario, the Miocene rocks that are in the drill, the southern Crater Flat basalts.

The petrogenesis in the variations that you see in the basalts is the same petrogenesis variations that you see in the Pliocene and -- rocks in Amargosa Trough.

They have a common petrogenesis. In contrast, if you go out to places like Skull Mountain and look at basalts there, you will see a very different characteristic.

The vapors -- there's a lot of this equilibrium. There's a lot of quartz, zenecris -- and the white elements are floating around. These are giving all the signals of magma that sat in the salicic crust in response to that larger tectonic regime associated with the calderas.

So, we believe that the Miocene within the

Amargosa Trough is relevant to understanding past patterns of igneous activity, because the petrogenesis of that basalt and the tectonics within that basin has been a continuum of a similar process for the past 11 million years.

In contrast, outside the basin represents a separate sort of event that doesn't give us any real insight on what's happening in recent.

PARTICIPANT: Thank you, I appreciate it.

CHAIRMAN RYAN: Moving to our next speaker, Dr. Bruce Crowe is here. He's going to talk about the 1996 probabilistic volcanic hazard analysis, one subject matter expert's perspective. Dr. Crowe, welcome. Thank you.

MR. CROWE: I like to stand by my slides and walk around with it. So, if people can hear me, I would prefer talking from there.

CHAIRMAN RYAN: Okay, we have a pointer.

1996 PROBABILISTIC VOLCANIC ANALYSIS: ONE SUBJECT MATTER EXPERT'S PERSPECTIVE

MR. CROWE: Okay, the reason I call this an out-of-touch look is I left the program in '96, so I want to just make clear that I have time. I reached the point where I told Frank just not to talk to me.

So, this is defining a cobweb. So here's what I'm going to try to do. I'm going to focus on how the logic and the assumptions and particularly the framework geology I used to construct my PVHA model.

And it was fun to be in the PVHA because we were allowed to say, okay, what is your best guess as an expert at how you think is the best way to do these calculations.

Mike Sheridan was involved. He can keep my honest when I deviate. But, it was fun to do it where we actually could inject some personal opinion and some personal biases into the program.

So, I present that. I also put that together for a book chapter that I wrote that was supposed to come out two years ago. I never know when it's going to come out.

I put together an influence diagram that I tried to assemble the logic of how you do these probability calculations. I'm going to step through that and kind of use that as a framework for my presentation.

And then I kept some new perspectives. I've been doing a lot of probabilistic PA modeling for the Environmental Management Program. And I've been working with Bayesian statisticians who have educated me a lot.

I've really learned that the best way to do is -- a geologist can do the prior, but let the Bayesians do the posterior and handle all of those messy curve fittings and those sort of problems.

And then, I have to interject some biases. I'm going to talk where I think that you can put some fairly logical arguments together whether there might be some bounds on the probability limits for these calculations.

And then we may be approaching the limit. We're getting down. And I think it is time to move on. But, again, it's a distant perspective. They gave me a whole bunch of handouts.

And I looked at them and stole a few slides from them. But, I don't profess to understand everything that was in all those handouts.

Way back in 1978-1979 when we started on this probably, when they were kind of focusing in on Yucca Mountain, after they looked a number of sites at the test site, I made the mistake agreeing.

I was told by the USGS to go look at these basalt volcanoes. It would just take you a couple of months and then you can move on to something more interesting.

Here we are in 2004. But, anyway, what we always pointed out was that, rather than use risk -- I probably should be saying hazard -- you have a fairly low hazard of disruption.

And ten to the minus seven and ten to the minus eight numbers are low numbers. But, what we've always pointed out is because you have a small number of volcanoes, you're always going to have a lot of uncertainty.

There's just no way of getting around that uncertainty. You have a lot of irreducible uncertainty by virtue of a limited geologic record. If you had a lot more volcanoes, you would have the luxury of having less uncertainty.

But you would have much higher risk. And so, clearly you want the trade-off. But it means that there are some limits to how well you can define this probability.

And so, what we always argue at that point, and I think it carries on today, is that you're going to have multiple permissive models. And, in my opinion, you don't have the dataset to resolve those models.

So you really shouldn't get too caught up into what is the correct model. But instead, you should look at what are the impacts of a whole spectrum of models and then use that to guide your intuition on the significance of the problem.

Okay, so here's starting out. This is basic probability that I first worked out in the late 70's. It still kind of holds. And, basically, just as the probability that for a disruption to occur -- the repository -- has to be an event somewhere in the region or in a volcanic zone.

And that event has to intersect or hit near the repository to be an issue. When we first put this probability together, we argued that these were independent events.

But, there are some couplings in these that I'll be talking about that I think are important, that affect how you assemble the probably calculations.

So, this represents an influence diagram. And I did the program -- and each box is set to show the different types of variables that go into this equation.

The square boxes represent either decision uncertainty or decision assumptions that you have to make in order to do the calculations. They are like boundary assumptions or modeled assumptions.

And you really can't treat those

stochastically. They're basic fundamental assumptions that you have made. Or, in the case of the repository, this is a decision variable that the DOE controls.

It is changed dramatically every year. I always have to go look up what the new repository footprint up. But, it has no uncertainty whenever the DOE finally firms up what that repository area will be.

These ovals that are here represent things that you can treat as stochastic variables, or you can treat them as a PDF and calculate them as stochastics.

And then you actually couple those together to calculate the recurrence rate. And then that feeds into the repository intersection. I'm going to stay out of this area.

I don't want to go there at all. So, I'll just be talking about these two things, E1 the recurrence rate, and E2, the probability of repository disruption.

Okay, so what we have to start out with with the experts was they said, given the conceptual model of why you think volcanoes are out there. And so, I kind of stepped back.

This is a diagram I borrowed from one of

Frank's papers. And I said, what's interesting, if you look at the base -- the Great Basin, the Southern Basin range, the Colorado Plateau, most of the activity is volcanic activity, is concentrated on the active margin.

But there's kind of an interesting tendency that you get small bits of volcanism in the interior parts, both the Great Basin, the Mohave, and the Southern Basin.

Basalts in this probably seem to like to pop out occasionally in places where you have to wonder why they are popping up there. Certainly their rates are much lower than these very active provinces.

And so, our challenge is to try to understand why these basalts are occurring where they do. I've given up tracing the petrogenesis models because they've changed so much in my 30 years or so of looking at them that I give up.

I think they are permissive and they don't tell you a lot. And people go back and forth on what they think is driving these things. But what you see, as Britt described, is fields like Lunar Crater, Cima, which are kind of big, high density volcanic fields.

But you also see phenomena where you have down to just individual separate cones, like the

Crater, or in Death Valley, where just one single cone can occur.

Crater Flat is interesting I think because you have to call it a volcanic field. There has been enough recurrence of events there. But, it's toward the low end of the spectrum of volcanic fields that you see in this whole province.

So that's fundamentally the conceptual model. I don't think anybody can say we understand why magma is either generated or comes up exactly where it does.

So, I'm going to focus a little bit more on what I think is a critical part of this part of the Great Basin that's unappreciated. And it's Basin Range.

But, toward the southwestern edge of the Basin Range there's a very strong overprint of what's been called the Walker Lane structure zone. And that overprint is an overprint of stripes of faulting.

And what you see with -- all the basins when we've looked at them in more detail, they show a component of stripes of faulting associated with extensions open the basins.

Crater flat has been proposed to be the stripes that -- we have new data at Frenchman Flat that suggest it's left step pulpar associated with this left step movement zone through here.

So, when you look at the structural controls of volcanism, you really should factor in this Walker Lane structural overprint that's basically overprinted on top of the basin range in the caldera models and the caldera cycles.

So, let's see. This doesn't show up very well, does it? What I did is I just borrowed this slide from one that I found in an NRC paper. I just wanted to show that what you're faced with, if you take a big zone is, how do you choose a record that is representative for doing your probability calculation?

And we wrestled with this for decades. Everybody has a slightly different opinion. And it's kind of fun to read to the PVHA because you see how each expert assembled them in a somewhat different way.

And I think the most important thing is not which one is right, but what's the range of answers that you get out of a sampling. I wanted to point out one thing right down in here that I think is important.

That's the formation and basalts at the green water range, because, at the end, I want to say a little bit about it. I think there's a big four million year event that you see associated with the opening of Death Valley or the one of the phases of the opening of Death Valley.

And it may have been responsible for this big event here. And I'm kind of wondering whether the Amargosa Valley record that we see is responding to that event.

There's a different tectonic event than what's going on in Yucca Mountain. Next slide. Okay, so here's how I put together the record that I think is relevant to the problem.

And it is a bit different from the NRC's approach. I basically -- if you look at this, there's a major phase of basaltic volcanism associated in the stage of the Timber Mountain and Oasis Valley caldera.

What you see is bi-model basalt roulades with a large volume of basalts. And then you also see another pulse of larger volume basalts when you look at the origin of each of these basins.

They opened up a fairly extensive -- as best we can time the extension. We can't time as well as we like. But it does appear that, associated with the opening of the basins, there were large volume basalts.

And these tend to be in the range of say nine to ten, maybe eleven or twelve million years in very -- basin to basin. What you see is they show up mostly in the subsurface, like at Frenchman Flat, Yucca Flat, Crater Flat.

And we now -- we drilled some holes in 9.5 caldera. What the typical thing that you see with these is these are big volume basalts. They tend to be one to ten cubic kilometers in volume.

And I think we're associated with this pulse of tectonics. What I think we now know is that that tectonism is weighing. We certainly know that extension rates are much lower.

Although, we're still debating those, but, what you see is, with the later stage basalts, is a switch-over to what I call small volume, post-caldera, post-extension basalts.

And they tend to have volumes in the order of about a tenth to a cubic kilometer. And this is the episode that I think is the most important thing to look at for Yucca Mountain.

It's the most current -- what I think is a current tectonic regime. Okay, next one. So, how would I assemble what I think is important? Again, here's a familiar map.

My argument that I use in my PVHA was, take a look at the volcanoes in Crater Flat. We also looked at the hidden cone, Thirsty basin units and the aeromagnetic anomalies in Crater Flat and Amargosa Valley.

But you have to be careful that some of those are probably associated with this older phased extension. We know from the dates that some of them are in the nine plus age range.

And, again, I mentioned that I don't think that the origin of the basalts are well known. There's been a constant debate over cause and effect between structure and the basalts themselves.

I think it's absolutely clear that local structure plays a role in the basalts. And whether that's simply that it's the guiding pathway for the last few kilometers or somehow that these waning tectonic systems can trigger episodes is a big debate that I don't think is going to be resolvable in the time of Yucca Mountain.

Okay, next. So, getting back to here, here's how I went to assembling this. What we found, one of the interesting thins in PVHA was Kevin Coppersmith was the person who led the elicitation.

He's a seismologist. And he kind of

guided us to think of a new way of starting. And seismologist, when they go to a problem, they come up with a seismic zone.

And they look for typical seismic characteristics of that zone and then apply recurrence rates for seisicity events to those recurrence zones.

And that having convinced us that that's probably the way we should be starting. Before we always did event counts. And then we looked at zones.

And then we tried to combine them. But, what we found out is, when you start with the zones, it does constrain you on how you use your recurrence event, because, depending on the structural definition of your zone, you may include or exclude some events.

And so, it's not fair to have a maximum recurrence rate but then apply it to a zone that isn't relevant to those recurrence rates. And I think Bruce is getting to the question that you are asking, that you want to bring as much geologic record and structural intuition into this problem that you can.

So, what I did was, I said, okay, let's start with zones and look at different ways to define zones. And then you also have to make some decisions, which are modeled assumptions about the distribution of events within those zones.

I think my next slide starts into that. Yes, what I did was, I said, okay, I'm going to take two approaches. One is I'm just going to say, let the geologic record be your guide and then see what the geologic record tells you.

And then the other was I said, I'm going to try to look at what I think are structural controls. So this is what I have that I call spatial models, which this represents in structural models.

So, what I started off with in the spatial models, I just said, okay, take the events and then draw areas around those events and see how those evolve through time.

And so, what you see if you just look at the record that I think is critical -- which is the last five million years, as Mike pointed out -- what you start off with is the oldest event is Thirsty Mesa at about 4.7.

And then you jump down. We did date this one anomaly in Amargosa Valley in 3.8. And you have a 3.7. So we see a northwest trending zone that you can then draw around these events.

It's going to change a little bit now when we see some more anomalies in there. But, basically, I would call this event on spatial zone. The one interesting next step is that, up toward Mesa up here, at three million years, it jumps up to the -- it's in the interior.

It's in the red tractor zone in the tephra mountain caldera. So, I would just draw the zone in that. And then you added the 1.1 million year event and then the Sleeping Butte that I think erupted around 300,000.

And then, finally, I can't forget Lathrop Wells. Lathrop Wells at 80,000 is then down here. So what you see that I think is kind of interesting is, if the space defined by the first couple of events kind of stays in there and doesn't get modified with the exception of one event out here.

So, what I did is I just said, okay, I'm going to use these spatial zones and I'm going to define my recurrence rates based on simply the spatial.

I'm making no structural interpretations. I'm just using the geologic record. Next one. The second step that I did I said, okay, I'm going to look at what I think are structural models.

And I had a range of structural models. I'm influenced by the Walker Lane that I first pointed out. You have that strong overprint in the Walker

Lane.

And you see, when you look at the patterns -- just looking at spatial patterns -- you see a northwest trend to the distribution of volcanoes, not the local trend, but the broader trend.

The local trend is following local structure. So, I came up with like a Walker Lane structure. And I had several different definitions, depending on whether I looked at the District Attorney record or the -- Attorney record.

And then I had a Crater Flat pull-apart model, which was both Pliocene and Quaternary. And then I included in different components of that the Walker -- I'm sorry, the Amargosa Valley.

So that changes there. I think I had seven or eight. And then I included Jean Smith's northeast trending zone. But notice, when you draw these zones, you are including an excluding events.

And so, again, you have to be careful to make sure that you sum your recurrence rates based on how you do your zones. Okay, next one. So what was really interesting with PVHA was, you know, I had done this for years.

In sitting down with a panel I was amazed with how many different ways people came up with different models. Mike can testify to that. We all -- almost every expert had a different model that he liked.

And each one seemed to have -- there was a spectrum of similar models. But each expert had one model that he would basically beat on the table and say, this is the right model.

All the other ones are wrong. And this comes back to -- well, what I wrote here. Many models are possible. There is limited data, so none can be disproved.

And nearly every expert had preferred models. So, why get into a debate over which model is right? Look at what the impacts of the alternative models are.

So here's just a diagram out of the PVHA which shows all kinds of different ways. Basically, here's Yucca Mountain. These boundaries represent different ways the experts drew their zones and then applied their spatial models to those zones. A wide range, it was impressive.

MR. HINZE: Could I ask a questions about that?

MR. CROWE: Sure.

MR. HINZE: What effect did topography

have on the -- we've been talking about here. What effect does topography have upon the decisions here?

MR. CROWE: That's a good question. I could tell you, in my model, topography had a major effect. I mean, basically, I agree with Bruce Marsh's assumption.

Our observation that you look at the set things in general. A few places they lap into bedrock. But, most of the places, the concentration is -- particularly Quaternary cone, let's say.

The Old Great Basin region tends to be an alluvial valley. And, if you go talk to the structural people, they say alluvial valleys is where the extension is occurring.

That's where the basalts are going to occur.

MR. HINZE: That's where the action is.

MR. CROWE: Right. And that's how would I use in my model. Now, we had different ways of doing that. What we ended up -- go back to that just one more time.

What we ended up with is what became really important was we had kind of a boundary. I think they drew this in the PVHA. And there was this raging debate over could things go in there.

The way I tried to resolve it, as I will point out later, was I said, okay, I'm going to locate my centers within my zone, but allow the dikes to extend out of there.

So, the dike lengths, the dike orientation will dictate whether or not they can result in a structural eruption. Okay, next one. So now here's just some interesting things I wanted to point out that I think the record tells you.

Again, I'm focusing on the younger ages here. And this is what I call the small volume, the point one to one cubic kilometer. And what I did here, because I fought with geochronologists for so many years, I hate to see histograms of ages where they are based on the number of ages.

What I did is I tied them to an age and an event. So, every place that I had an event and I knew the age. In some cases I had to guess the age. I called that one count.

And then I histogrammed this out. What you see is some interesting patterns there. There was a cluster of events in the seven to ten. These are the small volume events.

There was a hiatus here and then another cluster of three to five. And I think this represents this Amargosa to Death Valley event. And then another cluster just happens to have been the two really is a cluster of one and -- right here.

So, I think the record is showing you that there may have been three discrete pulses of activity possibly associated with pulse of extension.

And the question is, what's relevant to the future hazard. All the experts debated it. The majority of them used a five million years and younger.

Some only used one million years and younger. Some also included everything. But not many did it. Now, here's the second thing. What I did here was I just plotted the locations of these.

They are color coded in red as the younger group and blue is the older group. Then I just plotted an ellipsoid and the centroid of the distribution.

And what you see is there are two different spatial distributions. All the older ones occurred mostly toward the northeastern parts of the Nevada Test Site.

And then you see this centroid here located, not surprising, down in Crater Flat. And here's the anomalies of Yucca Mountain. So, to me, the record is telling you that there are some clear patterns here and you should incorporate those patterns in your probability models.

Next one. So here's a real interesting thing I did just before I left. And it has been buried in the paper I wrote, I doubt if anybody has even read.

What I did was I said, okay, let's look at an interesting exercise. Let's just go through the geologic record and let's say, where did each volcano occur and what's the sequence?

So, basically, these lines that I've drawn is I've covered both of these. I went to -- I started with the oldest events were up here. And then I just drew a line where the next event was.

And then I continued through that. Then I jumped down to here and repeated the process here. And what you see is this remarkable oscillation. And it tends to like a few spots.

But it oscillates back and forth. And, in fact, if you look at Crater Flat, the first event at is Thirsty Mesa, as I talked about. Then it jumps down to here.

And then it jumps up here. Then it comes back down here and goes up here and comes back down there. To my mind, there's a lack of predictability there.

Well, let me rephrase that. There's a tendency for things to cluster in groups, here and here. But, when you look at it in detail, the last event is a poor predictor of the next event.

It looks like magma will come up wherever it feels like coming up. You have to be very cautious. That's why I went to a random model. I just felt like we just don't have enough information to really say, why is it coming up where it is?

And so, what I did from my zones, I'd see this is a random distribution of event. But let me point out that there are two scales of clusters. And I worked with some spatial experts to look at this.

There are the clusters where, when you have an individual event -- let's take the 1.1 million year -- that clusters as a group of four things.

But that's clustering like one event that forms in probably a fairly narrow period of time. As best we can tell, it's largely synchronism. I gave up arguing with the geochronologist of whether there's any differences there.

But, the best we can date, we don't see any difference. Now, that's what I call an event cluster. And then there's a spatial cluster, which I think is just where you see patterns through time.

So, I distinguish those too. Okay. So now let's come back here. So what I did is I did my two types of zones and then I used a random distribution of events.

So now we come down to event counts. And anybody who had been around Yucca Mountain knows that this got debated for so many years and there have been so many different models that I got tired of even talking about them.

But, here's the parameter. You have to come up with an event definition. And Britt gave you one event definition. And that's basically -- it's a model assumption of how you chose your events.

And what I think is important is to make sure each expert defines that, because you can end up kind of muddying the waters using different event definitions and come up with recurrence rates that are variable and are confused because you haven't clarified your event definition.

You have to choose a time interval. I covered that. I'll talk a little about time distribution. But we argued that one -- as I understand it, I think both the DOE and NRC agree on a steady state of vent rate that they have been using for their probability models.

And then this undetected events I'll talk about in a little bit. But, those are the parameters you have to come up with when you do your event counts.

And here's one. I borrowed this one out of one of the things that the ACNW sent out because I thought it was really neat. The vent areas show up in red in this particular spectrum.

And what you can see is, if you take like phases of volcanism, they have a discrete event geometry to them. And it ranges. Britt described in some detail the 3.7, what you see.

When I originally mapped it I thought there was about fiver or six centers so that I could reconstruct. So, we had a cluster of five or six centers.

Now, are there five or six events there? Or is that one event that has an event geometry that's spread over an interval here. What's important really is that you can look at it almost both ways.

But, they have different consequences. So, if you're going to assign the maximum consequences, which would be a large event, you have to go back to the recurrence rate and treat it as a single event.

So, you can't over count it and then come back and weight it. So, what would happen is the consequences go up but the recurrence probability goes down.

So, if you look at the record, what you see is that there was about five here. We think there's about four here. We've got long debates about what counts as one or two.

And I don't think it's worth arguing over. Thirsty Mesa up there, I think I mapped three distinct event. Sleeping Butte has two way up here.

Lathrop Wells and the Mesa up here, we're just thinking they had one single event. So what you see is you have -- the record is telling you there's a spectrum of behaviors.

And I think you should just treat it probabilistically as probably as a uniform from one to six. And that's a nice way you can treat how you do your events.

But you have to be very careful to make sure that how you do your events is tied to the consequences. And then I mentioned that you have to do your event counts specific to the zones.

And, I mean, I made the mistake in my first calculations. I treated them independently. And you end up coming up with combinations that have no possibility, because they don't exist in the geology realty space.

Undetected events, to the best of my memory, Bill, to answer your question, was, the way we handled it in PVHA was most of the experts thought that if magma is going to ascend all the way up to repository depths of about 300 meters, it's going to make it to the surface.

So they felt like it's going to be unlikely to have an event that comes up into the shallow crust and just stops, that you're in the depth range.

We're starting to -- volatile. It should be the driving force that's going to push it to an eruption. But they felt that there could be an event geometry of more undetected events with that.

So say at Lathrop Wells there might have been some intrusions to the southeast of it. So they were adding -- that's what they call undetected events.

And that's different from having an event that came up and never reached the surface and created something in the volcanic record. So, you know, I completely agree with how Britt was describing that there.

You have to be careful on how the different -- PVHA looked at undetected event associated with known surface volcanoes. And there was a fundamental dispute over whether or not you could have an intrusion pausing in the very shallow crust.

Okay, now here's the -- these diagram I hate putting up because I get in trouble every time I talked about them. But, let me start with a simple one first.

If you look at -- this is just cumulative volume versus the time. What you see is a four million years event where larger volume has inversed slope.

And then the younger events have a different slope. And I think these are probably telling you that fundamentally they are different parts of the record, that they're probably responding to, I think, different tectonic regimes.

And, you aught to make some choices about which one you think is the most relevant to the future. The second one was -- this is kind of an exotic plot that I labeled.

What I calculated was, I took the time from the previous event, which I called the reposed interval -- so, in other words, like this is between Thirsty Mesa and I think the anomaly in Crater Flat.

That would represent this reposed interval. And then I plotted that versus time. First I fit a nice little linear regression. So what you see is a slight tendency to a decrease in that reposed rating.

Again, your dataset is pretty limited. Just for fun, I did a distance weighted square fit which shows an oscillation. When I put this in a paper, a reviewer said, oh, he just predicted the next eruption is going to happen any time now.

And, I don't know if I'd go that far. But, we used to have negative ages on Lathrop Wells. And we used to argue, there's your next event. Okay, next slide.

So, okay, coming back to -- then we summed all these event counts in different ways. And I did it for spatial and structural models. That then feeds into the recurrence rate.

And then that recurrence rate goes into the probability of repository intersection. And let me show you the way that I've ended up kind of liking to do it.

But there's a whole bunch of different ways to do this. Let me point out the variables that go with this repository area, the dike ledge, which you can treat as stochastic, and dike orientation, and then the probability of an eruption are an intrusion.

So, the next one. Okay, this again is just a reminder. This is how I assigned these to my individual structural zones. So what I said is I allowed these to have a random distribution of events within each of these zones.

Then go to the next one. Then I worked with Goulder and we used the code. And we run simulations where we assign the dike height, a dike length, and a dike orientation.

We just did simulations of the repository block that's buried down under here. And we just let them run. This one happens to be for the Yucca Mountain region.

And, because we find that the outer domain of our models, we put a lot of dikes in this one to extend past the model domain. But, in the other ones, we just basically gave a dike dimension, randomly located them within that.

And then we summed up three things -- two things, the number of intersections in the volume intersection, and then calculate that as our disruption probability.

It's very comparable to the way most experts did it in the PVHA where a geomatrix helped them use kind of a geometry of intersection. They treated dike length as a stochastic.

They treated dike orientation as stochastic. And they ended up -- you have a trajectory of only certain areas will actually project into a disruption.

So they brought that geometry of dike directions into an intersection. So, if you go back -- so, if you go to different centers, some of them are capable of a repository intersection, some are not.

So, disruption ratio becomes -- we basically are very influenced by the modern stress field, which says that dikes should be entering in a north-northeast direction, basically.

And the stochastic was centered about that. So, when you locate your events, you assign that to it. So, some events are going to occur in that zone.

But they have a virtually zero probability of intersecting the repository because either the orientation of the dike length takes them out of the ability to intersect.

So, okay, that's what I did. I want to say just a little bit about -- I did this right at the last minute before I left Yucca Mountain. I think it's also been buried and nobody has read it.

It's something that I didn't do until after I finished my probability calculation. I came up with a simple logic that says, I think there's some somewhat firm bounds you can put on this probability of repository disruption.

Here's the argument I went through. In the basin and range there is a background recurrence rate. Basalts tend to keep coming up. And so, I said, well, if you located a repository away from a defined volcanic zone or in this background, you should calculate the probability of it being in a background setting.

And, that's what I did. My particular -- well, I'll get to that in a second. And then I said, the other -- so that would define your minimum value for your probability.

So, in other words, the distribution shouldn't get less than background, or maybe you have to go back and question your assumptions. And then, on the other end what I said is, let's just take the repository and put it right in the middle of one of these zones that I defined.

And that should give you the maximum at the other end. It says, we think Yucca Mountain -- this is open to great debate, of course -- sits outside of the volcanic field, but close to it.

So, logically, Yucca Mountain -- the probability of disruption should be greater than before but less than putting it right in an active volcanic zone.

And this becomes the big debate. How far away from a volcanic zone is Yucca Mountain. And I don't think that's resolvable. So, okay, let's see what happens if you make those assumptions, what you come up with.

I use the Southern Great Basin. And I use this thing that was very popular during the PVHA called the Amargosa Valley Isotopic Province, or AVIP.

It's an area where there's a unique isotopic composition to most of the basalts. I'll let Frank talk about that. I'd like to stay out of that area.

But, basically, the AVIP defined this area of unique isotopic compositions of basalt. And so, what I did is I said, okay, let's take a 4.5 kilometer repository footprint, put it into these two provinces.

And I used event counts from the combination of expert judged in my own regional field studies where there wasn't any data. And I used that -- I used the recurrence rate and then the disruption ratio was simply the ratio of the area to the repository area.

And here's what you come up with numbers of what I would call background. Somewhere down in the low one to three times ten to the minus nine. So, what I would say is, anybody that calculates a number less than that, you should question how you assembled your probability calculations.

So, let's go to the next one. So then here's what I did if I plugged into my zones. And the numbers range from almost two times ten to the minus seven.

Two is low for this at the Jean Smith Northeast structural zone. And it's interesting for this one because there is a restricted number of events that that encircles.

The recurrence rate goes down. And so, the probability of disruption is lower. So, actually, this is the zone that includes Yucca Mountain.

And yet it has the lowest of the calculated. So, somewhere in this range, you would argue -- and I put the number up around one to one point two times ten to the minus seven would be a maximum bound.

So, what I would argue is, if you're getting much higher than that, you basically aren't paying attention to the geologic record and you should look at your probability calculations.

So, let's go to the next one. So, here's what I did. I love this phrase that basically you have to cut off the maximum, which is the uniform distribution between your min and the max.

Basically I'd like an uninformed prior is they way I like to look at it. So, my uniformed prior was the min and the max I calculated. So, I used on times ten the minus nine and one times ten to the minus seven.

That gives you a mean value of about five times ten to the minus eight. And, interestingly enough, our numbers -- everybody's numbers comes around pretty close to that.

I mean, in my opinion, some of the fights I've been in and I think are still occurring are you're just modeling noise around some numbers. It's probably unresolveable.

So, why not just kind of look at it that way. So, I went back and I looked at the PVHA disruption, which is here. So, this is ten to the minus ten, ten to the minus seven.

What I argue is they have a fair amount of detail that goes down below the ten to the minus nine range. So I'd argue that we probably should have truncated that and said that those are just a little bit too low.

And so, what you do is you reduce some of this huing on this distribution. You probably shift the mean a little bit over here. And then, going to the NRC model, they've been talking about a ten to the minus seven, ten to the minus eight for most of the data they interpret.

And I would just argue that, instead of using ten to the minus seven value -- which they do in their PA calculation -- treat that as a uniform and sample that distribution.

If you do that, the difference between this uniform and about there is not enough to get excited over. And I would argue it's getting time to move on to consequences, where all the uncertainty is.

So, next one. So, the final overview comments, I just want to comment a little bit about where I was when I thought I left with the aeromagnetic anomalies.

I haven't looked at the new data. So, it would be very interesting to look at it. As I mentioned, we did drill the one anomaly. And the fact that -- as Britt pointed out -- these anomalies are buried.

If there was surface basalt at the centers, they have to be fairly old. He used two. I would argue that I bet they are going to come out around four, because that's the one that we drilled, at about four.

And it also matches a regional Death Valley event that I think you see as an overprint in this region. So, if these things are about four, and they're mostly located down in the Amargosa Valley, the dike lanes and the dike orientations are not going to lead them any intersections.

So, you don't want to just look at the recurrence rate. You want to look at both the recurrence rate and the likely hood of an intersection with these new events.

I don't think that it's going to change

the relationships as much as people have been saying in a new era. There's going to be a range of change.

But, when you take into effect the recurrence rate and the likelihood of disruption, I don't think the numbers are going to change that much.

Here's the only thing -- in fact, before I left -- go to my current program with the DOE to do this. This is the anomaly near little cone. It has a normal polarity, which doesn't match anything we see in the record.

Everything else is reversed out there. We need to find out what that is. Because, if it is something in the record that we don't know of, then we really need that data.

And I had also argued that let's explore some of the anomalies in Crater Flat that are close to Yucca Mountain that might have a higher potential intersection.

And that should influence -- I mean, those are just so important. And my opinion is it's probably so important that you really should gather data on those.

We have the potential, so let's just go gather it. But, I would argue that, for Amargosa Valley, drill one or two of them. But, if they all come out at about the four million range, I think I'd walk away and feel pretty confident that you know what you're doing.

MR. HINZE: Before you leave that, if I might, the limited impact, is that based upon an assumption about where these aeromagnetic anomalies will be found? Could you expand on that a little bit?

MR. CROWE: Yes. It was based mostly on what I saw in '96 with the aeromag data, which was mostly Amargosa Valley. They have some new data in Crater Flat that I'd want to look at.

So, I should cavy out that. That's a '96 profile that I'm presenting. But, if you looked at what Britt was presenting, most of the anomalies are down in Amargosa Valley.

I'm guessing that a lot of the ones in Crater Flat are probably very tough, since it is so magnetic. You can fault it and get a pretty good signal.

MR. HINZE: How about in Jackass Flats?

MR. CROWE: I'm biased. But, I looked at Jackass and I was doing some work. There actually is a drill hole that penetrated the south in Jackass.

Way back in the nuclear rocket program in the 60's they drilled three holes, J11, J12, and J13.

And one of them hit a basalt at, I think about 1,100 feet.

I looked at the cuttings from it, and I think it matches what we call a basalt of EMAD, which you see at the surface, which we date at about 11 million years.

I walked the sections and looked at -- we have dates through all the basalts surrounding that valley. And they're all in the nine to 11 million years.

I think it's unlikely you're going to see a shallow anomaly there. But I want to see the high resolution data to see if anything shows up. But, I don't think I would get really excited about it.

The record seems to show that not much has been happening in Jackass Flat. Let's see, where was I? Okay. Here's on last thing I wanted to point out.

I really think that the Crater Flat pull-apart is where the active extension is. And the record is telling you that that's where the basalts are coming up.

And that's the major part of the record we should be looking at. And I think it's the critical thing to calculating future probability. I think people have neglected this.

I worked with Will Carr here originally. And he always pointed out what he called the Minot Spotted range system, which is a series of -- left slip faults.

And we now know that one of those control the extension of Frenchman Flat. What's really interesting is, in this basin -- I guess I would argue by inference the Jackass Flat Basin -- you probably have strike -- components to them in this left slip.

And most of the basalts that you see occurred primarily at the time of extension, as best we can tell. And what you see is fairly large volume basalts.

We know -- we have penetrated basalts in Frenchman Flat. In multiple cases, the -- testing dated down -- the maximum plug buried up in the bedrock to the west, dated five.

And, they are voluminous enough that they look like they probably are marking the major extensions. Similar arguments could be made for what's in Yucca Flat.

In fact, I now think going -- this bedrock that we dated 86 here is probably part of this extension of that basin. What's kind of interesting is most of the basins except Crater Flat and Frenchman

Flat have one major phase of basaltic volcanism associated with extension.

But, in Frenchman, there was the later stage of about 7.2 million years later. What's interesting is that this has been a persistent site of volcanism.

I think there's a little bit of anomalous for all the other basins here in that, not only was the -- older stuff that floors the basin -- we penetrated 11.5 million year basalt at 1,100 feet below the surface here.

We see it in the south exposed to the surface. But then there are these multiple pulses of younger. And that's where Crater Flat is a little bit unusual.

I personally think it may be a combination of Amargosa and Crater Flat -- is in the intersection of this spotted range Minot Mountain system.

And it has been influenced by a part here, and possibly might be influenced by the proximity to Death Valley. But, that's very speculative. And I'm just going there because I can get away with it because I don't go to the program.

CHAIRMAN RYAN: Okay, we have time for a few questions. Any questions? Yes?

MR. HORNBERGER: Bruce, you mentioned that when you did this you had a bound, and you said that less than ten to the minus nine is not credible. And you didn't think that higher than ten to the minus seven was credible.

MR. CROWE: Yes. I would go into maybe three times ten to the minus seven range, but somewhere in there. You might have to -- you might take all the expert judgment and assemble them to see how you are bound to compare. I just did my set of models.

MR. HORNBERGER: Right. I realize that. Can you think of any way consistent with your knowledge of the geologic system that you could say get to five times ten to the minus six?

MR. CROWE: No, I can't. I mean, you'd have to have some preferential mechanism for focusing events at Yucca Mountain. I think the geologic record says.

Since you can go back ten million years, there is that one Solitario Canyon event. But, I think that's associated with the maximum extension. If you go back and look at the ash record of Yucca Mountain, it was a basin when the eruptions occurred that formed most of the mass of Yucca Mountain.

And it was elevated between the two sheets of Timber Mountain. You can see this huge -- forming in the geologic record there. So, most of the tectonism that elevated that mountain, occurred about 11 million years ago.

And Yucca Mountain de-coupled from Crater Flat in my opinion at that point. If it didn't, it would still be a basin. But, it's a high standing range.

And, again, I believe the model that extension in the record all over the Great Basin shows that, with seismicity, that where the extension is occurring the valleys.

And that's where the basalts tend to occur. But they can spread a little bit. That's not to say it excludes penetration. But I would say our best guess from the record is in the valleys of where all the action is.

MR. HINZE: Even including the 10 million year old events, you still fall within the ten to the minus seven, ten to the minus eight?

MR. CROWE: You do, exactly right. Yes, I mean, I really have -- the only plea I would like to make is get on to consequences. I mean, that's where your uncertainty is.

And you're going to just be fine-tuning here. I mean, I really think you should drill these anomalies. I mean, I would like to see the new dataset.

But, the expectations are that it's not going to change it too much. And, if you look at your bucket of uncertainty, the consequences are so much more significant.

CHAIRMAN RYAN: Thank you very much Bruce. That was an interesting talk. Any last questions?

(No response.)

CHAIRMAN RYAN: All right, we'll press onto our next speaker. Mr. Neil Coleman of the ACNW staff will be talking about alternative views on the likelihood of an igneous event in the Yucca Mountain region.

And, while Neil is getting ready, let me recognize Dr. Charles is in the audience, a member of the ACNW. Thank you for your participation, for being with us.

ALTERNATIVE VIEWS ON THE LIKELIHOOD OF AN IGNEOUS EVENT IN THE YUCCA MOUNTAIN REGION

MR. COLEMAN: This talk represents background research in support of the ACNW's review of volcanism. I thank my co-authors, Bruce Marsh of

Johns Hopkins University in Baltimore, and Lee Abramson of NRC's office of Research.

I thank them for their contributions. Thanks to John Trapp of the Staff for providing NRC's PVHA code and -- Center for Nuclear Waste Regulatory Analyses.

PVHA stands for Probabilistic Volcanic Hazard Assessment. I should add at this point, this talk represents our views, the author's views, but does not necessarily represent vies of the Commission, NRC Staff, or the ACNW.

We suggest that our work be considered in evaluations of volcanism at Yucca Mountain. I will briefly describe the technical issues for volcanism and provide a brief summary of volcanism in the region.

Previous estimates of the probability of volcanism will be discussed. And I will show the results of our statistical and PVHA analyses. And we will compare Yucca Mountain to other volcanic fields.

Finally, I will present conclusions and recommendations. Next slide, please. A special topic in the earth sciences is using geologic data to evaluate very low probability events such as volcanic eruptions and earthquakes, and evaluating how these could potentially have significant consequences.

Now, of course, the technical issue here is the potential for inter-igneous activity very much like the repository. Here we are looking south from Yucca Mountain.

And, in fact, some of us from the Staff were on the crest of Yucca Mountain and had that exact view just yesterday. You can see the 80,000 year old Lathrop Wells cone in the distance.

Geologically, this is the youngest known volcanic event in the Yucca Mountain region. Next slide. On the left is a pan view of the underground repository.

On the right is a close-up of the waste placement drift showing the potential horizontal storage of alloy 22 waste packages. If the probability of an igneous dike intersecting the repository is less than one times ten to the minus eight per year, it may not be considered in licensing.

However, regional studies do suggest that the probability is just high enough that the Department of Energy must evaluate the consequences of dike intrusion.

Potential consequences will be discussed in the next session of this working group. Next slide. I want to take a moment to just put geologic time in perspective.

We toss these terms around, Quaternary, Pliocene, Miocene. Here's a timeline that compares volcanism in the Yucca Mountain region to other events.

This figure shows the last two million years. The tuffs that form the surface of the mountain are quite a bit older. They erupted between ten and 13 million years ago.

So they are off the left end of this chart. Not all the basaltic events in the region are shown. Here are some examples. The X axis here is in millions of years before present.

The last 1.8 million years represents the Quaternary. You can see the -- if I can find the button here -- the time frame on the bottom. 1.8 million years is the break between Pliocene and Quaternary.

And there's a Miocene-Pliocene boundary of 5.3 million. Older events are Miocene in age. Approximately 11 ice ages appear since the late Pliocene time.

Only once volcanic event at Lathrop Wells cone has erupted since the advent of modern humans on earth, that's the *Homo sapiens sapiens*. That was about 120,000 years ago.

The million year old cones in Crater Flat pre-date all the pieces of the *Homo sapiens,* including the Neanderthal. The famous hominid fossil Lucy, right here, (Australopithecus aphaeresis), dates back to the Pliocene time around the time that those guys were occurring in Crater Flat, the Pliocene.

At the far left is the Solitario Canyon dike that was mentioned, around 10 to 12 million years. There are two dates for that one. The key thing to point out at the top of the figure is that the uncertainty in the actual number of volcanic events greatly increases as you go back in time, because you had more time to erode basaltic events that occurred then.

Also, you had more time to cover them up with younger volcanic, like sevens. Next slide, please. The large surface exposures in the region outside of the basin data is a tuff produced between nine and 13 million years ago, the huge caldera formed eruptions, pyroplastic eruptions, some of it.

The largest pyroplastic eruptions that we know of anywhere. You see a series of these overlapping calderas north of the blue star, Yucca

Mountain.

Calderas are large areas of collapsed terrain that form during and after large volcanic eruptions. There are extensive Miocene and Pliocene basalts that erupted in and near these calderas, which represent kind of a unique structural.

Next slide. Dr. Crowe showed this slide. I'll just mention the repository shown in blue here. The black areas here are Pleistocene basalts. There are eight of them, including two up in the upper left hand corner, that Black Mountain vicinity.

Of course on the sort of black pattern sort of classing basalts, and the grades in the Miocene basalts, which occurred all over this area. After Miocene time, volcanism clustered to the west and south of Yucca Mountain.

There are no known Pleistocene or Pliocene basalts on Yucca Mountain or to the east in Jackass Flats. Next slide. Here is a satellite image. I think John Trapp showed this one also.

The Yucca Mountain site is, again, in the blue star location. The DOE has conducted aeromagnetic surveys. And we saw some initial results from that in an appendix.

They have plans to drill and date a number of suspected buried basalts. The latest drilling results show that the basalt penetrated at Nye 23P, they didn't encounter basalt.

What was sort of interesting, the impression that the DOE contractors had is that this may not be basalt, it may be a boulder zone that was penetrated.

But, I would suspect that if these were large boulders, they wouldn't come from very far. So that probably does represent an insidious basalt somewhere here nearby.

But the key is that this is not particularly surprising to find this. There is no magnetic anomaly associated with it. It is very deep, 400 feet deep in alluvium.

And the age that has been determined, the Miocene age is consistent with the ages of other basalts in Jackass Flats. Next slide. There have been approximately four known pulses of basaltic volcanism in the area.

And this is a different way of showing what Dr. Crowe showed with changes in the estimated magma volume over time. What you're seeing is volumes of magma erupted in cubic kilometers that are on the bottom scale.

The X axis is a million years before the present. The vertical axis is volume and cubic kilometers. The large bar, A, represents the Miocene eruptions, B, the Pliocene events, C and D the Pleistocene events.

The tiny bar under D is the Lathrop Wells cone. This figure shows the volume of volcanism was basaltic and the -- were constant. Support in an uncertainty increases a lot as we go back in time right to about the big bar A.

It is most certainly too small, because those Miocene results were probably buried by younger basalts and alluvium in Crater Flat. Likewise, the Pliocene events in B may similarly be too small.

The magnetic data that we saw yesterday gave a preliminary look -- shows that is indeed the case. The Pleistocene volumes shown by C and D are much more reliable because little time was available to erode or conceal those deposits.

Next slide. Here are estimates for volcanic disruption of a repository, some of which claim the probability could be much higher than previously thought -- ten to the minus six per year or higher.

That is on average one penetration of the repository per million years. I am aware of these reports and believe that each represents an earnest effort for the authors to come up with reasonable estimates.

Now, the rest of this talk considers some simple tests of whether the highest probabilities are realistic. We look at past volcanic -- four time scales, the 13 million span, the total length of time that the surface rocks have existed at Yucca Mountain.

One million years is the to the last four million years. 100,00 years, and then some inferences about present day conditions. We'll look at present day.

One impetus for a higher probability would be unusual crustal activity. In 1998 Brian Wernicke, et al reported in the Journal of Science that Yucca Mountain has tried to pull apart.

This claim is countered by Savage, et al in 1999 and in 2001 papers in the Journal for Geophysical Research. They used a larger GPS network to show that the extension rate is not anomalously high for this region.

And, therefore, present day strain rates do not indicate conditions favorable for the infinite triggering of volcanism. Next slide. The rocks that make up Yucca Mountain record an integrated tectonic volcanic history since the 13 million year old tuff in the repository.

Yucca Mountain is one of the most intensively studied places on earth. Over 20 years of studies have included detailed surface and sub-surface mapping, geophysical surveys and construction more than ten parameters of tunnels.

DOE drilled more than 450 surfaced bore holes -- depths. It seems unlikely that multiple dikes could exist in the repository footprint and escape detection.

We examined whether dike penetration rate was greater than two times ten to the minus seven per year are realistic given that no dikes have been found in or above the 13 million years old repository block.

Now, it was mentioned earlier that there is one event, a dike 10 to 12 million years old, that was a near miss. And you can see it. There we go, just to the west of the site and located within the Solitario Canyon.

You can see the expression of fault in the topography in this area. And here is a north-west extension of it as well. Although it is close, it is a near miss.

As far as we can tell, it did not penetrate the repository block. And, DOE does use certain criteria for set-back from faults for tectonic reasons, for earthquake reasons.

I should mention that, because of the age of this unit, we know that in true to these upper faults, during the ancient period of caldera formation, when basaltic volcanism was very widespread.

And, caldera formation had not ceased at the time that this dike was in place. There was still activity to the Northwest, Thirsty Canyon tuffs are younger than this unit.

The image on the right shows this Miocene dike is very close to the site. Exposures are small. The whole thing is maybe about 10 to 15 in length. It's about a meter across, less than one meter thick.

And it is highly eroded. What you see is most of what is there. It is possible that other features like this exist but have been undetected on the mountain.

Geophysical methods would be poor tools for finding dikes like this. And, in fact, that was presented in the appendix 7 yesterday, that low altitude magnetometer passes over this dike did not detect it.

This is the last high resolution work that was done. Although, that was a very preliminary result, and there were numerous other passes. But, yet, it is an extremely -- dike.

It was found in the geologic method. And the geologic method is the best tool. And the emphasis that was placed on mapping out fault traces maximize the possibility of finding this kind of feature.

Of course, the best way to locate any kind of dikes in the mountain, is in the underground tunnels. They have been mapped in great detail. No dikes have been found in them more than 10 kilometers of tunnels.

I should also mention that, on this trip when the photograph was taken, an NRC hydrologist was the one who located this, an individual with almost no mapping experience.

So, next slide. We could use the apparent absence of basaltic dikes to detect -- in the intrusion probability. Assuming a constant recurrence probability rate, the number of penetrating dikes in time, T, has a Poisson distribution with a mean of ëT.

The probability of no penetrations is the exponential of minus ëT for two times ten to the minus seven per year. The expected number of penetrating dikes is 2.6.

The probability of at least one penetration is .93. For a recurrence probability of one times ten to the minus six per year, that is a very high intersection probability claim, the expected number of dikes would be 13 and the probability of at least one penetration, as you can see, 0.999998.

These results are not consistent with the exploration evidence because no dikes have been found in the footprint. Claims of high intrusion probability failed as test over the 13 million years time scale.

Next slide. Let's look at some younger basalts. One the left is a vent complex in Pliocene H in Crater Flat. At right is Black Cone, which is a Pleistocene volcano dated around one million years.

And this series of cones -- Northern Cone, Black Cone, Red Black, Blue Cones, these are all dated around one million years. No features like these exist on Yucca Mountain.

An important point to make is that preservation of exposed basalts in southern Nevada depends on their age and topographic setting. Miocene and Pliocene basalts have been found in local basins buried by alluvial basins.

Partial burial has been reported for Pleistocene basalts. But they are too young to be completely buried, even in basins. Next slide. Now, you've also seen this slide before.

To further analyze the probability of volcanism intersection we require NRC's PVHA code, version two. And we analyzed the ten datasets that have been published with that code.

Here's an example graphic from Connor et al., 2000 in the Journal of Geophysical Research. This slide shows the spatial recurrence rate contoured for the Yucca Mountain region.

It's based on event cluster modeled that uses a kernel function. It has built in either the use of Gaussian or Epanechnikov code kernel function that produce similar results.

It's also based on locations of Quaternary volcanism for this particular case and information about the density of the earth's upper crust. To learn more about the code, I would refer you to that JGR paper in 2000, also to a report by CNWRA by Laura Connor et al., 2002.

Next slide. This slide summarizes our

results using all ten datasets. And they are described briefly in the left-hand column. These datasets represent various patterns and ages of volcanism.

The top file, all 64 events, you can see that in the back, covers a region that includes parts of Death Valley. It also includes some magnetic anomalies that are assumed to be buried volcanoes.

The bottom dataset includes just the eight known Pleistocene events. Eight of the datasets will include five to 15 magnetic anomalies that are assumed, generally without proof, to be volcanoes.

This makes for a robust analysis. This incorporates a lot of uncertainty about the possibility for buried volcanoes. For each dataset we evaluated the recurrence rates in the Yucca Mountain region that were required to produce repository intersection rates of ten to the minus eight, ten to the minus seven, ten to the minus six per year.

As shown in the far right column, a mean rate of ten to the minus six per year prevailed in the last billion years, 42 to 96 volcanoes would have erupted in the Yucca Mountain region.

In reality, only eight events occurred during all the Pleistocene, which is 1.0 million years long. That's a recurrence rate of just 4.4 per million years.

Now, if we divide these numbers by ten to reduce a time scale to the last 100,000 years, at ten to the minus six per years, the expected number of volcanoes is four to nine.

But there's only one, Lathrop Wells, event. We can see that claims of high probability of intersection, fatal tests of volcanic recurrence, and time scales with million years and 100,000 years.

Now, something more should be said about this because PVHA results shown here are based on a Gaussian model modified to include crustal density effects.

And you heard the discussion about that. This approximately doubles the dike intrusion probability at Yucca Mountain. However, gravity weighting isn't limited.

The number shown here would double, which is an extraordinary number of volcanoes. And, in fact, we do recommend not using this weighting factor of several reasons.

It is highly subjective. No basis has been demonstrated for including it. Also, the kernel estimator has already quantified the degree of clustering of the volcanoes.

The crustal density information simply provides a partial geologic explanation as to why the clustering occurs where it does. Finally, it should be said that the seismic tectonic regime represented by the density map, based on gravity, probably reflects the much higher extension and volcanism rates, both Miocene time and into part of the Pliocene time.

Present day extension rates are significantly lower. In other words, the primary effects of the lower crustal density probably manifested themselves long ago when the density contrast was created.

The decline in volcanism over time supports this interpretation. Next slide. The very large recurrence intervals in the previous slides in 40 to 96 volcanoes per million years or four to nine in 100,000 years or 80 to 192 in the last million years without gravity weighting.

And perhaps the answer lays somewhere between the sets of numbers. Let's look at other volcanic codes that have this level of activity. And the source of the slide is Chuck Connor, University of South Florida.

Also, one of the developers of the NRC PVHA code. If ten to the minus six per year were true at Yucca Mountain, then Crater Flat would be as active as many of the volcanic fields in this table, approaching one times ten to the minus four events per year.

The volcanic field at Cima, California, falls in this branch. Next slide. Cima is located south of Las Vegas. Here's Las Vegas Valley. Here's the location of the Cima field, to the south.

This volcanic range has more than 50 events and approximately 65 or more blows, covering an area above 150 square miles. Next slide. Here we have three panoramic views.

Crater Flat is at the top. And then there are two views of the Cima field. About 30 of the cones at Cima are Pleistocene in age, which means less than 1.8 million years old.

Yucca Mountain and Crater Flat have not experienced anything like this level of activity. If they had, probably the best view for you to look at to compare Cima is the bottom view, the widest panoramic view.

You can see that the horizon is covered with cones. You simply to not see -- your eye tells you this level of activity has not existed in Quaternary time.

But, if it had, what should Yucca Mountain look like? Next slide. This is a projection taking roughly 35 to 40 events and placing them approximately where they would arise.

A very high rate of volcanism had occurred. You will see that there was, in this case, a hypothetical impact at Yucca Mountain just once per million years.

But what, in fact, do we actually see? Next slide, please. Back to this figure. There were eight events in Quaternary time. Only six of which are here.

If you flip back and forth between those two, just for a second, it is a dramatic difference. I would say, where are all the volcanoes that should exist if these very high rates had prevailed through the last million years?

In arid to semi-arid climate of southern Nevada is very hard to obliterate the evidence of these very young volcanoes in Quaternary time. Next slide.

What agree with comments made in the paper Connor, et al. in JGR. Rates of basaltic volcanism comparable to those in Cima or also seen the Colorado Plateau volcanic fields, approximately 30 volcanoes per million years have not occurred in the Pliocene and Quaternary in the Yucca Mountain region.

And it is reasonable that the probability estimates we calculate for the volcanic eruptions be substantially less than those estimated for the larger, more active fields.

Next slide, a recommendation. We would recommend using the Quaternary recurrence rate to estimate the frequency of repository intersections. This has three advantages.

We are, of course, still in the Quaternary period now. Compared to Pliocene time and certainly compared to the Miocene time, the Quaternary best represents the present day seismo-tectonic regime.

Also, the Quaternary fully captures the most recent volcanism cluster of one million years. This cluster represents five events or less. But we consider also the maximum number that is somewhat conservative.

The biggest advantage, it is a more reliable recurrence rate. The uncertainty about the number of Quaternary events is greatly diminished compared to Pliocene events, certainly compared to

Miocene.

There has been insufficient time to erode or bury Pleistocene basalts. Next slide. Our estimate from this review, we use the PVHA code and the dataset of eight Quaternary events.

A Pleistocene recurrence rate -- that's 4.4 events per million years -- and zero gravity rating. We estimate the intersection frequency at five point four times ten to the minus eight per year.

Since the result is based on eight events, you can get upper confidence bound, in this case 95 percent, using the Poisson distribution. Upper bound is approximately one times ten to the minus seven per year.

Next slide. Conclusions -- our analysis raises doubts that a potential repository could be penetrated by a dike once every million years. We evaluated four time scales, as discussed.

And, at the 13 million year scale, non-detection of basalts suggests an upper-bound penetration rate of two times ten to the minus seven per year, on an average over 13 million years.

At the one million year time scale, using the PVHA code, it suggests 40 to 96 events to have erupted in the region in the last million years.

Without gravity weighting, that number goes up to 80 to 192.

But, only 80 events are known of all the Pleistocene. Next slide. The results are especially interesting for the 100,000 year time scale.

We contest a hypothesis that was discussed by the expert elicitation in 1996. Is it possible that the 80,000 year old Lathrop Wells cone was the start of a new pulse of volcanism.

For a dike penetration rate of ten to the minus six per year, the PVHA results indicate four to nine events would have been expected in the last 100,000 years.

Without gravity weighting, we do dispute the degree to which gravity -- you would expect eight to 18 events. Only one is known. Our best estimate for dike intrusion is more than ten times smaller than the highest probability claims.

The future volcanism follows the Pleistocene pattern. The probability of intersection is 5.4 times ten to the minus eight per year using the PVHA code.

Claims of greatly increased probability -- failed the simple test of reasonableness of four times scales. Spatial temporal models predicting intrusion probabilities greater than two times ten to the minus seven per year in the potential repository footprint are overly conservative.

Along with ongoing work by the Department of Energy, ongoing site investigation, our realistic models will be developed by considering non-detection of basaltic dikes in the potential footprint, and also known patterns of Quaternary volcanism.

I have one item that's probably best taken up in the discussion panel session. Listening to the presentations earlier today, I see some evidence that the NRC Staff approach to volcanism is not risk informed.

In the presentations by Tim McCartin over the years on performance assessment and the risk informed evolution of that work, you have seen what that can accomplish in other areas of the program.

The volcanism work that was done is not part of the overall performance assessment. Numbers were fed into performance assessment from that group.

And, particularly slide 13 Dr. Bill's, is one that we may want to discuss in more detail. That concludes our talk. Thanks for your attention.

CHAIRMAN RYAN: Thank you very much, Neil. Any questions? Ruth, you had a question.

MEMBER WEINER: Any chance that Neil can partly answer the question that I have. And that is, how does your estimate compare with what the presentation of -- can you?

MR. COLEMAN: I believe it was mentioned that the Staff currently have an assessment of probability around ten to the minus eight to ten to the minus seven.

But, with consideration of varied events, they feel the probability could be as much as an order of magnitude beyond that. However, that -- I would just add -- that is not consistent with the record of the last 100,000 years.

The simple tests may be enough to reject this extreme tail of the probability distribution. But it does not seem to be any evidence for events for probabilities of intersection greater than two times ten to the minus seven per year.

MEMBER WEINER: So, just to simplistically repeat what you just said so I understand it, what you're saying is the tail of their distribution is not supported by the record.

MR. COLEMAN: Right, we do not see that extreme. We do not see any evidence to support that extreme end.

MEMBER WEINER: Thank you.

CHAIRMAN RYAN: Just one quick question. And I guess I'm actually asking this out of ignorance. Why did you pick a Poisson distribution over any other to use as your model.

MR. COLEMAN: There were other distributions that could be used. That one has long been used in earth sciences for evaluating events, including clustered events of low probability.

It has been used in earthquake analysis, as well as volcanism.

CHAIRMAN RYAN: It's used in radioactive too. But, I mean, is it a standardized model of how to model these geologic events, is that what you're saying?

MR. COLEMAN: Yes, it is commonly used.

CHAIRMAN RYAN: Thank you. Okay, questions from the panel members or other participants?

(No response.)

CHAIRMAN RYAN: Other questions from Staff, or the audience? Yes?

MR. HINZE: A quick question. If I understand you correctly, you are suggesting that, in the Connor and Hill paper 2000, that the idea of an

Amargosa low, gravity low, is due to decompression that is because of lower pressures involved.

It is not a viable hypothesis for the concentration of volcanic activity. Is that what you're saying?

MR. COLEMAN: I don't think that's quite what I said, but --

MR. HINZE: But, you were suggesting that the use of the gravity weighting was inappropriate.

MR. COLEMAN: That's absolutely right.

MR. HINZE: And, the reason that they use the gravity weight was because they had to had did hypothesis -- if I'm understanding it correctly -- that it speed compression effects that are localized in that area.

So you're -- being complacent -- too insufficient to cause volcanic activity.

MR. COLEMAN: No, I would not suggest that at all.

MR. HINZE: What do you suggest.

MR. COLEMAN: I essentially agree with the decompression modeling. There are a lot of discussion and debates about relative depth, the rise of the magmas in this region.

But, the idea is that the density map that we see today -- most of it was created long ago, in Miocene time and into part of Pliocene time, and represents these crustal deformation effects produced by the very high rates of extension.

Then, to use it to modify the distribution for trying to project future volcanism, in the current Quaternary period, makes no sense. What does make sense is it's a partial explanation for why the continued volcanism is there at much lower rates. Have I answered that for you?

MR. HINZE: I understand where you're coming from now. Let me ask you another thing about your comments about using only -- focusing on the Pleistocene events to achieve a more robust analysis.

One of the reasons why I very much like to see us extend the area of volcanism that is involved is because, in this extrapolation, you need a large number of events, and, if you're going to have a robust analysis.

And, by including the Pliocene, what you're doing is you're increasing the robustness of the determinations. Is that not correct?

MR. COLEMAN: I believe that is correct. But there are reasons why we would suggest using the Quaternary grid. From a regulatory point of view --

something for the Staff, the Committee, and others to consider -- there is great power in approaches that can dramatically reduce some of the uncertainties.

And the whole question about buried events and their effects on the probability essentially vanishes. If the events that we're looking at in Quaternary -- we have a very high confidence in what that recurrence rate is.

But this other point, which I actually read about in the reports from the CNWRA folks, that the Quaternary events actually yield a somewhat higher probability because, as a group, they are somewhat closer to Yucca Mountain.

So, I would submit that it is the robust analysis in that sense. And, in a way, it partly responds to the model that has been submitted by Jean Smith, a model that talks about the pollution in volcanic fields and where new events might occur in the periphery of others.

This actually allows for somewhat of a migration slightly closer to the sight. And that is the reason that you see slightly higher probability, but still very low and far below the extreme tail that was presented earlier.

MR. COLEMAN: Thank you.

CHAIRMAN RYAN: Any last questions? Yes, please.

MR. MELSON: Yes, Bill Melson, can you make any comments about how low the probability is? You sound pretty clearly talking the tail off the high end. What do you do at the other side?

MR. COLEMAN: I don't have the figure here with me. But, when we take our central result and use the same Poisson -- the test for determining confidence intervals -- I will get that for you.

I suspect that the number will be slightly below ten to the minus eight per year. But, the results from -- the results shown on my slide fifteen would suggest that ten to the minus eight per year is too low, that we had more events than that in the last million years.

So, regardless of -- I think the best way to answer your question is, I still see evidence that the probability is somewhat higher than ten to the minus eight per year.

So, therefore, the consequences of low would need to be considered, as they will be in the next session.

CHAIRMAN RYAN: We'll convene if there are no other comments or questions. Oh, yes, Tim

McCartin.

MR. McCARTIN: Yes, Tim McCartin, NRC Staff. I would like to clarify something for the Committee, the performance assessment effort, as well as the development of risk insights and risk informing the NRC process.

It has been a team effort. And, in my opinion, the igneous activity is not a separate activity that was done offline.

CHAIRMAN RYAN: Thanks. Any other questions or comments? We'll reconvene our afternoon session promptly at one o'clock. Thank you very much.

(Off the record for a lunch break.)

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

SESSION ONE WORKING GROUP ROUNDTABLE DISCUSSION

CHAIRMAN RYAN: Okay. The first thing this afternoon is a panel discussion with five individuals, Dr. William Melson, Dr. Bruce Marsh, Dr. William Hinze, Dr. David Johnson, and Dr. Robert Budnitz.

Let me take them in reverse order of what's on my agenda. We'll start with perhaps Dr. William Melson. Can we have your comments, your thoughts?

What have you heard? What should we listen to?

MR. MELSON: Well, as Michael said, I'm Bill Melson. I'm a curator at the Smithsonian. I've worked with the TRV since about 1889, the volcanic --

CHAIRMAN RYAN: Since 1889?

MR. MELSON: I'm sorry, 1989.

(Laughter.)

MR. MELSON: My comments on the morning session are generally that I thought it went very well. Quite frankly, it's not adding a lot to what we already know.

But, I think we've come along further. And yet, I do wonder about what we can learn by examining some of the PVHA issues again. I think Bruce's comments about you have a small dataset that's been looked at in many different ways and, if it is change, will it really see the uncertainty limits on what we've already done.

So, for better or worse, I would think we aught to look at that very carefully if it's not too late about going ahead with it, and be sure there's a very strong feeling and rationale as to why it needs to be redone.

I was very gratified by the comment or presentation by Neil Coleman and actually using the repository as an experimental body to look for to use it to look at the big frequencies or likelihood of dike injection repository.

That was new. And, it certainly is consistent with staying fairly low probabilities of intersection. It doesn't, to me, raise any new flags that we need to be concerned about.

I think Bruce Crowe's comment son drilling and sort of finishing up some of the work of the anomalies very near the site would be well worthwhile.

I think, for now, that's about all I have to say.

CHAIRMAN RYAN: Okay. Well thanks, that's a great start. We appreciate your comments. Dr. Bruce Marsh, sir?

MR. MARSH: Yes. I was very pleased with the morning's presentations. And one of the things I was particularly struck by also was the fact that it's always a big problem in geology.

We look at layers upon layers and layers of things that have happened in sorting through those. But that's really not our firmament. That's really what we have in the record, the historical record, the geology when we look at it.

So, one of the things that I don't think has been emphasized enough -- it came up in Bruce Crowe's comments -- is that the tectonic development in the area, the history of that, can be read pretty carefully because we have ash loads and we have erosional surfaces, and we have fault histories and things, and questions, for example, of whether or not this block that Yucca Mountain's on, and that whole area, is still structurally attached to what's going on in the basin to the west of it.

It is a very important issue. And there's a lot of cogent things that can be said about that. A lot of the style of what caused the tectonic style that basically encouraged the volcanism and gave rise to what see today was set up in the Miocene, 15 million years ago or something.

And yet, when we look today at these things, it's like looking at the heat flow. The heat flow of the surfaces is reflecting the thermal conditions in the crust ten million years ago.

We can become confused a bit by that in thinking that, you know, we're in the middle of an onslaught of something new. So, it's very nice to carefully sort out that and realize what kind of environment we are in today and to look at that in terms of the last one million years, two million years.

And so, it's very important to put the geology into the models carefully -- topography, what's in the basins, what structural units are talking to each other, and which ones aren't.

The deeper we go -- there's an interesting phrase by -- I believe it was Francis Birch -- who said that it's interesting, the deeper we go in the earth, we know less and less, but our description of it becomes more and more exact.

And, this is what happens a lot. We actually go down in the mantle and say, well, we're melting the mantle, and we have fertile mantle, depleted mantle.

We have a thermal pulse here. We have a small plume. We have thermal convection. Really, objectively speaking, I mean, we have a hard enough time understanding how a volcano that's about to erupt is going to erupt.

And we have no chance of actually using any of that deeper information. So, in other words, in putting -- we tend to use that Poisson distribution for time and for spatial events.

If we go down deeper in the crust we know that basically we have an exponential decay of what we understand. In only using the geology of things, we understand very little to be used in a predictive model as we go deeper into the earth.

And so, there's a cut-off. We should use that. We should put stuff, and model it, we really know something about, and ignore stuff that's pretty below the horizon in terms of being able to scientifically say cogent things about it.

So, the other thing that's an interesting thing is that, I think, you know, at least we're all in the same room, in terms of we don't have -- there are disparities.

But I think they can be brought into line.

And I think the interesting thing about it is that there is sort of a common sort to Socratic element here that can be used to adjust each other's points of view or to convince one another whether or not we should take certain bounds seriously or not.

The idea of looking at things between ten to the minus eight and ten to them minus seven, and just agreeing that that window -- not worrying about so much where we are in that window, would be a very interesting way to approach these problems. Thank you.

CHAIRMAN RYAN: Thank you. Dr. Hinze?

MR. HINZE: I enjoyed this morning because, one of the reasons I think is that, as a result of this morning, your job is going to be less difficult than perhaps it could have been.

There's a certain amount of unanimity in the conversations that we heard, that we don't have all the answers. Bruce added on that very well. We don't have all the answers.

We're not going to have all of the answers. And, Bruce Crowe and Britt Hill both commented on the fact that we're not going to mover people very far from their models.

But the point is that the models aren't

making -- the difference in the models are not making that much difference. Now, I think, in terms of the probability, ten to the minus seven, ten to the minus eight, and 1.1 times ten to the minus seven or two -- I don't think we are smart enough to worry about those type of things.

I think we have to keep a pretty broad swab here. Let me say a few things about the recurrence rate. What we're dealing with here is a science where we're dealing with a situation where we don't have precursors that are in the right timeframe.

We have only the very basic knowledge of the -- Bruce we only have a very fundamental knowledge, basic knowledge of the physics or the geological control.

I'm sure that was said several times here today. And so, what we have to do is we have to extrapolate from what we do see. And extrapolation means that we're going to need to deal with probabilities, which we are.

We have uncertainties -- and certainly we do have those. How do we cut down on those uncertainties? Where are the points where we can go in and cut down on those uncertainties?

I don't think there are many points that we can go to to cut down on the uncertainties, the definition of igneous event or the length of the dike, etcetera.

And it turns out, as Bruce and others have suggested, that this is not making much of a difference in the results. But one thing that can make difference is the number of igneous events that have occurred within the last timeframe.

I personally would like to see a timeframe that extends to four or five million years. And I think that's backed up by the ten independent scientists that worked in the PVHA.

And, we have been saddened with inadequate way to look at these past events. The 1999 survey of the USGS solved the purpose of not accounting, and perhaps some others.

But it didn't solve at all the problem of the events that may be hidden in the -- beneath the alluvium in particular. And so, the DOE, I think very appropriately, has set -- embarked upon this new magnetic survey, which we've just seen the first light of.

You have to realize that there are -- I hope I'm not duplicating what Britt said this morning. But, there are basically three types of magnetic anomalies that we are observing in the area.

First we are dealing with long wavelength anomalies that are derived from rather massive structures within the rocks and pre-cambiam rocks.

And those are long wavelength and should be able to be discerned pretty well. But, they are going to overlap in spectrum with the magnetic anomalies that are buried within the rather deep alluvium.

The second type of anomaly is the anomaly due to the permanent and susceptibility, magnetic susceptibility, permanent magnetization and magnetic susceptibility of the tuffs.

These will produce anomalies that -- particularly where they are faulted or whether it's been structural disruption or some variation. And, finally, we have the basaltic rocks that we are interested in.

The problem is -- one of the problems is that the latter two types of anomalies may give somewhat the same signatures. And so, we have to be smart enough in analysis.

And we must have the right data in order to differentiate that. Ideally, the specifications of the magnetic survey were such that we could make great strides.

Due to an unfortunate set of circumstances, some of the data is going to be degraded -- is degraded -- from what the DOE wished to have.

And that's going to have a serious impact on the results. The DOE has very correctly attempted to -- or is going to attempt to differentiate between these two types of magnetic anomalies that have somewhat the same spectra.

That is the tuffs and the basalts. By feeding them against the electromagnetic response, and in this way attempt to identify the higher susceptibility basaltic rocks.

I'm going through this because I want to make it clear -- at least in my mind -- that the results of this new survey are going to have an impact, could have an impact.

But it isn't guaranteed at all that it's going to have an impact. There are many problems in interpreting these data. And one of them is especially the above mean terrain clearance, which has been degraded some bit, especially in the rich areas.

But, also, there is an overlap in the susceptibilities between the tuffs and the basalts, which may make the EM impossible to differentiate. In addition to that, there are some conductive zones, alteration zones, and fault zones where there's alteration as well, that are going to complicate the interpretation by trying to differentiate basalt from the top using the EM data.

My own very quick review of the data is that there's nothing that comes out and bangs you in the face and says this is obviously going to change the probability, the recurrence rate, in a quick look at the data.

But there are a lot of very interesting anomalies. And there are a lot of interesting anomalies, particularly to me in Jackass Flats that I think could have an impact upon the PVHA if the PVH goes in, as I understand it is.

But, it's going to take time, and it's going to take some effort. I think that prejudging the aeromagnetic results based upon the quick look that we had yesterday morning and yesterday afternoon is very -- it doesn't give credit to the DOE, nor their efforts to come to resolution on this.

So, the recurrence rate, which is the major way we can get an uncertainty in that probability factor, was going to be able to decrease with the new set of -- the interpreters will do that.

But, it's going to be a difficult process. And it's going to take some time, some effort, some resources. I guess I'll leave it at that.

CHAIRMAN RYAN: Okay, thanks Bill. Dr. Johnson?

MR. JOHNSON: Thank you. I think first I should say a few words about my background. I'm not a geophysicist. My field is in developing probabilistic formats and methods to support the decision making.

So, from that point of view, I hear the presentations about whether or not the frequency of intrusion is ten to the minus eight or ten to the minus seven, or even six.

From my background, my experience, and again not knowing much about the chronological issues, those tend to be in violent agreement in my mind.

That said, I think it is important. I think it was said earlier that it would be a useful exercise to have the experts go and try to present their findings, if you will, in a format of a probability of frequency format so we understand what their key assumptions are and how they affect their results.

This is obviously -- would be useful for more fundamental understanding of what's going on, but also as new information is derived in the future and issues pop up.

It might provide a pretty sound basis for quickly reacting to those sorts of events. I am kind of waiting for the so what to all of this. I am anxious to see what the scenarios look like from an initial condition to the final end states of the analysis.

I think once we have that in hand we can then go back and make judgments from judgments on whether or not our understanding of the frequency of volcanic intrusion is something we need to focus more on.

I do think that there is some -- for investigations of some of the near field anomalies that would make a lot of sense to resolve. I think I'm waiting to see what the big picture looks like before I go on any further. Thank you.

CHAIRMAN RYAN: Thank you very much. Dr. Budnitz?

MR. BUDNITZ: I'm Bob, Budnitz. I'm Lawrence Livermore Laboratory. But I'm on detail to the Yucca Mountain project DOE. So I'm here with a

DOE hat on.

But, I need to give you some background. I'm not an earth scientist at all, never mind a volcanologist. But, you know, it comes in by osmosis, so I now know about ten to the minus four about these other stuff, which is a hell of a lot more than ten to the minus eight.

I want to tell you something about the status and explain why DOE is in here. The Department has written the license application. We're sending it in December.

I imagine you will have it by then. It's only three months away. And right now it is an intense review, everything, not just the igneous piece, everything.

It is an intense review for consistency and to make sure that we do the validation and the quality assurance checks, and make sure everything that we're going to send in in December hangs together into a coherent application.

I'm sure you understand that. And, because that process is right now in its final stages, we found ourselves not in a position of being ale to talk too much about the details because it's just now coming together into something that's final.

But, I insist on December 15th it's going to go in, as we say. And we're going to put it in. And, when that's true, it's going to be a public document.

And anybody in the public can read and review it. Certainly if you were submitting it -- the regulatory Commission, the Staff and ultimately the Commission for a review for -- to get a construction authorization.

But, it will be in the public domain. And, at that time, anybody who wishes to review it will be able to do so. I have two or three things to say about the license application that are relevant to what we heard about this morning.

First off, everything we've done in the license application is risk informed and, in parallel, responsive to the Yucca Mountain review plan, which is the NRC's -- you know, the Staff review plan.

We know they are going to review it again some time. And so, which means because we have to be responsive with the Yucca Mountain review plan, some of the stuff that is in the license application isn't risk informed because the review plan isn't necessarily risk informed, although our criteria in the end is.

You know, the part 63, individual dose based criteria is, of course, dose informed. It's not risk informed, it's risk based. So, because of that, the license application analysis is intrinsically probabilistic through the -- because our regulation is probabilistic.

So, what you're going to see is a probabilistic analysis intrinsically -- that's just the way it is -- modified by the fact that, of course, we do have to respond to the Yucca Mountain review plan, which means a whole lot of stuff is in there that is supportive -- or in some cases we review other things that aren't.

You can imagine what that means. And, of course, we have to be attentive to the technical issue, you know, the agreements that we made. And, I guess, in that sense, we just look forward to submitting it.

And, somewhat later, the ACNW along with the Staff will have a meeting like this in which we can discuss what we've done, which we've just -- position to talk about here.

A couple of other things that are very important to say. And that is, although the work to support license application is done by definition --

we have not stopped work in the igneous area.

People know that. The aeromagnetic work that was done from March until June is just now being analyzed, and will be available perhaps the next six to eight weeks for public review.

We'll let the NRC review it at that time. And, after that, there's a plan which was discussed at yesterday's NRC meeting, to do some drilling of several of the sites.

Exactly what drilling will be done hasn't been decided yet. We're going to have to sort out exactly which targets and we don't have enough money to drill a thousand of these things.

We're just going to drill a few of them. And, how to select those, is a difficult choice between different agendas. Secondly, and I suppose many of you know, but I should tell the rest, we are beginning a new PVHA, probabilistic volcanic hazard analysis.

The first meeting to kick that off is in the second week in October. It's the data needs workshop in which the data needs for the PVHA are going to be discussed amongst the experts.

And that will kick that off. The PVHA, the new or revised, is due to be completed in the first half of the fiscal year '06, a year and a half away.

And, both of those -- and, of course, PVHA is supposed to integrate a whole lot, both of those are confirmatory in nature when we do. That is, we believe the license application is strong enough as it is, but we're doing confirmatory work because, as we must, we are going to continue to do that work over the years.

You never know whether you find something that doesn't confirm with the expected. And we're going to proceed on that basis, and challenge the data and assumptions and so on.

And, as other work may emerge that needs to be done over the future years, we will consider doing that too. We just don't know what that would be.

So, I'm just here to tell you that we're very close to having something that everybody will be able to look at and review. It will be a public document with the license application, with all the supporting data and everything else that supports it.

We are proceeding with more technical work now. And, whether more than that is going to be needed, we just don't know yet. We're going to let those chips fall where they may as time goes on.

CHAIRMAN RYAN: Thanks very much. Any comments from the Committee or the panel? Ruth?

MEMBER WEINER: Just a brief comment on Dr. Budnitz's last comment. If you're beginning new PVHA, and I assume that means the new expert elicitation --

MR. BUDNITZ: Yes.

MEMBER WEINER: What kind of differences do you expect to happen?

MR. BUDNITZ: We have no idea until it is done. We just don't know. The nature of this is it's a scientific investigation, like they all are. And, how it comes out will depend on how it comes out.

I'm not ducking that question. I literally couldn't say, because we have an open mind as to what the data will -- how it will be understood and what models will be used.

And who's going to argue with who about what?

MEMBER WEINER: What was the primary driver for this? I mean, I'm just curious, because it's late in the day.

MR. BUDNITZ: The last one was seven years ago. And a lot more is understood now than then. And so, we believe that we will be in a better position than we would otherwise be by doing it.

Otherwise, we would have nothing but that old thing and some patches here and there. Instead, we're going to have a -- how should I say it -- a coherent, consistent PVHA that is intended to integrate all of this into a framework that we believe the community will participate in and endorse.

CHAIRMAN RYAN: Thank you. Yes, sir?

MR. MELSON: Bill Melson. Bob, will the DOE's volcano assessment be close to what you've put out in, I think, January 9th of this year? We have gone through that and it seems like a pretty strong document.

So, is that, to your knowledge, what's going to go ahead?

MR. BUDNITZ: You're asking me to part with something that I'm not willing to do?

MR. MELSON: Well, I'm just wondering, because that gives us a preview, I suspect.

MR. BUDNITZ: You can peak if you want. You'll know on December 15th. I'm not ducking. It's just that it's hard to respond.

MR. MELSON: Yes.

MR. BUDNITZ: Whether something is close

to, of course not to be completely different, how close it is? You know what I mean?

CHAIRMAN RYAN: Questions from the Staff, other comments? I guess I want to try and summarize a bit if I can. I know it's a daunting task, particularly for a non-geologist.

My geology mentor on the ACNW, Dr. Hornberger told me geology is easy. They always want to dig one more hole. That seems to be the case today.

I guess that's one of two important elements. The aeromagnetic data seems to be a critical issue. I think, Dr. Crowe, you suggested some drilling and some value that could be acquired through that drilling.

I've heard three or four folks endorse that idea, that that might actually help reduce some uncertainties. And then I think the theme that we really haven't touched on, and I would like the other panel members to talk about, is -- except for David who mentioned kind of a more formal probabilistic assessment here.

I think Dr. Garrick mentioned that earlier in the morning, that a more rigorous treatment of probability analysis or a probabilistic approach, maybe a Bayesian approach to the event side of things, maybe in line with some of the things that Neil Coleman presented, might be crucial.

It's kind of the third leg of the three major components I heard this morning. It might be different or enhancements, or improvements to what we know now.

Is there any reaction to that? I mean, could any of you talk a little bit more about the probabilistic approach?

MR. GARRICK: Well, I want to follow-up with what you said, because it might make a difference. I was curious about the new PVHA. And Bob said that we've learned a lot more now, and we'll want to incorporate that.

And, I had a couple questions. One, is the same team that did the PVHA one going to do PVHA two?

MR. BUDNITZ: Eric Smithstead from the DOE Staff in Las Vegas, I think, can an answer that question.

CHAIRMAN RYAN: I'm sorry, John, maybe you can repeat your question so everybody can --

MR. GARRICK: Yes, I was very curious about the second time around. Bob indicated that we've learned quite a bit. And that's one justification for the second time around.

I wondered if the same team that did the PVHA one is going to do PVHA two.

MR. SMITHSTEAD: Right. Eric Smithstead at DOE, that's what we're looking at right now, is trying to reassemble the same team. We won't get everybody back. But we'll have the majority of them.

MR. GARRICK: Thank you. While I have the microphone, I wanted to ask Bill Hinze a question. Bill, are you awake?

MR. HINZE: With you talking, John, how can I help it?

(Laughter.)

MR. GARRICK: You mentioned a couple of categories, some things that you thought aught to be done, but probably wouldn't make much of a difference with respect to the probably and some things that you think aught to be done that will make a difference.

Can you elaborate on that a little bit as to why we want to do the things that aren't going to make a difference? How would you prioritize what we should do?

MR. HINZE: Well, we wanted to decrease uncertainty. And I think that's one of our functions.

And, the major uncertainty in the PVHA was recurrence rate.

And that very well typifies the fact that in reducing uncertainty, that we want to look at the number of volcanic events in the last million, two million, five million years, six and seven.

And so, that has to be done or -- the process of being done. I also pointed out that -- and this is not my original thought -- that there are some particular areas on that map that are interesting in the surrounding area.

And one of those is Jackass Flat. And the reason for that is that if we had the Quaternary volcanism jump across the ridge on Crater Flat to the other side of the repository, we would be -- I think this would cause contemplation on the part of any analyzer of the data.

I am reminded of Mike Sheridan's comment. At the last appendix seven meeting on the aeromagnetic that -- July of last year, as I recall -- Mike was the only one at that meeting that was part of the PVHA.

And Mike stood up and -- paraphrasing him, he can speak for himself usually -- if we knew volcanic sediments were found to the east of the repository and extension to the south, that he would need to reanalyze his PVHA position.

MR. GARRICK: I always find that --

MR. HINZE: Did I do it correct Mike?

MR. SHERIDAN: Correct.

MR. HINZE: Did I read it right?

MR. GARRICK: I was trying to get at what you would consider to be the biggest action that would give us the biggest bang for the buck.

MR. HINZE: Exactly.

MR. GARRICK: Yes. And I want to take the opportunity to indicate that, in the category where you said it wouldn't change the probability much, but it would change the uncertainty, it certainly changes the risk.

And we want to make that distinction. So, both categories have substantial impact on risk.

MR. HINZE: Yes.

CHAIRMAN RYAN: Any other comments from participants? Yes, Dr. Marsh?

MR. MARSH: One that that's a little bit, I think should be of some concern is that DOE submits its application, the way that they've assessed, or estimated, or come to grip with the probability hazards for volcanism, is basically using panel experts.

And they have built into it their knowledge, geology background, exhibit with volcanism, etcetera, a series of series of estimates.

And that's really a substantial amount of experience. But, it's not a computer program. On the other hand, what we've heard this morning, the Center and the NRC has a program, Connor et al.

And that has built into it several things on various premises. And, that's almost a different language than the other. So, we're going to use one set of principles to evaluate another set of results.

It's almost passing in the dark. It could be. In other words, you could be speaking different languages. And so, I think the in-between land -- worrying about where all the geological influences that the various experts would use to modulate their results, where do those exactly fit into a computer program or a program that someone would have?

Where are the analogs? Where do these things go in, you know, in layers that we can put in and take out? And, I think, to do an effective evaluation, you really need to have that expertise built in or you have to have that flexibility in the evaluation.

For example, this focus on putting

topography in and out, but, the mantle in and out -- slide these filters in. Otherwise, I really wonder, you know, how will anybody do an effective evaluation in the DOE program.

CHAIRMAN RYAN: Dr. Budnitz?

MR. BUDNITZ: I just want to be sure to point out that the PVHA, the structure, if it is executed properly, will do just that, as it is intended to be, by structure, a form for such explorations among the experts who bounce some things off of each other, and considering literature that isn't in the room.

And they arrive at a common understanding of all the underlying data and all the different models that explains those data, in order to deduce what is sort of the best you can do.

I don't know of any better structure than that to do that. In the end, there are -- that is, to structure such a way to pull out what the community's knowledge is and the different approaches to it.

And, if it is successful, why there won't be any stone left unturned. Although, of course, the experts themselves are the ones that have to sort out which are the important and which are the less important issues, which models may -- while they fit the data -- don't make sense for another reason, whatever comes to mind.

I know something about this because the methodology used in the PVHA is called the SSHAC methodology. SSHAC stands for the Senior Seismic Hazard Analysis Committee that developed methodology the probabilistic seismic testing analysis.

And I plead guilty to having chaired the SSHAC committee for three years. So I think I understand how all that works. And, if it is successful, it will, in fact, not only allow, but require the consideration of all the different models and data we've got there.

CHAIRMAN RYAN: Thanks. I guess I'd like to maybe turn a question to the NRC presenters from this morning. And, you know, I took note on, Dr. Trapp, your comment that you're on the vote of reviewing an application.

So, you don't have the burden to come up with the answers to all these wonderful questions we thought up today. But, I wonder if maybe you could talk about the following things, or Dr. Hill, either one.

You know, we've heard a lot about deterministic values, about bounding analysis, and now a little bit of discussion about probabilistic analysis and so forth.

Could you talk a little bit more about how all that fits together in your mind from your reviewing of potential application? Or is that too broad of a question to dive in on?

MR. TRAPP: If you take a look at part 63, it does require a risk informed analysis. It does require that you go through the whole probabilistic analysis to get to the end.

I'm not really sure how to answer your question.

CHAIRMAN RYAN: Well, I guess I'm reacting to a couple of comments that Britt made where you had deterministic kinds of thinking in the structure of your presentation.

How does that fit when you're trying to assess a probabilistic assessment?

MR. TRAPP: That normally is used to get some kind of value, etcetera. And a lot of times it is used when you do not have a good handle or can't resolve some of the underlying scientific basis.

CHAIRMAN RYAN: But isn't that a risk that you'll either include or miss something when you just decide on the deterministic value for a key parameter?

MR. TRAPP: I'm sure Dr. Garrick would say yes.

MR. HILL: This is Brittain Hill from the Center. The hope is to -- the licensing interaction, is to come up with -- especially for the conceptual models.

Now, at this stage, I don't think we're gaining a lot of information by trying to an artificial distribution on the limited range of alternative conceptual models because, ultimately, we're trying not to find the simple tendency and cluster of models, but look at, given the current uncertainties, and given the current testable hypothesis, what is the potential significance and the risk calculation from these alternative hypothesis.

So, this really is more of a testing methodology than trying to arrive at the mean value that we use to make a regulatory decision. So, that's why we haven't gone through the exercise.

We're trying to come up with a distribution function using alternative conceptual models for both the probability itself, and some of the probability parameters. Does that help?

CHAIRMAN RYAN: It helps.

MR. GARRICK: It helps. Well, before we

leave this probability discussion, I wanted to get a couple of licks in. I think that one of the things that the regulators are faced with always is how to make the analyses we've performed as transparent, as understandable as possible.

We talked a little bit this morning about how to, and at the same time how to reveal what's really going on, how to reveal the truth. And we talked about these igneous event scenarios, these categories, and the volcanic frequencies that you had associated with these categories, and how characterizing those and embedding those frequencies, because there's uncertainty in those frequencies and uncertainty in probability distributions to kind of convey with time and with conditions with aging and so forth, how the uncertainty changes depending on those conditions.

It can be very illuminating. Another thing that I think would be very illuminating, to pick up on Dave Johnson's comment and Mike Ryan's earlier comment, would be to more deliberately manifest the value added of new evidence systematically.

And, of course, Bayesian application are perfect for that kind of thing. And there's very little of that that's been done in the past in any truly systematic and constructive way.

And I think that, if we could somehow create a map of what value is added in terms of our knowledge about the risk as a function of pieces of evidence, I think that would be enormously beneficial in aiding the whole process of what the probabilistic analysis is telling us.

I would hope the second time around advantage would be taken not only of what we have learned about the earth and about Yucca Mountain and its geology and the rock, but also what we've learned in practice with respect to how to characterize risk in our analyses.

And much has changed in the last few years about that. I hope that we take full advantage of that, especially with respect to the transparency issue.

CHAIRMAN RYAN: Thanks, any other comments? Yes?

MR. JOHNSON: Just to add something, what I meant by saying that the model builder talked to embrace the uncertainty in the models as much as they can and then try to articulate it.

For example, if we're saying that there's a zero chance of these relatively recent volcanoes to occur in Jackass Flat, your belief is that's absolutely zero.

But, we aught to go look to see if -- any better. So, what we're really saying is we're not 100 percent certain of the fact that -- volcanoes from that particular source.

So, if that model were to express that level of uncertainty, it might be very certain, but not 100 percent. Then, as new evidence arrives, then the model can accommodate that, or we can look at the model.

It can tell us how important that new evidence can be. I think it's just a more robust explanation of our experts.

CHAIRMAN RYAN: Any other comments in the audience? Yes?

MR. MELSON: Yes, Bill Melson. I just want to comment that I've heard a little bit of the rumors going on about the appointment of the PVHA and who is going to be on it.

And I think Bruce's concern can be lessened somewhat. And that will include someone who -- the core of a lot of the NRC's contract work. So, I think they will not pay us in the night, these things will be.

That's the intention, I think, of some of the planning.

CHAIRMAN RYAN: Yes, in the audience.

MR. REITER: I'm Leon Reiter with the technical review board staff. I have a question about the PVHA. I guess the first is to NRC. Weren't there any methodological concerns with the other PVHA that DOE had to take into account?

And, if there were, did DOE take that into account?

MR. TRAPP: The PVHA actually was started a little bit before the Nureg -- PVHA or this time of elicitations. Two areas that really were of concern with the original PVHA panel was the criteria, documentation of the criteria, selection of it, and then, basically, the total documentation of the analysis itself.

These are areas that we thought could be improved and were areas that, in this panel would be better.

CHAIRMAN RYAN: Yes, a question.

MS. KEEFER: Susan Keefer, University of Illinois. I'm an incoming member of the NWTRB, but I'm sitting here until I master my acronyms. My question, Lathrop Wells, my reading of the literature was that there was water interaction indicated both at the beginning and the end of that sequence.

Am I misunderstanding it, or has that been considered?

CHAIRMAN RYAN: Do we have someone who can answer that question.

MR. CROWE: There is some controversy over the height of volcanic features. But they probably occur about midsection in Lathrop Wells. And then there always is -- the cone itself is an unusually large -- the ration of pyroplastics to lava is unusual for a typical cinder cone fields.

But, I think there's strong evidence that it's hydro-volcanic. But, I mean, Leon was on many field trips. We had maybe, eight or ten people at an outcrop that I thought was unequivocally surged.

And we had two or three people who swore up and down that it wasn't. So, there is some uncertainty in identifying those deposits. I think the majority of people feel that there was a hydro volcanic component, probably predating the main final cone that we see out there.

MS. KEEFER: I'm a consultant to the NWTRB.

CHAIRMAN RYAN: Any other questions or

comments in the audience. I want to thank all of the -- I'm sorry, is there a question.

MR. KESSLER: John, Kessler, EPRI. Two comments, both from a performance assessment perspective, surprise. One is a specific comment, and then the another a more general comment.

The specific one was on the discussion this morning and Britt Hill's talk about the temporal variability in the number of volcanic events that occurred and how one might deal with that performance assessment space.

And, Britt talked about, well, you could have maybe as many as something like 40 some events per million years if you look at the right million years, and then, maybe look at the mid-point between that and the long-term average.

Well, I would argue that, if you're going to look at the maximum, the mid-point isn't with the long-term average, but it's with the other end. It might be something like zero events in a million years.

And, from a performance assessment standpoint, you could say, sure, I'll show you everything that I see. I'll show you, for any million year interval, I'll show you a table of zeros, maybe.

And then I'll show you one where you have something when you get a higher dose risk for that particular million year interval. So, it comes back to what is it that's the compliance number you can use.

And I'd still say come back to whatever the scientists agree is the right number of years to average over. But you're still going to take the average, unless you have anything that suggests you know what's going to happen in the next million years or in the next time period, or whatever.

If you don't have any way of distinguishing that, to me, the long-term average, whatever the long-term you choose to use, is what seems to be the right course of action to take.

You can go ahead, of course, and add sensitivities on any particular variability around that, from zero up to forty some. But, in the end, I would think that, from the compliance standpoint, that would probably be what you would want to do.

Now for my general comment. It really falls right along the lines about what John Garrick was talking about, which is, I felt that a lot of the discussion this morning, where we're talking about maybe changing probabilities by factors of five, maybe ten, is interesting and all.

But the uncertainties in just the volcanic dose risk assessment, when you look at the risk triplet, what can happen, you know, what are those probabilities, and then what are the consequences, it seems that, you know, we should be focusing on a lot of other aspects, given the kind of changes we are talking about.

So, why are we talking about this? I mean, a lot of money, and a lot of time have been spent. Well, why is that? That's because assumptions have already been made about what the consequences are.

For example, in John Trapp's talk, he had one line where he said, well, we've had some analysis that said the containers are going to fail, which is assuming they go poof and that's it.

Okay, that's why the numbers come out the way they do. Those kinds of assessments were done, I don't know how much thinking went into that part of the assessment, but it's just as important in terms of coming up with the dose risk numbers, as getting this probability that we're talking about that may change by a factor of two, ten, something like that.

So, my point is that one can do a lot of expert elicitations on all kinds of aspects of the system. I just don't know, if I was king for the day, I'd spend my money on redoing the PVHA, versus some other aspect of this problem or something else in terms of getting at reducing uncertainties.

The uncertainties that are being looked at here are ones that, it could increase the uncertainty because we've already made all these other conservative assumptions.

That's why this one is being looked at. I think if we looked at what can happen and shouldn't just look at what can go wrong, but what can go right.

If we're looking at a best estimate assessment, if there are possibilities that we can replace the model that's conservative with maybe something else that's less so that could dramatically affect the dose risk numbers, perhaps that should be looked at just as strongly -- things towards what could go wrong.

CHAIRMAN RYAN: Thanks John. Any other last comments before we finish up the session. Yes, sir?

PARTICIPANT: My name is John. I'm a consultant. I have point of clarification and comment of concern, if I may. My point of clarification is with respect to our early warning drilling program, 23P, where we first talk about intercepting the basalts there.

Our geologists have taken a look at it. And they do not believe that they basalt below. They believe that they basalt boulder. Their reason for that is because, in looking at the cuttings, they saw roundings on the upper portions of it.

And they saw a rounding of the cuttings. The second thing is, they saw no alteration or difference in the sediment of any of the above and below.

So, we're not sure that we had basalt flow. I'll tell you, in terms of a comment, I don't think that enough time is being spent in looking at the structural relationships between these existing volcanic centers and flows, and what we're trying to get at here.

One thing that really jumps out to me in one of the presentations, Dr. Crowe put at the basin range and showed how the big activity was on the margins of it.

When we look at the magnetic math from yesterday and the previous work that -- commissioned back in 1999, we saw major magnetic east-west liniments being transected with north-south ones.

And, when you look at where these recent volcanics are, they are at the intersection of these. We think there needs to be more work done like this gentleman talked about in looking at the structural aspects of it.

And then, finally, on the concern, and this came out yesterday, the new magnetic work is exquisite. We can see some really good things in there.

We can see our EWDP wells, where we put in deep steel cases. Those show up on that survey quite clearly. My concern from yesterday was, when we're laying it out at the workshop, I pointed to one anomaly.

And I said, well, what's this one? And the response was, oh, we hadn't noticed that one. And that's a concern to us. We're not volcanologist.

We're not probabilistic people. We have to rely on other people to do these things. But, when we look at something after ten minutes and say, what about this one?

Then we think there's a concern there, that maybe there's a method that needs to be looked at to make sure that all the anomalies are indeed identified.

And the County doesn't have to come along after the fact and say --

CHAIRMAN RYAN: Thanks. I'll just add that I think many of us here, me in particular, weren't at yesterday's meeting. So, we're a little bit blind.

And we didn't have an appreciation for what you have in those. But, thanks for sharing it with us at this point.

PARTICIPANT: Again, they're doing a terrific job. It is a repressed timescale. People concentrate on what they want.

CHAIRMAN RYAN: Thanks very much. Any other questions or comments? Yes?

MR. MELSON: Mike, I'm wondering if an integration of changing the PVHA at this time, and it does examine as well a probability for canister dysfunction, and that the membership be changed -- the attitude, so there are experts of the type that we'll be hearing from some of the work that's been done this afternoon to really try to go after some of these volcanological issues quantatate.

PVHA has updated. It remains what it was. But it also --

CHAIRMAN RYAN: I think that is an interesting suggestion, Bill. If we can maybe hold that in our minds as we hear the second two pieces of the parts that go into the risk triplet, maybe we'll come to a better appreciation of how to answer your question.

We can sure think about it, because that's a great segway to our next segment, which is the repository interaction with the magma, and then on to the dose consequence aspect of it a little later on tomorrow.

We are scheduled at the moment for a public comment period. Do we have any other comments that folks would like to make? Yes?

MR. McCARTIN: This is Tim McCartin, the NRC Staff. In relationship to the previous comment, for the record, I would like to state the appendix seven meeting between the NRC and DOE is a way for NRC to get information from the Department as quickly as it is available.

And, not that I have to defend the Department, it's not my role, but, in the spirit of an appendix seven, we got this information.

And DOE has barely looked at it. They have not spent a lot of time analyzing it. And so, the fact that they may not have seen everything there is, we did press them to get this information as quickly as possible.

And so, there's a lot of analysis time with the information that we have that is yet to come. But, this meeting was held before they had done this analysis.

CHAIRMAN RYAN: So, a further study of their's and NRC's will be underway. Is that a fair comment?

MR. McCARTIN: Yes.

CHAIRMAN RYAN: Thank you. I appreciate the clarification. Any other comments or questions?

(No response.)

CHAIRMAN RYAN: That being said, and it being just slightly after two o'clock, we are scheduled for a 30 minute break. I'm going to propose that we limit our break to perhaps 20 minutes.

Let's come back at, say, 25 minutes after two. And that way we'll have continuity with our presentations for the rest of the afternoon on discussions thereof.

(Whereupon, the above-entitled matter went off the record at 2:03 p.m. and went back on the record at 2:25 p.m.)

CHAIRMAN RYAN: We'll begin our afternoon session with a presentation again by Dr. Britt Hill entitled NRC Review Capabilities for Evaluation of Potential Magma Repository Interaction Processes.

I might make a note at this point that the next talk that's on the agenda will not be held. The speaker and the other members of that panel were not available to be here today.

They all had other prior commitments. But the consequence review panel is documented. It has been presented previously to the ACNW and to the NWTRV.

And that is a matter of public record. Those records will be available. Mike Lee will help anybody that wants to find those references. He will help them get the identity of those records.

So, that presentation will not be held. And then, after our first presentation by Dr. Hill, Dr. Kozak, with introductory remarks by John Kessler, will follow-up with the EPRI alternate views on the modeling of magma repository interactions.

After that, we will have a closing working group roundtable sessions with panel members to talk about that second part of the igneous event. Thank you, without further ado, Dr. Hill, welcome back.

NRC STAFF PERSPECTIVE ON THE MODELING OF MAGMA/REPOSITORY INTERACTIONS

MR. HILL: First I want to make sure that we recognize a number of people who have made the key contributions in this work on developing our review capabilities for potential magma repository interactions.

They are essentially the lead authors on a lot of this work, O. Bokhove, Anne Marie Lejeune, Steve Sparks, and Andrew Woods. Notice they are all from the Netherlands.

Unfortunately they couldn't be here today to help with these presentations. Next slide, please. What I'd like to do this afternoon is give a brief overview of why these potential magma/repository interactions can be significant to do performance calculations.

I'll set the stage a little bit for why we are doing some of this work. I'll talk for a few minutes on some recent developments that have come about in understanding the water contents of Yucca Mountain basalts.

This has been done from direct -- experiments that I think provides real interesting insights on some of the important physical conditions that we have to consider in any sort of numerical model for igneous processes in the Yucca Mountain region.

I will give a fairly brief overview of some of our previously focused models for initial magma interaction processes, but not spend too much time on that, and then discuss some of the newer work we've done in the past year or so on modeling magma flow in conduits that have elastic wall-rock problems, sort of looking at some of the relationships between fluid pressure and wall-rock response, but building up the initial models we presented several years ago.

And finally, I'll give a pretty quick summary of where this current information is leading us. Next slide, please. Why does this matter? Why do we care about potential magma repository interaction processes?

The simplest answer is these processes control the source-term for igneous intrusive and extrusive events that we're modeling in the performance assessment.

Very simply, you can think of this in terms of three very basic conceptual models. The first one would be that the rising magma would come up along a dike.

And we're looking kind of down the plane of the dike. It would vertically intersect the repository with the probabilities that we've been talking about this morning, and then continue to rise to the surface, and, during the course of an eruption, form a vertical conduit.

And, the expansion of that conduit through time is the one that could -- waste packages in the conduit footprint. Now, alternatively, sort of a model that was first developed in the Woods et al paper in 2001, we are looking for the potential for developing a breakout at some place distant from the point of initial interception of the dike.

This could be for a variety of reasons. But, basically, we're looking at a zone of weakness that was easier for magma to propagate along a secondary plane than along the initial plane of intersection.

And, again, we have no real good historical analogs or any geologic analogs for how rising magma would come up to 300 meters below the surface, interact with a void that extend for hundreds of meters laterally and are five meters in diameter.

So, we have to take a conceptual model approach that looks at information that we can use for historically active volcanoes, experimental analogs, and numerical models.

And, one of the insights that we can gain from the historical basaltic volcanism, is this third option, or third thing that we have to consider.

During the course of an eruption, you get very simply the breakouts that can occur from the main magma system and rise at some distance away from the central volcanic system.

I'll talk in a little more detail about that later in the presentation. Basically, before we start to talk about any numerical modeling or experimental modeling of these potential conceptual models, we have to constrain some of the physical characteristics of the magma system that we're trying to simulate.

One of the most important model uncertainties, really just about any model that we do for all of the buried basalts, is understanding what are our magnetic water contents.

Those of you that aren't really familiar with the geology of this area, you can dissolve certain amounts of water into the molten rock. This dissolution of water isn't from groundwater.

It occurs very deep in the mantle where

the basalt originates from. There are minerals in the mantle and in the crust that contain water in them.

And, when these minerals melt, they release water into the melt. That water stays dissolved until you depressurize the metal at some point.

Then the water and other volatiles, like carbon dioxide, come out of the solution and form a gas base. Is this expansion of the gas base as the magma rises up to shallow depths, say on the order of a kilometer or so, that governs a lot of the eruption characteristics that we're trying to understand.

And these, in effect, are mass flow characteristics as well. So, we have to kind of give that language between volatile contents and the mass flow characteristics.

And, in addition, some of the uncertainties that we're going to have to address include the crustal properties, things on the order of the elastic properties of the rock as it goes down deeper into the crust, as well as the distribution of stress within the crust, not just at the surface, but with increase.

Next slide, please. Okay, the basaltic magmas that we see out here in the Yucca Mountain region are characterized by about three to five percent of larger crystals of olivine to small amounts of the mineral amphibole.

There's a little bit of the mineral called plagioclase and pyroxene. We're not going to worry about that at the moment. Amphibole is a hydra-silicate mineral.

It is unusual in basalts, but does occur in the basalts in the Yucca Mountain region. The reason we are concerned about amphibole is it's a mineral that has water in the crystal lattice.

So, obviously, if you have a hydrated mineral in this basalt, you have to have some activity of water in order to format the mineral in the first place.

Previously we had looked at some experiments that were done in basalts that weren't directly related to Yucca Mountain, but had some analog characteristics to the basalts that we see in Yucca Mountain.

Based on those analog experiments we would say that you would need probably greater than two weight percent water in order to crystallize this amphibole mineral you can see in the picture in the basalts in the Yucca Mountain region.

But, it was a very loose correlation, and we didn't have a good sense for two percent, three percent, etcetera, for these specific basalts. About two years ago, Jim Luhr of the Smithsonian Institution and Tom Housh at UT Austin had done some measurements with a very high powered microscope, as well as some work where they take a look at inclusions in the mineral.

When these minerals form, they trap glass in part of the -- when the basalt cools, turns into a glass. That trapped glass traps the amount of magma and dissolved volatiles in it at the time of crystallization.

So, we're capturing processes that are fairly even in the crust. When Luhr and Housh analyzed these glass inclusions, what they found was, for the ones that hadn't leaked, there was anywhere from three and a half to four and a half weight percent water in these cracked melt inclusions in the minerals in places like Little Cone in the Yucca Mountain region.

They also were able to measure anywhere from 600 and 900 parts per million of a dissolved carbon dioxide in these math conclusions as well. So, this is the first really direct measurements that we had that looked at what did the volatile contents be in Yucca Mountain type results.

And they're a bit higher than the two weight percent estimate that we had from the experimental analogs. Next slide, please. We were really lucky that this year there some experiments that were published in the Journal of Geology by Nicholis and Rutherford, where they had gone out and evaluated some direct experiments, all basalts collected at Lathrop Wells volcano and Little Cone volcano.

Essentially they are the same basalt composition. Now, what we see is, at Little Cones, they have done these experiments. Let me just take a moment to explain this diagram.

What we see on the lower axis is temperature increasing from left to right, and pressure increasing from the base up to the top. Now, this pressure is the total pressure in the experimental system.

They've also added water into this experimental system. So, the total amount of water, the pressure of that water is equal to the total pressure in the system.

For a comparison, these pressures here

from anywhere from 60 to 220 megapascal, corresponds to dents in the crust about five kilometers deep down here at 100 megapascal, up to roughly ten kilometers deep at 220 megapascal.

So, we're still dealing in the brittle crust, down there around ten kilometers or so beneath the Yucca Mountain region. What these experiments are showing is that, in order to form the mineral amphibole, which is occurring in this shaded area right here, and olivine, you need to have water contents of about four weight percent.

In other words, you have to have pressures with total water pressure of about eight kilometers depth in order to stabilize that amphibole at about 980 degrees centigrade, because this is the mineral assemblage that we see at little cone.

It is olivine with a bit of amphibole. Now, if we only have three weight percent water in the melt, that saturation would be here at about five kilometers depth.

Three weight percent would correspond to about 100 megapascal of pressure. If we have three weight percent water, what we would see as that magma cools, is first olivine, then plagioclase, and then this mineral pyroxene would come in.

And, only have you crystallized a large amount of plagioclase and pyroxene in addition to the olivine, would you being to see the amphibole. The fact that we consistently see olivine plus or minus amphibole and don't see any plagioclase in pyroxene, is telling us that the water pressures had to be high, and on that order of four weight percent water to get the observed mineral assemblies.

So, we have all the lines of evidence from these direct experiments on Yucca Mountain basalt, as well as the glass inclusions in Yucca Mountain basalt.

They are saying, in general, we are looking at four weight percent dissolved water when the magmas were down at a depth of about 10 kilometers.

This is a really important number when we talk about numerical modeling of the eruption processes, because we don't have four weight percent water in these magmas. Gas bubbles begin form at about eight kilometers depth.

When you start rising that magma up, whether it's simple solubility models, simple bubble models, you begin to form bubbles when that magma is about eight kilometers below the ground.

By the time you get to within one

kilometer depth, under an equilibrium sort of descent, when you're at one kilometer with this amount of water in the meld, you've got a very large volume of gas, something on the order of 70 percent of the total volume of material that's rising up, is going to be the gas phase.

You have so much gas, that you're beginning to break apart the magma in a very simple way of looking at things. In contrast, if you had only a couple of weight percent water, you wouldn't have a lot of bubbles, maybe 40 percent bubbles, by the time you got below one kilometer.

So, this difference in volatile content or water content makes a real important distinction in how you model a gas magma mixture at depths that correspond to the potential repository.

Next slide. Okay, I've been going on and one about volatiles. I need to shift gears now on how we're going to evaluate the potential magma-repository interactions.

I think the easiest way to do this is start with the simplest models. When we evaluate experimental analogs in fluids that lack volatiles, -- sorry about that, but that's just the easier way of explaining it.

What we're using at first is an experimental system that looks at potential interactions between a volatile-absent magma -- in this case a volatile-absent golden syrup fluid -- interacting with a subsurface drift.

What we're using is a golden syrup, essentially a sucrose syrup that has viscosities on the order of one to 100 pascals. A real plain English way of looking at that is it's kind of like a stiff syrup all the way down to a very weak syrup.

So, these are very sticky kinds of syrups. What we're doing is we're setting up an experiment lab where we have a reservoir just off the plain of the pitcher.

That reservoir is under pressure, and we have a pressure in this horizontal tube, and a little gate down here. So, we have the fluid come up into this vertical cell, which simulates a dike, a gate, and then a different pressure in this drift.

We open up the gate and wash the fluid response across different pressure gradients between the reservoir system and the tunnel system, and also for different viscosities.

And we could go into a lot of these experiments. They are documented in a report that's available from the Center by Lejeune et al, 2002. But, basically, the goal of all this work is to develop a numerical model on how volatile-absent fluids could flow into a potentially intersected drift and scale those experiments to repository conditions, and try to get a first-order feel for the flow rates and kinds of flow that you would see for volatile-absent flow.

This lower slide just shows a very simplified simulation where you have a different pressure to measure and measure the flow front through time.

The shape of the flow front is going to change depending on viscosity and the pressure gradient, and whether or not you develop a gravitational front on this so it looks more like a lava flow, versus more of a pressure driven flow, that doesn't have a gravitation front to it.

This could affect our understanding of how this would interact with structures in the potential drift. Next slide, please. One of the things we see from these experiments is that the viscous drag in the dike system is a much more important process than any sort of drag effects in the tunnel.

So, the dissipation of pressure in a magma system is really going to be controlled by friction in the dike, not really tunnel interaction effects. The controlling process that we have to worry about is the pressure gradient, which we need the pressurized fluid that's rising in this simulate the dike system, and, of course, the drift system pressure.

What we're looking at here are a series of open tunnel experiments where we're allowing the end of the experimental apparatus to be opened at the sphere.

So, we're not getting any air compression effects as the syrup flows in the tube system. And this could be similar to a permeable drift wall scenario where you have a lot of gas escape as magma would flow into the open system.

What we're seeing is that the flow velocities that you would expect in these simulations really is controlled by the overpressure in the magma system.

And here we're looking at pressures of five megapascal pressure drops, with the drift and the dike, up to ten megapascal pressure drops, and seeing of change in flow velocity on that order of eight to twelve meters per second.

A real simple way of thinking of these

experiments is, if we have volatile-absent model coming up into this potential drift system, and we have a pressure differential that corresponds to lithostatic to maybe five megapascal over hydrostatic or lithostatic in this system.

We should expect to see magma flowing into the drifts on that order of around ten meters per second, which isn't a huge flow rate. But, again, we have to remember that this is for a volatile-absent flow.

Next slide, please. We also have known some numerical models on volatile-rich interactions in each trip. And I know there's been a lot of discussion of these models.

Some of them are documented in the Woods et al, 2002 reports. But, basically, we looked at a very simplified model for initial magma repository interactions.

Some of the simplifications you get with this first order model, assuming we had no gas loss in the magma system, that the dike would instantly open into the drift, there were no elastic effects, there was no feedback between magma pressure and wall opening, and also that we have a closed drift, no gas loss, and the drift was completely smooth.

The important point of these experiments or these numerical models is that, if you optimize the system for rapid decompression, you would get a flow acceleration on the order of about 100 meters per second and potentially generate some sort of a shock as that accelerated flow reacted with the end of the drift wall and started to bounce back.

The important point is not whether or not a shock would develop, but, by using these optimized modeling conditions, the magnitude of that shock is still very low compared to the strength of a waste package.

So, we are not worried about transient overpressures from potential initial interactions between bubble bearing magma and an open drift. One of the concerns that arose from these sort of models, though, was the potential to generate fractures along the drift, especially at the end of the drift where we would have reflection phenomena, because, at this depth of about 300 meters below ground, it only takes about five megapascal to hydrofracture the rock at this depth.

So, the question was, even though we weren't disrupting the waste package, couldn't we be creating fractures during this initial stage of interaction that would then be exploited at some time by rising magma during the course of eruption.

Next slide, please. So, again, even though we're not looking at this happening initially, of the magma shooting out through a fracture, we still have to consider that, although we have more likely scenario where the rising magma continues along a plane of vertical ascent, there still appears to be a possibility of exploitation, all these secondary fractures at some time during an eruption.

Now, the reason we're concerned about this, and why we still have residual uncertainty about this alternative flow path, is that we know, from the numerical modeling that we've done, as well as the modeling of others, in shallow magma systems, you can have variations in the overpressure and underpressure that occurs within the conduit system.

This is, again, just a very simple numerical model in the Woods et al, 2001 paper that shows more steady state conduit assumptions. And, by the way, this assumes a rigid conduit wall.

There are no elastic effects built in on this conduit. You can see that, in these assumptions, the conduit would be under pressure until we got to around 300, 200 meters below ground surface.

And then, because of choking effects in the event, we would have some overpressure in the system on the order of several megapascals. Again, this is just a snapshot representation of a moment in time of an eruption.

During the eruption, because of variations in mass flow, and variations in conduit response these over and under pressures can be much more dynamic than you get from a standing model.

And, again, we're trying to look at, not just in any one instant of time, but what could happen, what needs to be considered in our risk assessments for the duration of an igneous event?

And one of the reasons we're still looking at this as a potential concern is that, in some cinder cone volcanoes, we see these secondary breakouts occurring at various stages during an eruption.

Next slide, please. Here's two examples of these secondary breakouts. We volcanologists commonly call them boccas. One of these that we're looking down on Paricutin Volcano, this is a big -- from Luhr and Simkin's book on Paricutin.

We're looking down from the air on Paricutin Volcano. And what we're seeing are these series of fractures that went from the southwest to the northeast, that represent some of the intial fracturing at Paricutin.

The main volcano localized, the main cinder cone formed at Paricutin right through here. But, during the course of the eruption, we have a secondary breakout occur, one of these boccas occur along the plain of initial intersection.

And this vent was active for only part of the eruption, but still effused lava and had an effect on the eruption characteristics for some time.

These are the most common kinds of breakouts that you see at these volcanoes, ones that are along the initial plane of intersection. They are not ubiquitous.

But, certainly, it's very common to find, along the direction of dike propagation, these sort of secondary breakouts. Another example is shown on the right, a very simplified geologic map of the cinder cone eruption in Russia in 1975.

We're seeing three main cinder cones that formed along a generally north, north-east trending fissure system. And there's a number of these boccas, the diffused lavas, that are localized pretty much on trend with this main fissure system.

These are not what we're concerned about.

We're concerned about these sort of secondary breakouts that occurred over a kilometer and a half from the plane of initial intersection.

I don't think any of us really understand the details of how the magma system is forming these breakouts from the main magma system. But, it does appear that, some time in some eruptions, you get these lateral breakouts from the system, away from the plane of initial intersection.

So we need, in our conceptual models, in our uncertainty analyses, to consider the likelihood of this kind of a condition occurring at the potential repository site.

Okay, next slide. What I'd like to do now is just provide a real quick summary of some of the newer work that we've been doing on modeling magma flow in elastic wall conduits.

Now, the results of these analyses are in the woods et al 2004 report that I believe was distributed before the meeting. Again we're starting off with building on previous models in which we've used rigid conduit walls.

And one of the criticisms and concerns that we had received on those models is that there really was no linkage between pressure in the system and wall rock response.

Because we can model some of this wall rock response in the simple elastic process, we know there's going to be deformation on the wall rock as the magma rises through time, and also as the volcanic conduit responds to pressure variations through time.

So we need to develop some sort of a basis to evaluate how this feedback between elastic properties of the rock and fluid pressure in the system can affect the eruption characteristics.

In here, we're using the base assumption I think is very generally accepted that fractures dilate from magma pressures that are going to be greater than the minimum principle horizontal struts.

So this model is going to be sensitive to the assumptions -- its stress ratio, in other words minimum stress to maximum stress in the horizontal domain.

What we're doing is assuming a two dimensional elastic conduit wall, and allowing that pressure to have a feedback in the elastic response of the conduit with the total pressure in the system.

These are essentially basic flow dynamic models that use mass in the land of conservation to evaluate this response with some different assumptions regarding magma pressure, horizontal structure ratio, and also taking a look at variations in volume content.

Next slide. For Yucca Mountain type ascent rates and magma viscosities, and dike geometries, we're ending up with buoyancy driven flows that have fairly low Reynold's numbers.

These are not very turbulent forms. We see Reynold's numbers on that order of two to 20. So we can make a number of simplifying assumptions in numerical models.

What we see in the model, and again these are models in the Woods et al 2004 paper, where the conduit width is going to be controlled by the difference between the pressure in the magma and what we're calling simply the pressure in the rock.

Where this rock pressure integrates the density variations, the stress ratios, and the elastic properties in the rock as well, we're seeing that the viscous drag in the system is much more important than the turbulent drag.

But again, this is only for the model with poor magma conditions. And what we're doing in this example is looking at a stress ratio of .7, in other words the minimum horizontal stress is 70 percent of the maximum horizontal stress.

And a magma viscosity on the order of 100 pascal seconds, which we think is pretty representative of the bubble-absent, crystal-poor salt in Yucca Mountain region.

And for these different mass flow rates, and the units here are meters squared per second, that's flow rate per unit length of a dike, anywhere from .3 to about 10 meters per second along a meter of dike.

We're seeing an opening in that dike from that depth around two and a half, three meters wide, narrowing up to on order of one, one and -- or one to a half meter wide as we approach the surface.

As the pressure in the system is dissipated by frictional losses against the dike as well as the variation in the duction in magmatic pressure as we increase distance away from the source region.

Now we're assuming in these calculations that the viscosity remains constant. We know as we bring volatiles out of solution and have a little bit of cooling to the magma, we can be varying the viscosity.

And also, as we have bubbles come and

appear in the magma, the viscosity is much more dynamic than the assumptions that were made. Again, we're not trying to realistically model with this stage the full range of magma set processes, but gain some understanding of how feedback between the wall rock and the magma system can affect flow processes in the shallow sub-surface.

Next slide please. And the reason this model becomes more important is when we starting adding bubbles into the volume. We have the volatiles in there. We model the exsolution of gas and start to account for the compressibility affects of the magma.

When you don't have bubbles in the magma, you don't have the volatile phase, that magma is very difficult to compress. It's essentially a big compressible fluid.

So its density doesn't change in compression. But once you start adding bubbles in to the system, it's much more sensitive to the density with pressure variations because a porous gas can be compressed much more easily than a fluid.

Here, when we add this volatile phase into the magma, again we're assuming the constant viscosity, we have to account for laminar, as well as the turbulent drag effects by having this volcanic fluid.

But we're still modeling this as a single phase flow. We're not trying to get into modeling different velocities between the gas phase and the magma phase.

So again, it is a simplified model, but getting into two dimensional, or two phase flow is a much more complicated step. Within those limits though, what we're seeing is that when this magma system is allowed to reach the surface and vent, we impose a condition of choke flow.

In other words, the flow velocities can't exceed the local velocity of the speed of sound at the vent. It's a common modeling assumption in volcanic processes.

Because of these choke conditions; we end up very simply getting an overpressure in the system that inhibits gas exsolution. So because of accounting for these compressibility and pressure effects, we end up suppressing a lot of the bubble growth and gas exsolution in the shallow subsurface.

That gives us a very different understanding of how bubbly these mixtures would be under a steady choke flow condition. Then if we made just a simple equilibrium ascent like I was talking earlier, here in this model again from Woods et al, we're seeing depending on the assumptions you make in a set velocity you'd have anywhere from about ten to 20 percent bubble fracture, in this case void fracture in the melt, once you fully realize the choke conditions and elastic effects in the conduit model.

For comparison if you didn't account for those effects, at 300 meters depth you'd have over 70 percent void fractions for a simple lithostatic pressure model, again using about two weight percent volatiles in the melt.

So this gives us a very different understanding of the kind of decompression effects that we may have to consider if we talk about decompression at 300 meters into an atmospherically pressure filled drift.

Next slide please. Now we haven't just done these simple flow models. A terrible effort's been gone -- has been undertaken by O. Bokhove and his colleagues at the university.

He used some non-linear invective diffusion equations. At the stage that I can record we've only been using non-compressible flows for these models, but the advantage is by using these non-linear equations, we can evaluate non-steady flow.

In real simple language terms, is that we can look at the variations in these flow processes through time, rather than previous models which are pretty much a snapshot in time.

We're looking at a single equilibrium sort of ascent, or equilibrium ascent and flow in the Woods et al models. And here we're using a full dynamic realization of the evolution of the system through time.

One of the reasons -- one of the ways we can gain some confidence in this approach is take our non-linear equations and set it to a steady flow condition.

In other words, make it kind of similar to the model I was just showing. When we make a steady flow assumption, we end up getting the same sort of variations in dike width with depth that we saw in the Woods et al 2004 model.

So, very simply, this alternative approach using the invective diffusive equations when we set it to the same flow conditions of steady flow gets you the same basic dike width depth relationships that you get from an alternative approach using the maximum interval versions to the Woods et al.

Now the value isn't in duplicating this

figure, but in the next slide. On the left hand side we're looking at a time history of how dike width would evolve from a reservoir located simply three kilometers away from the surface.

Each one of these lines represents a time step that's about three minutes in the simulation. And so depth is increasing from these meters below a surface to up to the surface here.

And dike width is going from one meter wide down to about a tenth of a meter wide. So, in the first step in the simulation at the reservoir, the dike would be around .7 meters wide, with decreasing depth, the dike gets narrower and narrower.

As the simulation progresses through time, in other words if the magma is rising, the dike width increases near the reservoir but also increases with decreasing depth.

Until finally you get an equilibrium condition right before it vents to the surface, to where you get a small variation from .9 to .8 meters width as the dike goes from three kilometers up to the surface.

Now, again, we're not using any eruption at the surface in this first simulation. And it's a non-compressible, non-volatile bearing basaltic magma.

The minimum stress assumption that we're using is that the minimum stress is again 70 percent of the maximum horizontal stress for the elastic response in lava rock.

On the right-hand figure, we're taking the same basic model in black and then allowing it -- excuse me, the same basic model in the red dash lines, and then allowing the dike to vent to the surface, and allow conditions of flow.

And what we see from -- faintly see the red lines that correspond to this figure here, but once break out occurs into the surface, there's this drop in dike width, and then dike width reappear -- re-widens until it becomes a steady condition of flow out to about a meter to about a meter to a meter and a half wide as flow to the surface is established.

So we're seeing a nice dynamic realization here for the non-volatile barium melt that shows that once we have eruption at the surface we get a pressure drop that transmits through the surface, the dike closes and then reopens, but forms a conduit that's going to be wider than a condition for no flow.

Next slide. So what did we learn from all this modeling? A lot of things, but putting at the top level points, we're seeing that models are pretty sensitive to our assumptions on magma viscosity, volatile contents, and our assumptions of minimum to maximum horizontal stress.

Even though the model's apparently sensitive to the assumptions, I think we have a pretty good technical basic to evaluate the range of uncertainty in these different parameters, and evaluate the sensitivity of those parameters and models.

One of the things is that flow choking at the vent can cause pressure variations in the magma system that really affect the characteristics of the gas phase.

So, when we look at the course of an eruption and try to simulate potential interaction processes for the duration of the event, we need to consider feedback between choke conditions at the vent and flow conditions in the sub-surface, and how those conditions of flow in the vent can influence bubble evolution and gas evolution back through the continuous magma system, not just at the surface but down into the sub-surface as well.

What we're seeing from the models is that the model eruption rates for a one kilometer long dike, which are on that order of 100 to 3,000 cubic meters per second, those eruption rates correlate pretty well with the eruption rates that you'd measure historical basaltic cinder cone eruptions, which again are on that order of 100 to 1,000 cubic meters per second.

So the mass flow relationships that we're using in the simplified model -- excuse me, we're deriving in the simplified models -- to the first order correspond to the kind of mass flow relationships that we see at typical basaltic scoria cone eruptions.

And finally what we're beginning to understand is that the model dike or conduit system can respond to changes in pressure on the order of minutes to hours.

So when we start talking about dynamic pressure variations in the course of an eruption, we can be looking at pressure variations that are transmitted through the magma system with velocities on the order of -- excuse me, on time periods of order of minutes to hours of response throughout the system, to these overpressure and underpressure conditions related to potential interaction processes.

So to conclude, as a summary of our

current information, we see that the available information is supporting that water contents on the order of four weight percent appear characteristic of the one million year old and younger basaltic magmas in the Yucca Mountain region.

If the rising basaltic magma also intersects non-backfilled drifts in the potential repository, that magma may flow into the drifts on that order of a hundred to ten meters per second.

Again these aren't fast enough velocities to report any chemical damage, we think, to the waste package, but represents a fairly rapid infilling of flow into the drift system on the scale of minutes to hours.

Continued vertical ascent following potential interaction appears to be the more likely scenario following intersection with the drifts. So we think that the available information would favor continued ascent along the plane of initial intersection during the early stages of the eruption.

However, we're still concerned about the development of these additional breakouts which sometimes are referred to as doglegs that may occur for short periods during an eruption the same way that we see these breakouts occur in some basaltic scoria cone eruptions that have been observed historically.

And finally I don't want to leave you with the impression that these are completed models. There is still a lot of work that is ongoing to evaluate specific conditions and uncertainties that are appropriate for the potential repository site.

We have that work or unfortunately have not gotten it to the stage of reports that are in public domain just yet. So with that I'd like to thank you again for listening to a fairly raspy voice, and open it up for discussion.

CHAIRMAN RYAN: Thank you Britt. Any questions from members? Allen? Okay.

MEMBER CROFF: I'm going to take a try at a question and I'm not sure I can articulate it very well, but given the statement earlier this morning that the NRC was assuming that there would be waste package failures from you know when magma interacts with the package, I'm sort of struggling to relate what you've said you know sort of this relatively abstract modeling of magma flow to the conclusion that the packages will fail, and that radionuclides will presumably be released to the magma.

Can you or anybody sort of elaborate a little more on the underpinnings of this assumption which seems to be fairly critical?

MR. HILL: Okay. This isn't really an assumption. There is a lot of the work that underlies that basis has been presented in like the 1999 issue resolution status report.

It considers the -- well let me back up for a minute. We had two scenarios that we have to make sure we're talking about. One is for the waste packages that would remain in a potentially intersected drift. And we have waste packages that would be entrained in the erupting conduit.

Part of the situation that we have to consider is that we're not instantly developing the conduit and throwing a waste package into the erupting volcano.

These conduits that we've interpreted from a lot of geologic information, some of this work again is documented in a publication by -- conduits open through time gradually.

So we have a scenario where the rising magma intersects a drift, it goes up to the surface, and then in the course of days to potentially weeks the drift -- the dike opens from a one meter wide conduit to essentially a cylindrical conduit that could be on the order of meters in diameter to tens of meters in diameter, that widens gradually through time.

But while it is widening you still have waste packages in the intersected drift that are exposed to the molten magma, or the thermal effects form that magma.

So first we have to talk about incorporation into an eruption conduit of a waste package that is at essentially magmatic temperatures.

One of the things that we've been frustrated by is that there have been bits and pieces of mechanical analysis but we haven't really been able to develop the full mechanical analysis of waste package response to the range of conditions that would occur in a potential igneous event.

For example, we know that there's going to be gas overpressure in a system as in the waste package as the temperature rises up to and over 1,000 degrees C.

We also know that the C22 -- out of the way, and the stainless steel innerpack have different thermal expansivity. This innerpack is 30 percent more expansive than the outerpack.

We have done some sculpting analysis that show that for the small gap -- and don't quote me but

I think it's on order of half a millimeter between the innerpack and the outerpack, that's not enough to accommodate different thermal expansion between the stainless steel and C22.

So, as it comes up the temperature, you've got differential expansion and significant radial and hoop stress on the waste package itself. So, by the time we talk about a waste package potentially getting to see the vertically erupting conduit, it is already at temperature and has a significant material stress from internal pressurization from both gas expansion as well as differential expansion in the alloys.

We're then taking that material and putting it into a very complex pressure regime, where material is flowing by anywhere on the order of tens of meters per second to 100 meters per second under this dynamic pressure variation.

Our waste package people -- and I'm not going to speak for anybody but our waste people at this stage -- have concluded that given the kinds of mass that we have in an eruption volcano, and the condition of the waste package at the time of incorporation, that we would not see resiliency of the waste package when it is thrown into the conduit of an active erupting volcano.

That's been documented in, for example, our issue resolution status report in 1999.

MEMBER CROFF: So I should take from your talk that that part of it is assumed, and you're trying to better understand the rate at which the conduit forms, widens and intersects waste packages?

MR. HILL: We're trying to understand, yes. Some of the -- ultimately we want to understand better the mechanics of magma flow in a conduit when we have this drift system in the subsurface.

There is a number of effects that we have to consider that we never really have considered before in volcanology, because we're having this horizontal tube full of a volume of magma that is a bubbly magma.

Where are the bubbles going to go in this tube? We're going to have segregation of the gas phase and the liquid phase, and the possibility of return parameters.

In addition, we have this unusual geometry, and unusual stress distribution, that you normally wouldn't see around a volcanic conduit where you've got this drift system sitting here.

We need to understand at a better level the uncertainties in the mechanical response in rock, and of the conduit given this perturbation in the system, because we can't use a simple analog which doesn't exist.

So the first part of your question is this has been a long standing series of analyses and information that's been in the issue resolution status report, and in many of the performance assessments.

It's not a new assumption in that sense. And second, the reason that we're looking at these sort of flow models is really to understand this perturbation in the system that arises from the presence of the engineered system in the subsurface, not to better understand the volcanoes themselves.

MEMBER CROFF: Does the experience from a reactor accident analysis and the experiments that offer any insights for the two phase flow in this case?

MR. HILL: I don't really think it's analogous for the two phase flow dynamics. There is a good body of literature and a lot of work that's gone on to understanding volcanic eruptions.

One of the reasons we're working with the consultants that we are --Anne Woods and Steve Sparks in particular - is they're some of the worlds leader in understand mechanics of eruption dynamics, fluid dynamics of molten rock systems.

It's more a matter of trying -- a lot of the uncertainty really is trying to understand what's going on in the subsurface from very indirect evidence.

You know, we can't really observe physical conditions in the eruption volcano. The eruption products that we see have been highly modified by the time we see them from their condition in the subsurface.

So some of the work, some of the modeling that was initially developed to understand volcanic flow processes, did arrive from basic fuel cooling and basic thermal fluid dynamic relationships.

But we're not trying to derive from first principles these models. They're already from a fairly established volume of literature.

CHAIRMAN RYAN: I appreciate the modeling effort you have underway to improve the modeling of magma, but I'm sitting here thinking of the question how did any of these variations of your modeling impact your basic assumption, which was the packages entrained, and if I understood John Trapp earlier correctly, that the package offers no confinement or containment so all the radioactivity is in the magma?

MR. HILL: There are two -- three ways that this affects the source for the igneous scenarios. I can't demonstrate this yet because we haven't finished the modeling.

But the potential here is to understand conduit widening processes in this disturbed geologic settling -- excuse me, disturbed geologic setting.

Right now we're making an assumption that the diameter of the volcanic conduit is completely unaffected by the presence of repository grips. We want to understand the stress distribution and flow response in this disturbed regime to say whether or not that assumption is supportable, or should have a larger variation of uncertainty because of these conduit drift interaction processes.

In other words could the conduit be larger or elongated, or have a larger source-term for volcanic eruption because of these flow interaction processes that we're currently assuming?

CHAIRMAN RYAN: Let me ask you to follow up on that point. If all the radioactive material in a package or a number of packages is entrained, isn't the source-term constant?

The concentration will vary based on how much magma you have but the amount of -- that's involved is cut down constant.

MR. HILL: For one waste package.

CHAIRMAN RYAN: Well for any -- pick a number I mean one, ten or 50. But what he's saying is that one package or ten. Okay. I think I'm understanding a little better.

MR. HILL: All right. I'm sorry I didn't make that clearer.

CHAIRMAN RYAN: Okay.

MR. HILL: That as the conduit widens, the number of waste packages intersected would also increase.

CHAIRMAN RYAN: Okay. And then of course there's the complicating feature of is it entrained or is it sequestered in the end of some tunnel or something. That's what I'd imagine.

MR. HILL: That was the second part of this story, of the three part story.

CHAIRMAN RYAN: Okay.

MR. HILL: Is we have -- we're talking about the direct volcanic release but we also have what was called the indirect release scenario. Where all the waste packages that remain in the drift.

Now we want to understand a better mechanical approach to how these igneous events, for the duration of event and afterwards, can affect the waste package performance.

We believe there is sufficient information to show that as the magma was in place, and cooled, and the stresses involved, would cause breeching of the waste package for these waste packages that are left within the drift.

By understanding the variations in pressure through time and temperature through time, we have a much better mechanistic basis to evaluate potential waste packages response to the physical conditions of magnetism, and evaluate the conservatism or non-conservatism of the assumption of damage extent in an intersected drift, as well as waste form behavior following a potential intrusive event.

So even though we're not getting those damaged waste packages out during the igneous event, we're still following the even at the resumption of normal hydrologic flow and transport.

And so the scenario here, the risk significance, is that the conditions don't cause waste package failure. Then we don't have a large number of waste packages fail following an igneous event, and there's no increase in the hydrologic source-term.

Conversely, if we're intersecting a number of drifts, and the contact of magma with the waste package is sufficient to cause failure of the waste packages in those drifts, we have a large source-term that has to be considered in performance assessment given the condition of the igneous event.

CHAIRMAN RYAN: I guess one friendly amendment I'd ask you to think about is that it's really not a source-term yet. It's an available inventory.

MR. HILL: Okay. I'm using that --

CHAIRMAN RYAN: Are we on the same page here?

MR. HILL: -- loosely. There are many steps to go between disruption --

CHAIRMAN RYAN: Okay.

MR. BRITT HILL: -- or potential disruption of a package, and the release mechanism.

CHAIRMAN RYAN: I just wanted to make that point that's fine. And the third one?

MR. BRITT HILL: The third one was these horizontal doglegs and breakouts. We have it as an alternative hypothesis in the Woods et al 2002 paper.

I think it's fair to say that we are less concerned about that condition occurring during the initial stage of the event, but still need to think out, and get a good basis for evaluating, developing these breakouts at any time during an eruption.

And, of course, if we had a horizontal flow path away from our existing conduit, that could also entrain and potentially eject more waste packages than we're currently assuming in the performance assessment calculations.

So there's the three risk significant impacts for the work that we're doing in this area.

MR. GARRICK: I just wanted to follow up what Allen Croff said about the -- and the accident aggression analysis --

CHAIRMAN RYAN: Turn that up please.

MR. GARRICK: That'd be a good ides. I wanted to follow up Allen Croff's question as to whether or not the technologies that have developed in accident compression analysis of reactors have any impact on the source-term development, particularly with respect to entrainment, because there have been enormous amount of work gotten done in this area, and in some cases some major surprises as to the confinement capability of the debris.

And it seems to me that this could have an impact on the form and of the material that eventually is in the cloud. And I just was curious, I wanted to press on that point a little bit, has this technology been examined at all?

MR. HILL: That's one of the areas of ongoing investigation. I'm hoping we're going to get some insights today or tomorrow from some of the other presentations, but I'm afraid that it's not at the stage that I can really comment or report on.

We haven't had any major breakthroughs in that area, and one of the major limitations is trying to relate the physical conditions of those scenarios with the physical conditions of an igneous event.

They're not very comparable, but still trying to look at the -- how difference in physical conditions may or may not affect our understanding. It's not very straight forward.

And that's why we haven't been able to make a lot of rapid process in that area. But certainly this was something that Dr. Weiner had mentioned earlier in the year.

We have been following up on that area all -- a lot of other areas. I'm just afraid at this stage it hasn't come to fruitition.

CHAIRMAN RYAN: John, do you want to make a comment?

MR. TRAPP: Just one comment. I did state this morning that yes, we are making this assumption on a waste package. Now, in order to understand how a waste package is going to respond to this type of environment, you have to better understand the environment.

This is what this work is doing. It's getting us a better handle of the mechanical thermal environment than we have to put the waste package in.

And therefore there may be a possibility that there could be some say to use Dr. Garrick's terminology, a more realistic model that maybe does say that some of a package can survive.

But unless you understand what is really happening form a mechanical and thermal response in a volcano you're just -- and there's more.

CHAIRMAN RYAN: And I have been looking at the volcanology and flow of magma in it's purest form to understand that. I appreciate that, but at the end of the day it is important only in the context of a waste package and its interaction with it, and then what happens down the line.

And again I apologize for jumping ahead a bit, but I'm trying to keep a whole range of parts and pieces of this question in my head at the same time. I appreciate your comment. Ruth?

MEMBER WEINER: Well I have -- should I use the mic? I have a couple of possibly unrelated questions. First one is, your talk is titled NRC Review Capabilities for Evaluation Potential Magma Repository Interactions.

Could you expand a little bit on how you're going to use this model to review what DOE has done? Are you going to say -- well I'll let you respond to that question.

MR. HILL: All of us are faced with an extraordinary challenge in trying to evaluate this process. It's unprecedented in trying to look at the interactions between a volcanic system and an integrated system.

We have no good natural analogs, we have no objective basis of comparison to use the part 63 terminology. So really about the only way that we can try to look at eventually the risk significance of this is by doing some of this actual work.

It's very typical to review something that's state of the art if you're not actually doing some things that are kind of state of the art. We don't use this modeling as the correct way.

This is one insight of many possible ways to approach this problem, but it does give us a knowledge base to kind of understand in doing this model, what's important, what's not important to process level, and how this would affect our understanding of the downstream risk impacts for this challenging problem.

But again I want to emphasize, we're not viewing this as setting the baseline for a comparison of whether we're right or wrong or any other group is right or not. Is that answering --

MEMBER WEINER: It does.

MR. HILL: Indirectly?

MEMBER WEINER: That's a partial answer, and one of my questions was, well suppose DOE comes up with an entirely different model, are you going to say well ours is right and yours is not or ours is not and yours is?

MR. BRITT HILL: Well --

MEMBER WEINER: But --

MEMBER WEINER: -- I would just very speculatively say if -- there's the two conditions. DOE comes up with a model for magma flow, and it completely disagrees with our model, and we have a completely different risk insight from it.

Were going to have a challenge in evaluating who is right in that sense, or how we're going go. And it may be we just have to let the licensing process take care of that.

But, conversely, we have alternative modeling approaches, and they could be very close in approach. We could be getting the same basic insights.

And I think that would be a useful thing. To -- we have no proof. We have no basis to say it's right or wrong, unlike some other things. So we've done an independent effort. The department has done an independent effort.

And all these answers appear to be conversing in about the same risk difference. And we can evaluate the differences, and they could be insignificant.

I think we've all done the best job we can in that case. So I'm going to hope that we're going to be successful in this. Unfortunately I can't comment on ongoing reviews.

But I'm optimistic that this approach that we're that using is not completely out in left field. And let me leave it at that.

MEMBER WEINER: Thank you. That's a very comprehensive answer. My totally unrelated question is, your slide six has a rather elegant experiment analog, elegant in its simplicity.

And I commend you for that. Is there any way to take your experimental setup and add analog waste packages to it? You would face some difficulty in scaling the scale up.

MR. HILL: Yes.

MEMBER WEINER: But is there some way that you cam do that because it seems to me that would give you some insight at least into the pressures and gas bubble interactions.

MR. HILL: This apparatus is designed to look at the initial fold. I think the state of fluid modeling is sufficient to show that, given these conditions of flow for simple geometry, you know we couldn't make an analog that was anything more than a simple waste package anyway.

Whether or not these conditions are sufficient to entrain or bump things around, and even -- I'll fall back on the Woods 2002 paper -- even under the conditions of optimized accelerated flow, there may be slight movement but nothing that was high enough velocity given this very simple geometry to really pick things up and move them around.

So I think we have the insights we need for the risk assessments that, with this apparatus and those conditions, we're not really concerned about low impacts on a waste package.

We do have ongoing experiments that are looking at sustained flow and circulation. And some of those will be considering experimental analogs for engineered systems, again, trying to gain insight and model verification for the simplified calculations on how these circulation effects may of may not affect material located in various parts of the analog system.

MEMBER WEINER: Is there a fluid dynamics model that could help you model entrainment better?

MR. HILL: Yes. There's a number of them.

MEMBER WEINER: Yes.

MR. BRITT HILL: And again we are doing a lot of work. This is why we're doing the basaltic work at the University of Bristol crew, who have Steve Sparks.

One of the people who's currently working with us is Dr. Jerry Philips. He's not in the current -- or presentation but he's another one of the leading experimentalists fluid dynamics people for magma repository interactions, also a parallel reference going at Cambridge University, with Professor Andrew Woods, and some of his colleagues, to really come up with solid fluid dynamic basis to evaluate these processes to the best of our ability, and without uncertainties.

MEMBER WEINER: Thank you.

CHAIRMAN RYAN: Panel members, any other questions, comments? Bill?

MR. HINZE: A couple of quickies. The ICPR, the Igneous Consequence Peer Review Panel certainly made it clear that they felt -- I think the words are most unlikely that there is a dogleg.

And I gather that your work suggests that that's not the case, that a dogleg this possible. Or are you talking -- is the difference here related to -- of preexisting zonal weakness?

How do you rationalize what you're saying with what the ICPR has come up with?

MR. HILL: I'm afraid I can't comment very much on the DOE's peer review comments. What I can say is that there have been -- we have at a subjective level, and like I said in this presentation, we don't believe that this is a likely scenario.

However, all the assessments have been very qualitative and subjective about -- well it seems less likely, but we're not -- have not received a input that says it cannot happen.

And we have to be careful in distinguishing between what may or may not occur in the initial stage of potential interactions, versus what may or may not occur for the duration of an event.

And I think it's fair to say that none of us have presented a model or an analysis that truly looks at the evolution of the system for the duration of an event.

Most of the work is focused on the initial stage of interaction. So I can't go too much farther with that.

MR. HINZE: Do we have a -- an explanation for the current boccas in nature? Is this a chocking, rocking of the main conduit? What causes this?

And how do you get at the likelihood that this may happen as you say in, in -- to carry it up?

MR. HILL: I certainly -- my personal opinion is we don't have a great understanding. But what we can see from observation is especially -- I used -- as the best example right now, just because they're isolated cinder cones.

Mass flow rates are very comparable to what we would expect in Yucca Mountain. And there were a number of boccas that formed during the course of this event.

There wasn't really a hiatus in activity in the central cone, and a shift to the bulk. What you saw was simultaneous eruption of lava and tephra.

Sometimes that eruption is occurring from the central cone itself. Other times the diffusion rate of lava may decrease slightly from the central vent, but increase at a bocca.

But it's not really a straight forward thing where you shut down the main conduit and have everything coming out of the bocca. It's a much more complex plumbing system than that.

So I'm drawing a cartoon in a cartoonish view that's a great simplification. But I'm in that intermediate position of here's what happens in nature.

This could potentially affect our risk understanding, but I know there's a lot of complexities. And it's not a one for one for one scaling relationship either. So we're working on that.

MR. HINZE: Speaking of the natural conditions, I remember forty years ago going on a field trip to Kings Fall, and the Snake River plains, and seeing the basaltic dikes which obviously had backwash.

MR. HILL: Correct.

MR. HINZE: And where are you in modeling or could you be in modeling that considers backflow and how do you approach it, and so forth?

MR. BRITT HILL: What I san say is this is part of our ongoing work at the University of Bristol. We are looking at circulation and flow effects for steady and non-steady flow conditions in a conduit system.

Now, again, it's very hard to get people to climb up to the center cone to look down when there's a hiatus in the eruption. I had a colleague in Nicaragua that did this, and he was very lucky, but he didn't see too much because you get a lot of rubble coming in.

The cinder cone conduits, it's very hard to say whether you're going to get a hiatus in the eruption, that would cause -- to rain to hundreds of meters below the surface.

It seems kind of unlikely, but I can't eliminate the possibility. But it is a very different kind of conduit system than what we would see at these rift dominated systems like Craters of the Moon, which are very Hawaiian.

And you've seen drain back and cessations of eruption as a very typical feature in those kind of systems.

MR. MELSON: Yes, but I think certainly we it should be happening here. It's one of those models that I think should be considered, at least just proven that it can happen.

MR. BRITT HILL: And I think with the ongoing work, we will be able to evaluate whether that kind of drain back phenomena would have an effect or no effect on the engineered system.

MR. HINZE: Let me ask a last question. The last bullet of your summary of current information continues to refine these models. Where are you?

How much do you have left? Do you have a feel for this at all? What are your plans?

MR. HILL: Well, what I think it's pretty obvious that we haven't presented anything that looks at the specific geometry. So, obviously we're going to be applying that through a complex geometry that would represent potential dike drift systems.

That's why the first step if, of course, makes your numerical, gain confidence in the model. Start simple, build upon that, and then apply it once we have an understanding that we're not just modeling, but modeling something in a reasonable way, and then apply that to the complex system as the final stage. That is ongoing work.

MR. HINZE: I understand how research works and how we can keep solving more problems, but if we leave this problem out and -- have a duration. Are you writing this proposal for two years, four years?

Where are we now from zero to ten, in this whole understanding of the intersection of the magma with the repository? Where are we, from zero to ten?

Give me a number and I'll give you what percent we need.

CHAIRMAN RYAN: That's a so odd question Bill?

MR. HILL: The goal is to have the work completed and of course written by December 15th.

MR. HINZE: Okay.

CHAIRMAN RYAN: Bruce, you had a question?

MR. MELSON: I sat through as a number of people did here, the Igneous Consequence Peer Review Panel, where much of it dealt with the Woods paper, which I assume was contracted by your group and what not.

And a lot of interchange went on there of a very substantive nature. I don't know if you've ever gone through the documents that came out of there or not but I'm just -- I mean it's so construct in the sense of allowing you to look at your work, and you said you couldn't talk about that.

And so I was just wondering -- because you never did mention it, as if it never even happened. But I assure you it did happen. And I'm wondering, is it a license thing or something?

Is there some legal reason you can't deal with people who are feeding what I hope is constructive criticism into your work.

MR. HILL: I thoroughly appreciate the desire to be able to have open communication and open interchange on topics all over the map. Unfortunately we are approaching a very complex legal arena.

And we have to be very sensitive on what we communicate in terms of publicly available information and the format in which we communicate that information.

It is unfortunate that we have not documented the results of our review and our thoughts of the DOE's Peer Review Panel. We have not done that.

And so, in this format, I unfortunately

can't give extemporaneous feedback on our thoughts that are not in the -- and, again, this is solely because of the approaching license application deadline, and a concern about Staff's independence, and the various roles that different groups are going to need to maintain during this complex legal proceeding.

So that is why there are very obvious gaps in this presentation. I am not commenting on our views of the current DOE models, or anybody's models. I can't, not at this stage.

CHAIRMAN RYAN: John?

MR. TRAPP: Just -- one thing I can say is there is a document that is currently in review at the Nuclear Regulatory Commission. It's called the Integrated Issues Resolution Status Report, revision two, or one.

It's the revised issue resolution status report. I think will be issued in the next couple of months.

PARTICIPANT: Sooner than that.

MR. HILL: Sooner than that. Well, we're hoping to get it out as soon as possible. That will have the staff's current view of many aspects of the DOE program, including some of the comments, and major process level concerns that came about from the peer review meeting.

So, I guess a delicate way of putting it, a lot of those comments from the DOE Peer Review have been incorporated into the latest DOE documents as well.

So, while we may not be commenting in the integrated Issues Resolution Status Report directly on the peer review conference, we will be commenting on the Department of Energy analysis and lava reports that have already incorporated those comments from the review.

So we're not just leaving these comments out of the vacuum. They will be addressed. But unfortunately I can't do that today.

CHAIRMAN RYAN: Britt, thanks for your detailed explanation of that. I think we can get a clear picture of where you are and why, so --

MR. BRITT HILL: Sorry to be --

CHAIRMAN RYAN: It's all right. John, you want to make a comment?

MR. TRAPP: I just want to carry on with what Britt was saying, because when we got into this whole thing and talked to our lawyers, what did you see?

CHAIRMAN RYAN: You know what, I'll tell you. We're really here to discuss technical issues, and the position that you're in with regard to all of that we kind of understand that --

MR. TRAPP: I just want to say that we cannot discuss a lot of these things that we'd like to. And I'm sorry, but that's --

CHAIRMAN RYAN: I think we all appreciate that very clearly. And you have both done a nice job of explaining that to us so I want you to realize that it's, from at least the Committee's viewpoint, not negative.

We understand that. So we appreciate the position you're in. Neal you had a comment?

MR. COLEMAN: Yes. Neal Coleman, ACNW staff. At the spring AGU in Montreal, I had the pleasure of hearing a lecture by Michael Menga, who's a leading authority on behavior of magmatic conduits.

I noted from your slide eight, under the assumptions in your simplified model, no gas losses. He stressed in that talk that it is very critical how much gas escapes.

It has a very key role in determining the power -- the energy of an eruption. So you -- this isn't a question it's a suggestion. You want to include that kind of information, and also recognize that the tuffs at Yucca Mountain are highly permeable.

And I've given you something to start a range of say 10 to the minus 10, to ten to the minus 12 meters squared.

MR. HILL: What's the diameter of the drift? About 20 meters square. So yes there is the permeability, we agree. But it's not completely permeable.

In relation to the area there isn't that much permeability, if you're talking about compressibility.

MR. COLEMAN: What I'm giving is --

MR. HILL: These assumptions aren't clearly stated. It is not meant to be a realistic representation of all things. Now to address your second or first comment.

We've also done some very simple calculations that anybody can do about bubble rise speeds in magma and the magma that would be ascending on the order of a tenth of a meter per second or a meter per second, in the crust.

And bubble rise speeds are orders of magnitude below the magma ascend speed. So, when you want to talk about bubbles escaping from the magma system, you have to come up with a mechanism to segregate the bubbles beyond just simple buoyant rise.

While I agree that, yes, there are gas escape effects, there isn't that much gas escape in the rising magma until you get to very shallow levels.

And again when you want to talk about a fully realistic realization and simulation of magmatic process, that's great. We all know that these are not single phase flows.

But to try to model them as two phase flows is a heck or a challenge. It takes a lot of computational resource. We're just trying to gain first order insights on this, not think that we're going to come off with a fully four-dimensionally realistic model. That's not our role.

CHAIRMAN RYAN: I hate to cut off the exchange but we are running long on this talk. So if we can perhaps finish up with perhaps, Neil, one last comment. And then I think Dr. Marsh has a question and we'll finish there.

MR. COLEMAN: There may be a lot to learn in the work that we're finishing up from a world expert on this topic.

MR. HILL: That's why we're involved in world experts.

MR. MARSH: This is very interesting, Britt. And these kind of calculations are very difficult to do in a single phase. The problem with a lot of calculations is -- in the earth for example is that -- especially in these type of calculations and all of our sciences that initial conditions are always a problem because, in general, in the earth we don't know any initial conditions.

And yet, to solve the problem we need initial conditions. And we don't know initial conditions when the earth formed. We don't know the initial conditions for how any single crystal is grown.

We don't have initial conditions for how a volcano starts out.

But -- so what do you do? Well you actually impose the problem so that the solution does not depend heavily, entirely on the initial conditions.

Now, if you go back to the conduit experiment there, your horizontal -- could you show -- Britt.

MR. HILL: I don't have control.

MR. MARSH: Okay, Bruce, could you go back to the experimental level? Six. So, for example, just to use this as an example, Britt was saying himself about how things start up.

They start off very gradually and open very small. In fact you can see arrested sills and arrested dikes in places and you can actually see way out -- a kilometer two kilometers in front of them.

It's a tiny little one centimeter or even just a millimeter crack that starts out and enlarges maybe up to a couple hundred meters or so. So, in a situation like this, you have to start the problem somewhere of course.

And so they start the problem at the nozzle, give it overpressure. And suddenly you open this thing. And so if you actually tweaked it open just very, very slowly, you would get a different solution than if you just open it up, or if you puncture a membrane or something like this.

So the initial conditions are very important. Also the pressure drop is enormously important. And you can see that the ensuing velocities that you get -- and this is a flow that has no volatiles in it.

But it has -- you know the meters per second. So in other words that flow would be in Las Vegas in a few hours, and -- from Yucca Mountain for example, the volumes would be enormous.

So how do you actually temper these things? You temper these kinds of calculations by looking at the geological record of what you actually see in terms of how fast the lava is actually -- emerge, how fast the flow field gets larger and larger.

And I think, for example, Lathrop Wells and things like that you actually have some control over looking at how fast these things advanced. When you actually use two faces flow and tephra shooting out it's a little bit different.

But the other factor -- another assumption of course is they all start out at one meter wide -- dike in the calculations and you more from that. Now that's an initial condition assumption also.

The other thing is that there's no solidification whatsoever in depressurizing. The volatile is coming out of solution. What is does it changes the entire phase.

If you want to go up to the phase diagram -- five. Let's go to five. So this phase diagram for example, up in the left hand, the dashed or shaded area there, if the magma starts in that region it starts to ascend to the surface.

So you have pressure on the left, so it has to some to the surface. And so, what happens is it can't hold its -- a magma's like a diver, it gets the bends. This stuff comes out of solution, and as the bubbles can't escape, Britt talked about it drives the whole eruption and you get this back and forth all through.

But never the less it has to come out in solution so with the phase diagram it starts going to a low water content and the whole thing shifts to the right.

And at those temperatures, 980 degrees -- it actually starts out at that by the time it gets to the earth's surface, it wants to be solid, entirely solid more or less.

Something at 55 degrees -- 55 percent crystals can't erupt because it's at maximum packing. It's a solid and chokes and conduit, becomes explosive, for example.

But it will actually shut down the whole system. That's what volcanoes are in many ways. They're like -- they're trying to shut down so they build and pile on top of it until they actually shut themselves down and they degas.

So the suffocation effects are enormous, in terms of loosing -- just loosing volatiles. And that's a way that the volatiles actually get trapped out of the system, partitioned out.

And this is like many have talked about somewhat. You have an enormous solidification effects and they start eating out this stuff.

What happens in fact the volatiles are richest where the solidification is the greatest of course, because the amount of melt is low, and there is excess melt of fluid around it?

So not only here is it very important and this is coming out, but in a volatile free environment, any time the magma rises up into conduit, a major problem is it starts undergoing thermal death.

In other words the solidification is at right angles to the flow. So heat loss is at right angles to the flow field. So, no matter what the flow does, it cannot actually impede solidification it just starts growing and growing and growing and trying to stop the conduits.

And the conduit either has to overpressure itself, keep opening itself to offset this solidification. But these effects can be enormous. So the thing that I would like to see in this -- these kinds of -- this kind of work -- you know an investigation of initial conditions, as related to what kinds of things we see on the surface, in terms of related to the volumes of output and the times, of course big time duration effects.

Systems start out -- it's like puncturing a balloon and then you pressurize -- and affects its solidification of the reoccurring viscosity. These are really big factors that I'm sure you agree that they're very hard to model, but they -- these are critical issues.

And it would be nice to see that kind of approach in here somehow.

MR. HILL: Well again, the approach we've been taking is trying to build in the realism if we can. You know, certainly I agree that the effect of volatiles is very profound in fully thermal mechanical effects that is going to occur during a complex depressurization.

But we do know, for example, with these volatile contents, that we've erupted a number of volcanoes in the Quaternary that have a cone phase, a tephra phase, and a lava fall phase.

So there still is the ability of these magmas for -- of descent to avoid the thermal death and crystallization choking, because we see them at the surface.

CHAIRMAN RYAN: I think we could go on for a long time. I'd like to bring this to a close.

MR. MARSH: The real question is that, the incorporation of these effects in the modeling. We certainly see magma under the surface. That's not the question today.

But are you going to try to put these kinds of serious issues in the modeling, that's --

MR. HILL: We're trying to build in the variable viscosity. We're trying to build in thermal control as well. But you're again getting into, like you're saying, very complex models.

But you know if you want to start building in some sort of experimental apparatus that has variable viscosity, variable openings, and variable temperatures, that's quite a challenge.

We're starting with the apparatus, because it's an established apparatus, and it would gain us the insights that we needed in the initial state. So, while I appreciate the desire like we all have to make this as realistic as possible in our models, we do have a limitation in the knowledge, and in the ability to duplicate this unusual situation in the lab as with a computer.

We're working on it. A number of people are working on it.

CHAIRMAN RYAN: Thanks. Thank you for your presentation and for the good discussion there after. I'd like to move now to our next presenter, if we could. I think Dr. Matt Kozak will be making a presentation on the Alternative Views of Modeling of Magma Repository Interaction at Yucca Mountain.

And that -- you'll be handling all comments?

MR. KOZAK: Yes.

CHAIRMAN RYAN: All right. Is John going to make a comment?

MR. KOZAK: No John just said it --

CHAIRMAN RYAN: Okay, Great.

MR. KOZAK: Now this is still --

CHAIRMAN RYAN: No that's a lot easier.

MR. KOZAK: It's a lot easier. I don't need to project from the diagrams.

CHAIRMAN RYAN: No. You can if you want.

ALTERNATIVE VIEWS ON THE MODELING OF MAGMA/REPOSITORY INTERACTIONS AT YUCCA MOUNTAIN

MR. KOZAK: Well first I'd like to say thank you very much for the opportunity to come here and present this work. I'd like to emphasize that this is the work of the project team.

Next slide please. Project team contributors, a number of whom are here today, myself, Mr. Apted, Mr. Bursik, Shane Findlan, Randy James, John Kessler, Frassier King, Mick Morrissey.

MR. MARSH: Excuse me, might I -- are there handouts for us?

CHAIRMAN RYAN: You did not bring hard copies for us.

MR. KOZAK: Yes.

CHAIRMAN RYAN: You did?

MR. KOZAK: Yes.

CHAIRMAN RYAN: They're on their way, thanks.

MR. KOZAK: Before I really get into it I'd like to remind you about the EPRI's role in this whole process is. That is where federal agencies have -- we look around us and we follow the work of the Federal agencies and we try to fill in things that perhaps they're not doing.

And so we looked at this and we looked at some of the arguments related to the probability of the event that we talked about earlier. And decided that our time was really better spent, and we could take a bigger impact by starting to look at consequence side of things.

First and foremost was to look at the waste package. We have this waste package that's one of the toughest materials known to humanity. And really in and really in the DOE and NRC analyses up through TSPASR which is the most recent information that we have available to work on.

The waste package plays no role. It essentially disintegrates as soon as it's contacted by the magma, and is blown out to the top. So we thought, that by looking a little bit more in detail at that, we might be able to develop a more reasonable assessment.

Next Please. And to do that we're trying to develop a reasonable expectation base. To give you a heads up of where we're going with this, what we end up finding out is that we can't get the material out of the waste package when we start taking into account the degradation processes that can occur during the eruption cycle.

That's not to say that the igneous -- the magma does not have an effect on the waste packages, but during the time frame of the eruption, the waste packages are not damaged sufficiently to cause releasing during that period.

They may be sensitized so that as we heard earlier that the ground water releases at later times would be affected. And we're going to be looking at that more in a future report.

Next please. Let's just go through. It's sort of instructive to look at it when we start looking at the waste packages themselves, to look at the sequence of the events of the eruption.

And the first is that this dike rises sort of as a sheet to intersect with the repository. And the -- as that rises through, there is significant amount of degassing that goes on in there.

The surface area at that point that could even take into account surface of the amount or degassing what might have taken effect is very hard because it's perhaps a kilometer long dike that's coming up as a sheet through the zone.

And so there could be significant degassing that's going on during that process. So the dike eventually raises to the repository level. Next one please.

We get perhaps an intersection if it intersects with a drift, you get an intersection perhaps with a single waste package. Meters -- meter diameter -- meter with dike coming up make it a single waste package.

So we consider that to be an initially impacted waste package, which is what we're thinking of when we're talking about the initially impacted waste package in this presentation.

And then over a period of time, that dike evolves into these cylindrical vents. Next please. And, at one or more places along the dike, it may evolve into these vents.

Now, over a period of time those vents can widen, so that initially impacted waste package, which may or may not be on the center line of where a vent would occur.

If it's not, then, when it evolves to the vent stage, then that waste package is not going to be contributing to releases up the vent. If it does happen to form a vent on the center line of a drift, then that waste package and perhaps a few more to either side of that waste package can contribute to the release that might occur coming out of the vent as it heads toward the surface.

Now if you look a little bit further out down the drift, down here, we have the magma is coming up and it's going to start flowing out down the drift.

And the heat losses that are going to

happening as the magma flows down, would sufficient that fairly quickly the magma's going to solidify and we're going to get a basalt plugs down the drift.

So, the number of waste packages that can contribute is the number that are actually actively in this vent at that state of the eruption site. And the ones that are further down there's really no driving force for radionuclides, even if those waste packages were breeched, there's no driving force for that -- the radionuclides to come up a gradient of magma, to think of it that way, to come back up and go up the conduit.

So if you can identify the width of this conduit, and the number of waste packages that could conceivably be affected by that, and consider the fact that there could be more than one conduit, then that gives you an idea of the bounds on the number of waste packages that could be affected by it that contributed at this state in the eruption.

Next please. And then at that stage we have to get this tephra plume to come out here 18 kilometers. One of the important things to recognize is that as this initial dike comes up, the initial dike intersects with the drifts at the drift level, at that stage, the type of eruption that comes up when the dike hits the surface is fire -- and at lava flows.

There's very little tephra production at that stage. It's not until it evolves into this vent stage that we start getting these significant tephra plumes.

So we find that, if the waste package initially were to just dissolve as soon as the magma were to hit it, and the fuel were to just come up as chunks -- which is in a way a conceptual model for some of the DOE and NRC models, except the waste package is just instantaneously gone -- that if it were to come up at that point along with the dike eruption, that it would be coming out in lava, and it wouldn't come down and it wouldn't affect the record.

It's only if the waste package does not fail at that initial stage, it fails at some intermediate stage later on, so that whatever radionuclides are released could come out during this period, then it would be associated with tephra that could get down to producing dose.

Next please. To look at this we broke the system up again to initially impact the waste packages and neighboring waste packages that might be in a vent.

And what we did was go through sort of a logical diagram of what type of effects we need to take into account at each stage of the eruption. And the main thing to recognize in this initially impacted waste package is, as you come down there, if the C22 and the stainless steel shells were to completely be destroyed upon initial impact, again, if you come down here to bottom, you have decision branch to say whether or not it's actually going to get down to the RMEI.

And, for these types of eruptions, as it comes out, it's not getting to the RMEI to produce zero dose if that were the case. So, but we did want to evaluate that.

We wanted to find out whether or not the conditions at that stage of the eruption were severe enough to cause this extreme damage. And we'll be talking about that a little bit later in the presentation.

We also have a logical diagram of how the waste packages can fail and the types of failure mechanisms that would come in for these waste packages that are not initially in the dike.

CHAIRMAN RYAN: You don't want us to read that?

MR. KOZAK: No. The details of this are, you know, we'll be going through each of the sort of the phases of that as I go through the talk.

CHAIRMAN RYAN: Okay.

MR. KOZAK: One of the things that we wanted to do was to look at this issue of magma down the drift and the evaluation of this dogleg of magma that could come down a drift and then go up someplace else.

We wanted to look at the waste package failure mechanisms. Next please. I wanted to talk a little bit about this Nicholis and Rutherford paper.

And I'll come back to it a little bit later in a different context. But, when we look at the view of the TSPASR conditions of which the initial dike hits the drift, they're looking at magma temperatures up to 1,200 degrees centigrade and on the order of four centimeters per second for the upper end of that.

This paper by Nicholis and Rutherford, which was referred to in the previous presentation, one of the other implications of that paper is that the temperatures that are consistent with the observations of the basalt, would be much lower.

That's very important for waste package performance. And it is very important for the viscosity and the cooling rate of the magma as it potentially flows down the drift, and also in terms of the ascent rate.

If you have a much more highly viscous fluid, it's going to be moving at much slower rate. This actually -- the conclusion that Nicholis and Rutherford came to said that it needed to be greater than four centimeters per second.

But my argument here is that, we're probably at the lower end of the magma ascent rates because of the higher viscosity and lower temperature.

That's an interpretation of their information. It's not from their paper directly. And we actually did not use this information as part of our evaluation of the extrusive scenario.

So, the results that we will be showing here may actually be more conservative than they would be if we were to have done it, including the Nicholis Rutherford evaluation.

The first thing that we want to look at was the rise of the magma into the drift, the flow of magma along the drift, and to look at whether or not these shockwaves and the potential formation of a dogleg, a new pathway to the surface types of phenomena could occur as we show in the Woods et al model.

The evaluation that we did with using this computer code called SAGE -- and Dr. Morrissey is here to fill us in on the details later if we'd like to.

Essentially this is a code that was developed for evaluation of underground nuclear testing. So, it's fully coupled -- it's a big, elaborate, fully coupled heat mass -- to transport, and has a good deal of acceptability in certain very exotic communities of subsurface flow phenomena.

In the SR, the DOE -- some of the initial temperatures on the order of 1,200 C and corresponding viscosities on the order of 140 pascal seconds.

If we look at the Nicholis and Rutherford information, we are actually down in the much lower temperature range and the viscosities are right near a break point.

So, as soon as the temperature starts to drop, the viscosity is going to go up very quickly. And that's an important point because, as the dike is rising up to the repository level, if the magma begins to flow down the drift, it's going to cool very rapidly.

There's no other heat source around there. And, as it starts to cool, that viscosity is going to sky rocket. So, we're not going to get these huge water-like flows going down the drift.

This is going to be more like the flow at the surface -- at the ground surface where we see sort of clinkers forming at very gradual lava-like flows into the drift and solidifying and setting up as the initial stages of basalt formation at anything offline from where the drift comes up.

Next please. This is -- go back. Yes. Could you back up one? There we go. Okay. This is the first few seconds of the dike rising into the drift.

If you squint very carefully, off to the right in the drift you can see that it's a little bit narrower. And that's the effect of the waste packages.

The waste packages a re in this half of the drift. And there's no waste packages in that part of the drift. What this is a plot of pressure as a function of time just as the initial tip of the dike hits the drift.

And, what we see is the formation of a high pressure center immediately above the dike, and relatively low pressure propagating out from there. There is no indication of the shockwaves of pressure moving up and down.

By the way, this is essentially a two dimensional version of the Woods model that showed the shock pumps.

MR. MARSH: What's the real time?

MR. KOZAK: This is on the order of a few tenths of a second, I think.

PARTICIPANT: What it's showing you is you take the Woods et al model of gas entering into the tunnel. And, if you put the vertical dike into the horizontal drift, what you're going to see is this pressure concentration on top of the drift where it's fairly high.

And so, that's what we really wanted to show. You're not going to get these larger pressures down the drift to the dogleg scenario. You're going to have the continuation of any dike up through.

MR. KOZAK: And this very rapidly exceeds the fracture stress limit for the rocks above the drift. And so, the dike just continues going on the way it wants to go, rather than coming down here and creating a dogleg.

Next, this is the temperature on the same timescale, same space-scale. Here we're assuming no heat transfer at the boundaries. You can see the heat moving up here.

The main reason for putting this up is to show that we can do fairly complex heat transfer behavior in this zone and look at what the effect of the heat on the waste packages is, and, to demonstrate the behavior.

Next, please. So, the implication of this modeling is that the down-drift pressure is much less than the pressure above the drift. The pressure above the dike rapidly exceeds the fracture stress limit.

So, the dike will continue straight to the surface. And we don't have a dogleg in that case because there is not these high pressures to create them.

And that shockwave appears to be an artifact of their 1D model. And, when we put it into the similar kinds of conditions into the 2D model, that disappears.

Next please. The next thing we did was to look at the effect of this initially impacted waste package -- rising up, hitting it to a single waste package that just happens to be located over the dike.

We wanted to see what happened. So, what we wanted to do was look at sort of a worst case on this. And, what we did was to evaluate a -- even though we're looking at the dike stage where this is a plainer source of magma that was easier computationally to look at something that's more like a cylindrical jet coming up and hitting the waste packages.

And so, this is a rising column of magma with magma densities and treated as a -- space for the purposes of calculations. The properties are associated with the temperature of the magma and the temperature assumed to be of the repository early on in the repository history for the intial conditions of the waste package.

And, what we did was a collision calculation using this detailed finite element model with this code called ABAQUS/Explicit, which is an EPRI code, I believe.

Well, it was developed to support EPRI programs for different kinds of waste package and other pressure vessel failures under collision scenarios.

So, what we do is we model it as an impact with very conservative conditions. Again, we consider the 100 meters per second to be an absolute maximum that you can see under these circumstances, -- for my diagram width of the magma column, and then we're going to look at what happens.

Next please. These are very early in the collision. This is on the order of a 100^{th} of a second. You can see the level of detail that goes into the model.

We have -- the waste package internals are modeled and the deformation of the shell is calculated as a function of time. Move on to the next one.

This is a couple hundredths of a second later. And you can see that, because of the collisions, it is showing that the internals of the waste package are being disrupted.

They are not completely filling the waste package. So, it's talking into account all of these things. This analysis, even though we're only showing the first couple hundredths of a second, the analysis was carried all the way through for the full collision.

And actually, because of the extreme conditions that we put on it, there was a second, very substantial collision with the roof of the drift.

So, the magma comes up, it hits the roof of the drift, and then we look at what happens and how much damage there is. Next please. There is a cut-away view of a sequence of what happens to the fuel elements and the internals to the waste package.

Initially we've got it in pretty good shape. By the end, there is a fair amount of damage to the internals. And, just to summarize this without going into the bloody detail of the calculations.

Our estimate is that the energy applied to the waste package, if you look at the direct coefficient kind of calculations, is on the order of 100 to 10,000 times what it would actually be experiencing and the type of rise rate that we think are reasonable.

So, it's extremely conservative. Based on this paper by -- that I was talking about earlier, the Rutherford paper, the rise rates may be on the order of centimeters per second rather than on an order of tens of meters per second.

But, even under this very extreme condition, it wouldn't break the package. We had a structural dent, and possible minor tearing on the C22 shell.

There was damage to the internal elements. But there was no rupture of the internal structural shell, the stainless steel shell. And so, to summarize, simply the impact, none of the other failure mechanisms, but just from the impact, we don't get any release.

PARTICIPANT: Is the weld as strong as the container?

MR. KOZAK: The weld, I believe -- we'll get out of my area of expertise pretty quickly. I think, at high temperatures, the weld beings to lose its strength faster than the material.

But, at the temperatures that we're at here, which is at the repository temperature, they are of comparable strength. So, that becomes more important later on.

Then, as the temperature really elevates, then we look at a little bit longer term effects. But, even there, it's not the weld that's the critical failure or critical potential failure.

So then, if it doesn't fail because it has been slammed by the dike, now we're going to look at -- we still only have one waste package that is affected, until the eruption cycle starts to go into the conduit cycle of eruption.

That stage may last on the order of weeks or longer. And we essentially took the information from DOE from a TSPASR in terms of duration of eruption.

And we used that as a probability density function. We used the DOE's information on that one. But, over a longer timeframe, the failure mechanism diagram that you guys couldn't read before -- we're starting to get into what some of the other failure mechanisms are.

There's a concern of erosion, that there is a corrosive and abrasive material flowing past the waste package. And the conclusion, this was from Frazier King from Canada, who's been with waste management business for a very long time, went through an evaluation of what the effects of erosion corrosion would be.

And, he came up with an estimate of an erosion corrosion rate and put it together with the duration of the eruption. He came up with a maximum on the order of two millimeters.

So, a failure or a stripping away of the C22 shell is bound to be very highly unlikely. Next please. The second mechanism that we consider was failure by this internal overpressure, which, in some of the documents, this it the failure mechanism that is used to justify the neglect of waste package from the TSPA analysis.

And, essentially, the idea is that because we've got the temperature going up in the waste package, that we generate the stresses from the differential between the expansivity of the steel versus the C22.

And we also have internal overpressure from the air pressure going up. And, you can think that if the pressure exceeds some threshold value and threshold value will be going down as a function of temperature because the yield strength of the material is going to go down with the temperature, if it exceeds some threshold, you could get it popping open like a can of soda bursting.

It's generally the concept of what we're talking here by internal overpressure. So, material strength is decreasing at an increasing temperature. But, one of the things that has not been taken into consideration in these evaluations, and I think we're the first ones to look at this, is that, whatever internal pressures and internal stresses are built up, they are offset by static pressure exerted by the column of magma over it.

We have the waste package sitting in the column of magma, very dense fluid. As it rises to the surface, we have hundreds of meters of this heavy fluid sitting over the waste package, exerting a positive pressure on the outside of the waste package.

And, going through some calculations, we can show that, after a very short time, the stress on the waste package becomes compressive, so the net stress on the waste package is not from the inside out, it's from the outside in.

And that's an important factor because the waste package is stronger than it is the opposite. Okay. So we consider a range of magma conditions. And our conclusion was that the waste package will not fail on overpressure.

The next thing to consider was creep failure. At the high temperatures, we don't have creep data for some of these high temperatures, so we have to extrapolate from lower temperature.

So that is kind of an uncertainty in our evaluation. The contact temperatures of magma on the waste package are lower than has been considered by DOE or NRC.

Based on the Nicholis and Rutherford paper, if we were to do this analysis today, we would actually consider even lower still. For creep rupture to occur, we have to have some way of accumulating strain.

In other words, there has to be a differential stress across the waste package. And, because of the geometric constraints of the waste package in the drift and the C22 next to the stainless steel inner shell, there is not enough space in the different constraints for it to develop enough strain to fail by creep failure.

The final -- I think we're up to the final one. We have considered several failure mechanisms now. Now we're up to corrosion. There is very limited available data for nickel chromium alloys in magma.

And so, what we did was we went and we looked at -- we evaluated literature data on nickel chromium alloys and a variety of electrolytes did some probabilistic analysis of corrosion based on those literature data.

And we were able to come up with some initial estimates for what the corrosion rate would be. But, not being satisfied with that, we decided to do some experiments and to find out what the corrosion rates that we could experimentally measure alloy 22 would be if it were immersed in magma.

Next. So, what we did was took samples of alloy 22. We got samples of basalt, melted the basalt, and put the alloy 22 samples into the magma. And so, this is a picture of the alloy 22 sample being removed from a graphite crucible.

This is from a one hour exposure. Once we take it out, the magma solidifies very rapidly. Here are some results from a one week test, doing some micrographs of what the surface effects were.

This was old magma used with an inert gas purge for a one week test. C22 remained intact during the test and showed some degree of surface voiding.

There was no evidence of inter-granular attack, which actually was one of the primary mechanisms that the metallurgists were concerned about, the potential for inter-granular attack by the type of contaminated materials that are in the basalt.

Next, please. Going on to two weeks, very similar results to the one week test with a bit of an increase in void/inclusion density, and still no evidence of inter-granular attack.

Going out to a month, the surface voiding was more extensive, deeper, up to 600 microns from the surface and still no evidence of inter-granular or other degradation attack.

The net result of magma contact is the C-22 shell was not breached by the mechanical impact. The inner shell was not breached by the mechanical impact.

Essentially, here is a picture of C-22 samples, which you can think of as being C-22 waste packages if you care to, embedded in the basalts after it has been sitting in molten lava.

And so, there is virtually no effect on the -- little bit of surface attack, but not nearly enough to -- the waste package during the time frame of an eruption.

Now, what this may do, the metal may become heat sensitized and corrosion rates may go up over the long term. So, once the eruption has ended and water starts coming back down through, the long-term corrosion rates may go up.

And that's something that we're evaluating now. We've got some initial results on that. And, we will be evaluating that as part of an intrusive scenario of the evaluation.

We will be publishing more on that next year. So, our conclusion is that we have reasonable expectation, given all these failure mechanisms that we've gone through.

And they have found no way to breach the waste package under any reasonable conditions that we apply to the waste package. We have reasonable expectations that we will get no waste packages to fail during the eruption.

And, again, that doesn't refer to the period after the eruption when we haven't had the corrosion. We may have effects that we can maybe look at then.

But, during the period of eruption, we were not able to get radioactivity out of the waste package. Thank you. So let me transition here for just a minute, because up to now we've been building our case solely by looking at the evaluation of the eruption as it would affect the repository and as it would affect the waste packages.

We wanted to do more than that because we wanted to look at the total system as a whole. So, the remainder of the presentation is going to be an assumption that, even though we think that within reasonable expectation no waste packages would fail, we're going to assume that the waste package fails anyway.

So now we're going to take into account effects if we haven't perhaps taken into account extreme enough effects in the eruption or something that we haven't taken into consideration.

What this allows us to do is look at the other aspects of the system and see how much they contribute to performance and to look at alternative assumptions that we can put into other aspects of TSPA.

Next slide, please. So, what we're going to look at is the ash dispersal modeling, biosphere analysis, and we're going to do a series of sensitivity studies.

And we call these conditional results rather than just sensitivity studies, because all of them are conditional on the assumption that a waste package fails, even though we don't think it will.

So, we're going to assume it fails anyway, go through and come up with a credible release mechanism for how radionuclides could get out of a partly failed waste package.

Given how tough this material has proven to be, we are going to take some credit into account of the waste package itself. And, what we really want to do is demonstrate defense in depth from each part of the system.

So, we're looking at the multiple variables now. The first part that I want to talk about is the ash dispersal model that we did. We went through and did an evaluation with multiple models.

We used the ASHPLUME code that is used by NRC. There's a commercial version called TEPHRA. We used that one. And we also compared that against three other models that are common in the ash dispersal literature.

Ultimately for the results I present in the

TSPA analysis, we focused on the results from TEPHRA. And, what we found was that we had a lot of realizations when we went through and did probably calculations using TEPHRA to look at a variety of eruption magnitudes, eruption energies, column heights, things like that that were all within a credible range based on our range of analog eruptions that we were looking at.

And what we found was that a very large number of them had negligible deposition of tephra at the -- point of the record. In fact, depending on what you want to call negligible, it was between 60 and 80 percent of the realization produced no deposition of tephra.

So, even though we don't think the waste package fails, if it fails, probably 80 percent of the time we get negligible accumulation and negligible dose down wind.

The other thing to point out about this, is, this may be a little bit hard to see just what it is. But these are our tephra contours here, tephra iso-depth contours coming from the TEPHRA model, from ASHPLUME model.

This is compared to a different model, a PUFF model. And what you'll see is the receptors would be right down here. This is the point at which the RMEI occurs.

And so you'll see, even though 80 percent of the realizations gave negligible accumulation, it's still a very conservative model. The PUFF model actually has most of the deposition going someplace else.

And so, it would give a lower number of realizations that would -- that you would calculate using the ASHPLUME model.

MR. HINZE: When do you get a different distribution?

MR. KOZAK: The PUFF model takes into account variability. The ASHPLUME model assumes that the wind blows toward the receptor through the duration of the eruption.

There's a number of effects. One is the wind distribution, the other one is that the PUFF model takes into account the thermal circulation that occurs around the eruption during the period.

So, that's why you have a lot -- it's kind of hard to see here. But, the eruption is actually right down there. So you actually have a lot of deposition upwind.

In fact, it would be upwind from the volcano because of these thermal cycles -- convective cycles into the air. And so it is moving it in every direction.

It doesn't just move nicely down the wind grade. The final point to make about this is that the particle sizes, when they are deposited, aren't respirable.

They are much bigger than respirable size. In fact, the respirable size end up in Canada someplace for some of these calculations.

(Laughter.)

And so, we need to take that into account when we do this. We also have to do biosphere modeling to convert this deposition of ash into dose.

And, one of the things that we wanted to look at, I don't know if you guys have looked in depth into the way biosphere modeling is done for this scenario.

But, essentially, probabilistically, you have to sum all of the previous volcanoes that may have occurred, assuming that the ash stays around for a very long period of time and you add up all the previous potential volcanoes at the time that you want to calculate the dose.

So, it's not just the dose that occurs

during that year. It's the dose that occurred ten years ago, that the ash is still sitting around that you combine all these probabilistic.

It's a very complicated way of doing it. And, so one of the things that we wanted to look at -- when you look at past analogs, meaning how people behave after an eruption, they clean up.

Here's an example of Mount Pinatubo. People don't just sit there under several centimeters of ash because it is nasty and awful, and it clogs up machinery.

This is a significantly different way of doing the dose calculation of the biosphere. We can argue whether or not that's appropriate. In fact, there are some interesting philosophical arguments we can have over a beer about whether or not we should take into account cleanup because it's not intervention.

You're doing a radiological assessment for this practice. But a point of fact, the cleanup is going on, not because of the radiological content of the ash, but because of the ash itself.

Even if people are unaware of the radiological content of the ash, they will clean it up. They will clean it up to differing extents on agricultural land as around their home and so forth, but they will clean up.

CHAIRMAN RYAN: I just wanted to point out the fact that there's actually radiological content in the picture you showed us.

MR. KOZAK: That's right. That's a whole different -- yes, you're absolutely right. So, the implications of this in terms of a biosphere model is that our dose is predominantly in the first year and, to a lesser extent, comes from later years, depending on what we assume about how quickly it is distributed in the system.

As a result, we don't have to add up doses over many years. I'm not sure quite how many years the DOE and NRC assume that the radioactivity can persist in the biosphere.

But, we assume that it goes over the -- so, we have different important pathways in the first year and in later years. And we wanted to look at this.

So, as one of our sensitivity studies, we are going to look at this issue of consistence of biosphere. But it's not part of our sort of base case.

The ash particulates, as I said before, are not respirable. And, indeed, in the first year dose, you could argue that a lot of people are going to be acting like this guy right here.

They're not going to be going around and just breathing the dust in or the ash in, because it is going to be unpleasant, not because they are worried about radiological content.

But there will be some degree of removal using masks and things like that to reduce the amount that they will inhale. Just normal behavior of people.

I don't think it is reasonable to incorporate human behavior that's not normal after an eruption. So what we did was evaluate the biosphere dose inversion factors for different particle size ranges and differing deposition.

Of course, we want to look at that as -- sensitivity right there. So, our conditional analyses, again, are conditional on a release from the waste packages that we don't think will occur.

So, what we did was we assumed that there was some failure. And, in this case, we assumed that the failure did occur along the weldment and split the waste package open to some degree.

And then we have diffusive releases from the waste package into the magma as it goes by. There is a whole series of things that have to go on for the magma to contact the internals of the waste package.

There's sort of a whole series of events in which magma needs to flow into it, contact the fuel, dissolve the fuel, diffuse back out through the waste package, and get to the outside.

By and large we're not looking at that because we just didn't have the time. It was complicated. So, what we did was look at a diffusion layer across an opening in the external shell of the waste package.

So, our base case assumptions are that we have this type of release from the waste package. Something that I haven't talked about that we discuss in the report is that the thermal field around the repository changes the stress field and has the potential as the dike is rising up, that it could divert.

The peer review panel discusses this concept that has a potential to divert the dike because of the changes in the normal stress field. And so, as part of our base case, we assumed, for lack of a better assumption, that the higher the temperature was, that the more likely the dike would be diverted.

So, if it were diverted, that lowers the probability that the event occurs at all. So, at early times, when the temperature is highest, you have a lower probability of the event occurring at later times.

At later times it ramps up until the year 2000, in which the thermal field is essentially gone. At that point it reverts back to the normal pre-waste placement probability of the event happening.

And we're going to look at that sensitivity study too. So, if you don't like that assumption, we'll work around that. Another one that we're going to assume is that the waste package doesn't limit releases from the fuel inside.

So we have sort of a dissolution mechanism inside and a diffusion out of it. We have no cleanup of ash occurring at compliance points. We have ash fall respirable particles, even though we know that that's not going to happen.

We have to take into account the fact that the ash is breaking down in size or if there is some other mechanism. These are things that we have to explore as part of it.

So, we're carrying these out in spite of there being an initial assessment that we have a zero release during the attack. Okay. So this is the first conditional case.

We have between one and nine waste package failures. This is based on geometrical considerations of how big the vents are. A single vent could get us up to three waste package failures.

If we have these multiple events that the multiple events in a single event, that we could -- up to three vents would give us up to nine waste package failures.

In this one we have the temperature dependent dike diversion. And what we find, we have the 95^{th} percentile and a mean right here. We get about nine orders of magnitude dose lower than TSPA-SR.

Next please. One of the sensitivity cases that we wanted to look at is persistence in the environment. We didn't set up our analysis to do this in the elaborate way that DOE does it.

So what we did was in an approximate manner. We found, after going through the calculation, that the doses in year two to about ten were about constant.

So we just took that constant and added that for as many years as we wanted to. So, our worst case analysis for this particular assumption, the persistence in the environment, assuming that it stays in the environment forever, and all it does is change by decay, it would be this top curve up here.

So, it increases the doses over our sort of nominal conditional case. We don't want to call it nominal. But, our first conditional case that was sort our base of assumptions.

It increased it by about two to three orders of magnitude. But it's still very low doses.

CHAIRMAN RYAN: Let me just ask a very quick question while we are --

MR. KOZAK: Yes.

CHAIRMAN RYAN: That no depletion case is the case where you're assuming the radioactivity that's deposited is available at that same deposition forever?

MR. KOZAK: That is correct. That no depletion in the ground, it doesn't blow away, it doesn't redistribute, it just sits there.

CHAIRMAN RYAN: I'm going to ask the audience a question. I want to peak around the corner and see where that red graph flattens out. Can you tell me when that happens?

MR. KOZAK: That's a million years?

CHAIRMAN RYAN: Oh, it's a million? I

thought it was ten thousand. I see, sorry.

MR. KOZAK: Based on the way we did the calculation, it will continue going up because what you're doing is adding in the consequences of all the previous eruptions.

So, essentially this is the number of years that have occurred prior to it.

CHAIRMAN RYAN: There are more and more eruptions as time increases?

MR. KOZAK: Yes.

CHAIRMAN RYAN: I got it. All right.

MR. KOZAK: Exactly.

CHAIRMAN RYAN: Thank you.

MR. KOZAK: The effect of waste packages, now we took out the waste package. And so we assume that, when the dike hits it, it will disappear. And we get on the order of five orders of magnitude increases over a base case.

We're still, because of just doing this one parameter at a time, one sensitivity variable at a time, we're still very low, six orders of magnitude below the dose standard.

We looked at a whole bunch of other events, some of them positive, some of the negative. The effect of dike diversion, if we -- if the temperature effect is really very strong in changing the direction of the dike, we could get full dike diversion in 2,000 years, which means we couldn't get an event for the first 2,000 years.

On the other hand, if this effect doesn't actually occur, then we actually just increased the dose of early times by less than an order of magnitude.

Going and looking at respirable particle sizes versus non-respirable particles sizes, it was less than an order of magnitude increase in dose. Now, the reason for that was we were looking at -- the dose factors that we were looking at included the nasal pharyngeal contribution to inhalation.

The increase in lung dose for the smaller particle size wasn't that much. That actually surprises quite a bit that it wasn't that big of an effect.

The other thing that we looked at that would be a positive effect, that we didn't include, was the conditional probability that the vent occurs in between the drifts.

We assume that, for our basic conditional analysis, the dike intersects it and we start to form these vents, that they hit a waste package, essentially.

This one we took into account the space in between the drifts, which is quite a bit of space, and drops it by about a factor of six. This one is where we took all of the conservative assumptions, all these different orders of magnitude, put them all together in one analysis.

The interesting thing about this is that, when we look at this early time, that's up where the TSPASR dose level is. So, we can recover the orders of management of dose that DOE dose by adding in all these conservatisms.

So, the point of this is, by doing what we consider to be a more reasonable analysis, we can show the level of conservatism that's associated with each of those assumptions.

When we add them all back in, we still go up to where the DOE dose analysis brings us, and we still comply. So we can demonstrate the relevant fact that each part of the analysis can provide conservatism.

CHAIRMAN RYAN: Is the next step of taking this to a probabilistic approach, looking at each one of these, considering each of these variables?

MR. KOZAK: I'm sorry, I wasn't clear enough. This is a probabilistic calculation.

CHAIRMAN RYAN: This is a probabilistic calculation.

MR. KOZAK: This is run using at risk --

CHAIRMAN RYAN: Okay.

MR. KOZAK: -- with a thousand realizations -- sample. And each of the distributions is laid out in the report.

CHAIRMAN RYAN: Okay.

MR. KOZAK: They are subjective probability distributions, so --

CHAIRMAN RYAN: I understand. That's fine.

MR. KOZAK: So, to summarize, our reasonable expectation approach has led us to a conclusion that we would get zero release during the eruption.

We looked at multiple different kinds of failure mechanism in the waste package. We couldn't really come up with a credible waste package failure mechanism for any of the circumstances that we looked at.

The key lines of evidence that lead us to that, the conditions of the drift level are not as extreme as has been assumed previous by the NRC and DOE in our judgment, based on our evaluation of the available data, based on our model.

The magma entering the drifts is much less violent than has been assumed previously. The conclusions by Woods et al -- we have shown that the shockwaves and these extreme things that go on are not going to happen when we take into account two dimensional flow of the magma.

The waste package provides a very significant barrier to release. I think it is extremely conservative. Our analyses show an order of six orders of magnitude conservative to ignore it.

The magma entering the drifts is going to cool and solidify pretty quickly to isolate dike and event. And so, to conclude, we think that the analysis that shows up in the TSPASR is extremely conservative on the order of nine orders of magnitude.

But the thing to keep in mind is that, despite all the conservatism, it still complies. So, any potential changes that we could do -- you know, when we start talking about pushing it in the direction of being more conservative.

There's always the tendency when we do TSPA. Everybody is always trying to think of a more conservative analysis. There's a lot of these things that will drive it very strongly to being less conservative.

And so, we were able to demonstrate the

amount of conservatism introduced by different parts of the analysis. And, of those, the waste package is by far the most important.

CHAIRMAN RYAN: Thanks very much.

MR. KOZAK: Thank you.

SESION TWO WORKING GROUP ROUNDTABLE DISCUSSION

CHAIRMAN RYAN: I guess I'm intrigued by your modeling of the -- the detonation modeling capability that you mentioned. Could you expand on that?

MR. KOZAK: I'm going to completely differ that to Megan Morrissey. That's her specialty.

CHAIRMAN RYAN: Could you come up here and tell us who you are.

PARTICIPANT: Can't hear your question.

CHAIRMAN RYAN: I'm sorry. I asked the question, if there could be a little bit of expansion on the underground modeling and the use of it for this magma modeling.

I just would like to know little bit more about the model itself, how it is used, what it is used for, and so forth.

MS. MORRISSEY: My name is Megan Morrissey. I am in the Colorado School of Mines. The model was developed at Los Alamos through the thermal nuclear group.

They allow me to use it to do volcanic simulations of flow-through cracks and whatever. So I tied it to -- well, we first of all wanted to know to interpret the Wood and other's pressure time group.

So, I was looking at that and said, okay, let's really put it in a two dimensional, vertical and horizontal. What it does is it is a compressible fluid flow, multi-gas, multi-phase.

The walls -- there is some expansion to it. But we used a rigid case in what we showed today. You can set it up with any geometric configuration you like.

So what we did was formed a dike similar geometry with analysis configuration. And we used steam, increased the density a little bit to account for ash.

And we allowed it to -- you know, just let it go into an empty drift. And what you saw was the actual -- actually what you saw was a shockwave did develop, not the shockwave that they believe had occurred in their simulations, which is a whole other story?

CHAIRMAN RYAN: They?

MS. MORRISSEY: The Woods et al. What

happens is you set up a shockwave, an expansion fan type that is large enough that it expands onto the top of the drift.

At the same time it reflects off and you see these oblique shockwaves moving down the drift. But those shockwaves are within the steam, the magnetic gas moving down.

You can see the front. And so that's just moving down slowly. It's going to be at the end of the drift soon and start filling up. But what we wanted to try and demonstrate was the fact that that expansion fan with the shockwave on top creates a pressure concentration right at the top of the drift, right above the intersection of the dike into the drift.

So that was essentially what we were showing there. And we can use it for a full range of pressures, different temperatures, different starting conditions, and keep expanding on the complexity of the problem.

So that's what we've done. But we just showed you a fairly simple scenario to show what the Woods et all model would look like in a true vertical horizontal situation.

MR. MARSH: Was the dike open to begin with?

MS. MORRISSEY: It was open to begin with, yes.

MR. MARSH: So you didn't open it with the fluid flow.

MS. MORRISSEY: No, it was a little nozzle. It started out at rest, and let the pressure --

MR. MARSH: The fluid was there behind the nozzle?

MS. MORRISSEY: Yes. The fluid was there behind the nozzle. It was a very, very narrow nozzle, and let it open, just let it go. And that's -- the thing is, we're not trying to do anything realistic.

We're just reproducing based on the Woods model.

MR. MARSH: If you let the dike get out, it would open gradually.

MS. MORRISSEY: Yes, something like that. And that's what the DOE model were going in that direction, showing, okay, here's the opening, it's going to go straight up.

And one model I didn't show, the little -- just a little pinhole above it, and a lot of the fluid just goes straight up. And you do get diversion down the drift.

But pressures are not lowered if you want to use the same 10 to 20 megapascal reservoir pressure.

MR. MARSH: If you add solidification it even --

MS. MORRISSEY: Yes, this was a gas. So we're going to the extreme of a compressible, high discharge. But, if you considered a de-gassed lava or magma, it is very viscous at the 980 degree C temperature.

It's going to start moving down. It's going to have viscosity around 100 pascal seconds or higher. And, as it decreases in temperature with crystallization, that viscosity is going to keep going up and up, really prohibit --

MR. MARSH: Maybe choke the drift off.

MS. MORRISSEY: I don't like to use the work choked. But, it will slow up the flow. It will -- plug and let the rest go up, yes. Exactly. That's one scenario.

CHAIRMAN RYAN: John, you had a question.

MR. GARRICK: Not necessarily for me. I realize that your reference case here was the Woods model. But, looking at it from a total system point of view, of course, the consequence is very dependent upon -- is it not -- the time at which the magma event occurs.

For example, if your latent time where the waste package started their degradation process are degrading substantially, you're certainly going to get a different result than if the waste package is still at their full integrity.

And the other thing -- and you can comment on that -- the other thing that's true here is that now you have to have a disruptive event. And, even though there is zero release, you have disrupted the total system performance in the sense that, number one, you now have deformed and damaged waste packages.

And so, you've accelerated the mobilization of the radionuclide process. And you may have introduced and set the stage for other events, such as downstream criticality or what have you. Would you care to comment on those types of things?

MR. KOZAK: Yes, your first comment is very well taken. We have not taken into account the long-term degradation of the waste packages prior to the event in this analysis.

If we were to do so, the worst case would be the last one that I showed, where I assumed that the waste package didn't contribute to the release. So, at worst, it's going to increase it by some orders of magnitude.

But, yes, you're right. Certainly after, on the order of -- depending on who's model you believe on the degradation, but, when you get out to the 100,000 year range, when the degradation is advanced, it certainly won't have the structural strength that we've assumed in this analysis.

That's completely correct. On the second point, you're also absolutely correct. That does affect the overall total system performance. And we are going forward with that.

We have done some initial calculations on the increase in degradation rate, increase of corrosion rate that would be caused by the sensitization of the material by the magma.

And we have incorporated that into some new calculations. Right now all we have is sort of a very conservative one where we assume that the eruption occurs essentially at time zero.

And so then, that enhanced corrosion rate applies to the rest of the duration of the facility. To do it in proper ways, it would be very complicated because we have to assume that there's a certain degree of corrosion up to -- if we're doing it for 30,000, if it corroded up to 30,000.

And then we have to hit it with magma and do a different corrosion rate thereafter. So, it gets to be a complicated analysis to do that. But, we're looking into ways of doing that for next year.

And so, yes, you will have more rapid failure of waste packages, and you will have increased corrosion in that. The thing to remember is that that's going to be increment over the nominal case, as opposed to the relatively high doses that you get from an extrusive case.

It's an increase in the sort of nominal dose over the dose over the nominal case. But, the nominal case is a probability of one. This has a probability very low.

And so, the net effect of that is probably not going to be very large. The effects, you mentioned criticality, and I have not thought about that.

I don't know where that can come from. It wouldn't be any -- it wouldn't really be that much different to having this kind of event in terms of flow and transport processes compared to the nominal.

MR. GARRICK: Well, I'm only thinking that any time you change the geometry of the fuel assemblies. I'm not thinking during the time of the event itself.

I'm thinking that you just changed the vulnerability of the waste package to a -- issue, such as criticality. That's all I'm saying.

MR. KESSLER: I'd like to address that, John Kessler of EPRI. We're not going to look at criticality in this particular case. We think, as Matt has already explained, that the amount of deformation due to this magma blast of this single container is probably, you know, the maximum amount of deformation.

We already know that there are analyses out there done by DOE that show criticality and how that changes if you have collapse of the container internals.

Our understanding is that the maximum criticality is probably about in the -- as original case, because you've got a near operable moderation. So, we're feeling that it's not an issue. But, we are not planning to look at it.

MR. GARRICK: Right, I'm not concerned about criticality. I don't think it's a major issue either. But, from the point of view of other people, you have changed the conditions of the fuel.

MR. KESSLER: We certainly get it on the transportation side. And that's certainly the hottest issue right now for transportation, fuel reconfiguration.

MEMBER WEINER: I have a couple of questions. When you model with ASHPLUME AND TUFF, how does your plume go before it starts to move down?

MR. KOZAK: Mike, would you want to answer that? That was worked on by --

MR. SHERIDAN: This is Mike Sheridan from the University of Buffalo. This work was done by a colleague, Marcus Bursik, who is an expert in volcanic ash plumes.

He did a lot of iterations, I think 30,000 or something.

MR. KOZAK: There was a range that was considered based on the power of the eruption. And the power of the eruption came back to Mike's work on which eruptions were appropriate analog behavior.

So, there's a whole thread of logic that's gone into answer that -- it's hard to answer.

MR. SHERIDAN: Regarding the height of the plume, we can say all of the ranges of plume heights in that diagram that Britt showed of say, VI4, three and two, were replicated in the simulations.

MEMBER WEINER: Thanks. I asked because, as it happened, I was in Washington State during and after the Mount Saint Helens eruption. And I happened to go hiking on Snoqualmie pass a year after the eruption.

And you could still see deposited fine particulate on the vegetation there. And that's a good long distance. I just wondered --

MR. KOZAK: Well, yes, to address that, Mount Saint Helens is not a good analog at all for these eruptions. These are much smaller, much more quiescent, and nowhere near Mount Saint Helens type of behavior.

MEMBER WEINER: That was exactly the point of the question. The other question I have is what kind of particle size distribution or settling velocity distribution did you have?

MR. KOZAK: That's in Marcus' report again.

MR. SHERIDAN: This has been a big concern of ours concerning particle size distributions because the question is, the total particle size produced by volcanic eruption.

And that sort of data is difficult to determine because, generally, we find the products only at one location, which had been size sorted by falling through to the atmosphere.

So, the compilations of total grain size distributions are extremely hard to come by. But there have been some for these strombolian types of eruptions.

And this is the particle size of the volcanic particles. But we're also concerned about the radioactive particles from the canisters. And this is something that I don't think anybody knows the answer to.

And it's a great puzzle to me of how the material cane come from the canisters into the plume and be transported in this very fine size, because, within the canisters, it's in size of centimeter scale. We're talking about micron size. So, --

MR. KOZAK: Mike, let me --

MR. SHERIDAN: Okay.

MR. KOZAK: Let me comment on that because, one of things that you'll see is we are doing this on a very tight time scale. And we weren't sure which of these mechanisms would become important.

So, when you look at the report, you will find that we have models for mechanisms that ultimately don't show up very much in the TSPA. And one of them is fracturing along ring boundaries in the fuel and being transported out of these particulates.

I didn't present it here. But we did ash plume calculations where the ash was represented by UO_2 particles and that UO_2 density. And so, it is fallout of particulates.

The ones that I showed were for dissolution of contamination into the ash so it's being transported as dissolved in ash or dissolved in magma, which then evolves as ash.

CHAIRMAN RYAN: Matt that's really important assumption. The action is where is the radioactive material.

MR. KOZAK: Yes, that's right.

CHAIRMAN RYAN: So you just offer the radioactivity, the radionuclide content of fuel into the ash?

MR. KOZAK: For these calculations, yes.

CHAIRMAN RYAN: The volcanologic of it, nothing left behind in the chunks of fuel. All the radioactivity is dissolved in the ash.

MR. KOZAK: Not all of it gets out during the eruption.

CHAIRMAN RYAN: That's what I want to know. What fraction get to the ash?

MR. KOZAK: The mean case, I mean, it's a distribution, because it's all probabilistic. But, the mean was about ten to the minus fifth, I think, of the inventory of a waste package would get out.

CHAIRMAN RYAN: Hopefully we'll address that kind of question a little more in detail when we hear about the perspectives on issues on aerosol and modeling, etcetera.

MR. KOZAK: But, keep in mind, that was based on our model of release from a waste package.

CHAIRMAN RYAN: Right.

MR. KOZAK: Which actually itself was probably pretty conservative.

CHAIRMAN RYAN: Right.

MR. KOZAK: Because, it only accounted for diffusion across the boundary of that opening. I mean, something that's diffusing along the line of all the reactor -- the waste package internals, that's a very long diffusion path.

It has to come through all this magma as it is solidifying, and all these other considerations. We just didn't have time to develop something --

CHAIRMAN RYAN: I don't agree or disagree, but I just want to kind of establish in everybody's minds the realism is not so much where the particles or how to they get created, or where they go.

It is where is the radioactive material. And, it may or may not be distributed uniformly, nonuniformly. And we need to understand how people assume that.

I guess the models tend to put more of it into the respirable particle size range for the longer haul -- perhaps be a little bit aggressive in putting the radioactivity in the air where it's, I think, less might be in the air.

MR. KOZAK: Yes. See, our original thought when we start putting the model together is we expected that there would be at least some circumstances when we were going to have blow chunks of UO_2 out like a whole waste package, you know, granularized and blow it out.

So, we did a lot of work on that and got a really nice model in one of the appendixes that we never ended up using. But we developed it, so we put it in there, of how they react as they're going up the vent.

You know, all these kinds of things are in there. There reason they're in there is because we had to develop a parallel. And then, once the evidence started coming through on what was important, we found out some that we didn't need.

CHAIRMAN RYAN: If you're rating the dose perspective is unimportant -- the plutonium an americium.

MR. KOZAK: Yes, but it would all be -- I mean, the majority of it is going to behave like uranium particulates, not from a dose perspective, but from the particulate perspective.

It is all associated with fuel. Just to let you know, the report number, if you're interested, you can either get it from Monitor or from EPRI.

It's an EPRI report number 1008169. And it was published June 2004.

MEMBER WEINER:

MR. BRITT HILL: I just have two more. When you calculated the inhalation doses did you include resuspension?

MR. KOZAK: Yes.

MEMBER WEINER: What resuspension model did you use?

MR. KOZAK: I believe it was -- let me think. I'm pretty sure it was simply a resuspension factor.

MEMBER WEINER: Yes, but, did you use the same resuspension factor that everybody has used forever, which is basically the one --

MR. KOZAK: I'd have to go back and check the specific file.

MEMBER WEINER: Okay, thanks.

MR. KOZAK: I'm not sure. I think it was probably a conservative value.

MEMBER WEINER: Yes, I didn't mean to put you on the spot.

MR. KOZAK: So, even when we try to do reasonable expectation, we find ourselves slipping back into doing conservatism. It's just we fall into it all the time.

MEMBER WEINER: Okay, my very last question is, what has been the NRC and/or DOE response to this contention that you just mentioned?

MR. KOZAK: This is the first time we have presented it in this form. There has been no official response.

CHAIRMAN RYAN: You are the response.

MEMBER WEINER: I am the response.

CHAIRMAN RYAN: Al?

MEMBER WEINER: Unless somebody from NRC wants to comment.

MEMBER CROFF: Well, I would --

MEMBER WEINER: Tim is.

MR. McCARTIN: This is Tim McCartin, NRC. It would be premature for the NRC to comment on this information.

PARTICIPANT: My name is Sharon. Regarding the internal overpressure temperature, is your contention that, because of the static pressure due to the magnetic forces on the waste package, that that would offset the internal pressure?

MR. KOZAK: Yes, in part the analysis is based on that, that is correct.

PARTICIPANT: And I would agree with that for fully embedded case. Have you looked at scenarios where you have a partially embedded waste package or a waste package that's not impacted at all magma, but is exposed to the high temperatures?

MR. KOZAK: We have not looked at that as yet. We expect that to be more important for the next part of the analysis, which is the influence on the nominal scenario, essentially the intrusive scenario where you're looking at ground water releases.

PARTICIPANT: Okay.

MR. KOZAK: Because, if it happens away from the vent, it's not going to contribute to what gets back up in.

PARTICIPANT: Okay, so for intrusive scenarios?

MR. KOZAK: Yes. We haven't looked at that yet.

PARTICIPANT: Okay.

MR. KOZAK: That's going to be a complicated analysis. And we haven't looked at it yet on how important it will be.

PARTICIPANT: Okay.

MR. KOZAK: But it probably will play a role.

MR. MARSH: Along those lines, what's the thermal state of the canisters at the time before the magma hits them?

MR. KOZAK: It is assumed to be the thermal state of the repository at that time, which is it is elevated in temperature, but it's not --

MR. MARSH: They're not hot?

MR. KOZAK: They're not -- it's the hot repository design. So, their temperature is from the usual time history of the repository temperature that you see.

MR. MARSH: For example?

MR. KOZAK: Well, 176 Fahrenheit sticks in my head.

MR. MARSH: So you might have to worry about magma quenching against the canisters.

MR. KOZAK: Yes.

MR. MARSH: Magma can quench against the

canisters.

MR. KOZAK: Yes.

MR. MARSH: Then that actually makes a bigger thermal --

MR. KOZAK: And we are considering that. The question would be how fractured that basalt would be at the end of it. And we're looking at that.

We haven't really come to any -- but the initial calculations that we did, because we didn't know at that point, we assumed it was fractured.

MR. MARSH: Sure. But, in terms of corrosion, it changes your corrosion because it actually helps the whole corrosion probably because it --

MR. KOZAK: It will keep the water away.

MR. MARSH: Well, you don't have molten magma right next to the thing at months at a time.

MR. KOZAK: That's right.

CHAIRMAN RYAN: Allen?

MEMBER CROFF: A follow-up to John Garrick's initial question. As the waste package degrades, the radionuclide inventory is also going away.

So, to compensate in effect, I don't know how it works out in longer times. You don't have too many acronyms left. And then the question, I'm not even going to try anything in a detailed technical level. Has this report been peer reviewed?

MR. KOZAK: Within the team, yes. We went over it very heavily. Outside of the EPRI team, no. In answer to your first question, those curves that I showed account for all the decay and in growth for the entire inventory.

We worked based on the SR inventory, because that's -- there's a screened SR inventory, which is the best information that we have. It's the best information that's publicly available.

CHAIRMAN RYAN: I guess at this point let's open it up to any and all panel members and the audience, or staff members, anybody.

PUBLIC COMMENTS

MR. McCARTIN: This is Tim McCartin, NRC. Your curves for the persistence in the environment, I guess I'm somewhat puzzled by the no-depletion results in getting a peak at 10,000 years, because radioactive decay very significant for the main contributors to the inhalation dose.

And I'm just not sure. When you have no-depletion, what exactly are you doing? Or the mass loading or the resuspension factor. Is there something else going on there? Or is it just persistence of the deposit?

CHAIRMAN RYAN: I guess -- can have that back. Again, I'd offer it would be helpful to take that out several more decades, the shapes of those curves might, or at least a couple.

MR. KOZAK: Yes, which curve are you looking at Tim?

MR. McCARTIN: Well, the title is persistence in the environment.

MR. KOZAK: Yes.

MR. McCARTIN: And I'm assuming the largest values are for the no-depletion.

MR. KOZAK: Yes.

MR. McCARTIN: And, it would appear that the peak is at 10,000 years, at least the way I read it.

MR. KOZAK: No, that continues to go up.

CHAIRMAN RYAN: That's what I asked about. He said it was going up.

MR. KOZAK: Yes, because you have to sum all the previous years. Each year that you add adds an incremental dose to it.

MR. McCARTIN: Right, but radioactive decay continues to go on. And, generally those disappear for say the first couple thousand years. And what exactly are the assumptions when you say nodepletions?

Are you burying everything else in your analysis except the deposit persist forever?

MR. KOZAK: Oh, I see.

MR. McCARTIN: Is that the only thing you're doing when you say no depletion? I guess that's my question.

MR. KOZAK: Yes, this is an approximately way of doing this. There's no question about that. We haven't set up our analysis to really take into account the release at this year of decays in the environment out to there.

What we found was, if you look at this right here, after a certain point, it's approximately constant for the latter year doses. And so, all we did was take that as a constant.

And we'll multiply that by the number of years prior. It's just to give an indication of what the doses are. And that's probably part of the reason -- I don't remember why I cut it off at 10,000.

That's probably part of the reason why, because you get more and more of the limiting assumption to do that. This is definitely an approximate way of doing it just to give an indication of the orders of magnitude.

MR. McCARTIN: Okay. I think you'd get a different result if you actually try to simulate --

MR. KOZAK: Yes, absolutely.

MR. McCARTIN: -- events in multiple years.

MR. KOZAK: Absolutely. And that's a complicated analysis to do. And we didn't have the time to do it.

MR. McCARTIN: Okay.

CHAIRMAN RYAN: Other questions? Yes?

MR. HINZE: Just a quick question. I was taken with Britt's suggestion that differential thermal expansion would lead to stresses and other stresses that might cause the canisters to lose their integrity.

And yet, I heard from you that these differential thermal expansions were inconsequential. Is that an overstatement? Or do we have -- where are we?

MR. KOZAK: We did an evaluation of those. I wasn't the one to do it. And I know the discussion of those stresses is in the report. And I don't want to speculate on what the answer to that is. I don't remember.

MR. KESSLER: John Kessler, EPRI. My recollection, again, I'll have to remember what it is that Frazier did exactly, but, the arguments that he included were how much creep he felt the alloy 22 could manage at temperature given its yield strength at temperature.

I don't remember exactly whether there's a specific analysis about thermal expansion and how much expansion it is compared to the amount of creep that he thought alloy 22 could manage at temperature.

I think we've got enough to piece together an answer for you that would suggest alloy 22 can manage that amount of creep very easily. But, we're going to have to go back to look to get you a more specific answer.

MR. HINZE: Let me ask another similar question. In the initial contact of the magma with the canisters, do I understand that you did not consider a collision effect whereby you might have impacts on succeeding canisters, essentially --

MR. KOZAK: The train wreck kind of thing.

MR. HINZE: Where you might cause the integrity of the canisters to be -- the acceleration of the deterioration of the canister as a result of the impact with effect. Is that taken into account?

MR. KOZAK: Acceleration of the deterioration, if that becomes -- one package to the next, no we didn't, because this initial impact one was so much more severe than the secondary impacts.

In fact, we did a secondary impact on the roof of the drift for that one waste package. But, that secondary -- that secondary collision was not in that analysis, the analysis of the finite element analysis.

MR. HINZE: Right.

MR. KOZAK: No, we didn't because, essentially, we're lifting it up and hitting it up against the roof.

MR. HINZE: Yes.

MR. KOZAK: But, the increase in the degradation rate may play a role in the longer term release, but not during the eruption.

MR. HINZE: Another quick question, did you take into account the geochemistry of volatiles in terms of their impact upon the canisters?

MR. KOZAK: For corrosion?

MR. HINZE: Corrosive types.

MR. KOZAK: Yes. And, in fact, that was one of the things we wanted to test by doing the experiments, by looking at putting the C-22 in magma.

We weren't sure what some of the other constituents of the magma were going to be.

MR. KOZAK: But, the basalt itself that you melt might be quit different than the magma that you're developing.

PARTICIPANT: Two comments and two questions. Remember, this is a small diameter sheet. I mean, this isn't a large -- your train wreck lava coming down. We're looking at the initial micro second, two tenths of a second of impact of a relatively narrow sheet coming through.

So, we're only looking at one package. And, you know, gravitationally rising, there's not a possibility for a train wreck during this sort of initial impact analysis being done.

On your chemical question, one of the reasons Matt put up sort of the old basalt, we purposefully looked where basalt was loaded with a number of these sort of chemical factors, phosphorous and so on, in which we sort of tested at very adverse types of basalts.

CHAIRMAN RYAN: Just for the reporter.

MR. MARSH: I didn't hear anything about drip -- where's the drip --

MR. KOZAK: We didn't consider it.

MR. REITER: Leon Reiter, NWTRB staff. A couple questions, in your dip and dunk experiments, you didn't have any volatiles in the gas escape. Wouldn't that -- symptoms of corrosion rate?

I assume the patches you get by magma volatiles -- there could be gasses that could affect the corrosion rate.

MR. KOZAK: It could. But, again, corrosion over these time scales was insignificant. If you look at --

MR. REITER: Well, but that's based on what you assumed.

MR. KOZAK: But it was also based on literature, data, and a variety of other evaluations, which the experiments were consistent with.

There are multiple threads of evidence there.

MR. REITER: And they also assume the presence of volatiles?

MR. KOZAK: I would have to go back to the report, I'm not sure.

MR. REITER: Another question. You have five different mechanisms of waste package failure. And you said that when you release those you increase it by five orders of magnitude.

MR. KOZAK: Yes.

MR. REITER: Which of those are the most

important? Which of those could cause the most damage?

MR. KOZAK: Do you mean which --

MR. REITER: There was a creep failure, there was magma contact with erosion by the magma, there was impact -- do you have a ranking of those as to which --

MR. KOZAK: In terms of the amount of release that they gave, they're all the same because we couldn't get it out of any, no matter what we did.

But, in terms of -- I mean, we did the impact analysis by such an extreme calculation, we feel that that's -- any credible behavior in the mountain -- you're going to have a hard time just breaking it open and exposing the waste.

That seems to be completely out of bounds. Of the ones that are left, I don't know. It's hard to say. What we ended up doing the calculation on, we based it on overpressure because the corrosion rates were so low and the erosion rates were so low and everything.

So we said, of the ones that are left, we'll do it on overpressure. But it still wasn't -- we couldn't credibly get releases by that mechanism. John, did you want to --

MR. KESSLER: Leon, we didn't do an analysis that way. And I would say though, the one I would probably care about the most is the general corrosion failure because, if we have creep failure or something else, we have the small breach in part of the container, we still have a container there that will tend to do a lot of blocking.

That's what Matt's next set of analyses were, that that slow, diffusive pathway, or whatever pathway you've got, really reduced the amount of release that can occur.

Of course, if the container completely disappears because it is completely corroded, then you would expect that you would get more release. That would make me think that maybe that's the one I care about the most.

But, in terms of ranking them, in terms of which one you think would go first, we didn't look at that.

MR. KOZAK: Yes, and I wouldn't expect that to happen during the eruption. If there was something that was accelerated by the constituents in the magma or whatever, it would be part of an intrusive analysis of the impacts on something later on.

None of these things -- the duration of the eruption is just too short like that. General corrosion is just -- certainly that would be of concern if it could happen.

But, over a couple of weeks, you can't get it.

MR. REITER: One last question. You mentioned a lot a Nicholis and Rutherford article. It seems to me that you've put a lot of importance in what that's telling you about the background.

Is there any way that you can give us a quantitative idea of what kind of impact, assuming those kinds of temperatures, would have?

MR. KOZAK: Do you want to call it a quantitative?

MR. REITER: You know, what kind of order of magnitude, you know, if assume releases, let's say with one temperature and with the -- of the temperature, say with the Nicholis and Rutherford temperature, or the velocities of them.

MR. KOZAK: Quantitatively, I don't think I can do that without going through some more evaluation. Qualitatively, I can certainly say the alloy 22 is much stronger at lower temperatures.

The magma is at much higher viscosities, so it's moving much slower. So, the dynamic effect are a lot less. But, there's a lot of things. It's very important.

There's a lot of implications, particularly at the lower temperatures and the higher viscosities, I think, are absolutely crucial. I can't give you a quantitative response to that.

MR. APTED: Leon, I think you have to look at what we did in the report prior to that type -- so we looked at tremendously adverse higher temperatures and higher impact and so on.

Everything about that event would make our calculations, which we find robustly defensive, in terms of protecting the package during this impact, weeks to months, to maybe a year or two years type of event, absolutely even more conservative.

So, I mean, I think -- is more. As we're doing our evaluation in an intrusive case, in terms of looking at the sub-variance of how much basalt goes down these drifts, the release of gas, and so on.

MR. REITER: Right.

MR. APTED: I'd like to sort of pose a question maybe, that it strikes me always that sort of like you juggle three balls and then somebody says, can you juggle four balls?

And then you juggle four balls. And then you juggle five balls. So, I think EPRI is juggling about five balls here. NRC, DOE is maybe juggling one.

(Laughter.)

And so, I'd like to ask maybe somebody like Dave Johnson what -- you know, we talked earlier about sort of the risk side adversity probability.

And we find it there and in consequence. It's sort of after hearing these folks.

MR. JOHNSON: Well of course, the two talks -- thank you, but the two talks were quite different. I had reaction to the NRC one kind of wondering how they were using risk information to direct their ongoing research and kind of concluded maybe there's not enough information there to really have a direction.

So they're chasing the science there on its own. It may be unfair. I'm certainly surprised and a little bit overwhelmed by the EPRI presentation.

It does go from the initial conditions, if you will, to the final end stage, which is what I and others have been looking for. I guess my initial reaction is, you know, how robust are the models?

Can we visualize reasonable scenarios not included? And, again, I'll fallback on not being a geologist. But, a scenario that the initial dike doesn't intersect the drift, but only does so after it has grown wide enough -- can I transport a package up to the surface and not have this counteractive pressure?

It may be a silly question, but, I think maybe at you're at the time where a detailed peer review would really benefit that. And maybe that, you know, could involve NRC and come to some sort of consensus here.

MR. KOZAK: We actually did think about whether or not the waste package could be entrained. First off, the buoyancy courses aren't high enough to actually bring them up.

But, if they do, if the waste package were to get all the way out, it's still zero dose. It's got to get 18 kilometers further down before you get a dose.

MR. JOHNSON: But it changes the scenario.

MR. KOZAK: The whole thing comes out, it is deposited on the surface, which isn't going to happen. But, if it did, it's still no-dose. There is a multiple barrier existing.

So, some of those things we have tried. You're right, it is conceivable, I suppose, that someone could. That's what we all do in this business, we look for the more conservative case and say, well, have you thought about this and, you know, try to push and prod at it. And that's got to happen.

MR. JOHNSON: I'm not necessarily looking for the more conservative case. I'm looking for, are there reasonable, at least as credible scenarios that aren't in your model now?

We certainly can't answer that today. But, it's an impressive amount of work. I can't speak to the science of it, though, I'll say that also.

CHAIRMAN RYAN: To me there's a science in the probabilistic analysis part of it. I think that, at the end of the day, is what substantiates and brings together all the pieces of it.

And, to be fair, I must say I think the NRC is juggling at least more than one, if not as many as everybody else. They're just presenting parts and pieces of it today.

And I think it would be very fortuitous to think about the constraints in which they gave the presentations today. And, to be fair to them, I think there is a broader spectrum.

They are addressing across the same spectrum. It's just that we heard the whole piece today. So, I offer you that to think about. As we conclude, I think, with any last couple of questions that we might have. Yes, I'm sorry.

MR. MELSON: I just have a quick one about your immersion experiment where you put the wire in the magma. You said you put that in a -- gas atmosphere. Is that correct?

MR. KOZAK: Yes.

MR. MELSON: Well, you know that's reducing?

MR. KOZAK: Yes.

MR. MELSON: And so, that would give you more stability, unless corrosion and -- where you have a oxide buffer.

MR. KOZAK: Yes. But, the conditions in the mountain are expected to be reducing, which is why we did that. The conditions of -- I'll let the chemist answer.

MR. APTED: I think that the conditions -- the conditions in the basalt rising. The basalts are, you know, nickel oxide, something like that. That is very reducing.

The activity margin is extremely low, probably -- than anything, doesn't achieve in reducing the conditions enough compared to those that would occur.

CHAIRMAN RYAN: Las question, please.

MR. MARSH: There is the possibility that you could actually do real experiment. I mean, we have canisters. And you do it just like this. And, the world is producing slag everyday -- lots of it.

And, you can dig a pit, you can pour it on top of canister, and you can see what happens. It might be very interesting. And I've actually been involved in trying to actually make a small vat of magma.

So I've actually been into this in some detail in talking to companies. Many of them think I'm crazy. Other ones are more open to this. So, if you want to, I have a foothold and can help you get into the system.

MR. KOZAK: Well, I wish you hadn't mentioned the slag, because we were hoping that it would get transferred to Hilo.

CHAIRMAN RYAN: Well, that's a great lead-in, Bruce, actually, to our perspective tomorrow on aerosol modeling issues. You know, I have a slag experiment.

But it's a very small version of it. It's things like level gauges in steel mills that happen to get -- melt. It would be interesting to think about how much of that radioactive material stays in the ball of steel that hits the floor and how much ends up in the bag house.

That might be an interesting series of cases to take a look at. So, we'll hear a little bit about those kinds of issues from Dr. Fred Harper, actual experiments in the aerosol generation area.

Resuspension modeling issues from Dr. Lynn Anspaugh, a person who has written on this over the years. And, of course, Dr. Keith Eckerman is going to talk about dose modeling and perhaps give us some insights on conservatisms and none conservatisms, particularly for our radionuclides of interest -- plutonium and americium.

So, we're kind of getting to the third leg of this school on the igneous activity discussion, which will be one of the aerosol generation characteristics, resuspension and dose modeling characteristics.

And we'll close out our approach to re-examining this question. Let's see, our schedule is that we'll convene at eight o'clock. We'll be in promptly at eight o'clock and press on according to the schedule.

(Whereupon, at 5: 44 p.m. the above-entitled conference was concluded.)

CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: Advisory Committee on Nuclear Waste

Docket Number: n/a

Location: Las Vegas, Nevada

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and, thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Crissy Willis
Official Reporter
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